A HISTORY OF PHYSIOLOGICAL OPTICS

from 1650 to 1800

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P. M. O.

ABSTRACT

The main body of the thesis is divided into the following sections:

a. The seat of vision and visual acuity.
b. Accommodation - how the eye focuses on objects at different distances.
c. Colour vision - including the discovery of colour blindness.

There are other sections peripheral to the areas listed above.

Before the main body of the thesis:

1. An introduction outlining the scope of the thesis.
2. A brief, modern explanation of the functioning of the eye.
3. An evaluation of the role played by experiment and hypothesis in the discoveries made in physiological optics between 1650 and 1800.
4. A brief account of the development of theories of vision and the working of the eye from ancient times to 1650.

After the main body of the thesis, there is an appendix giving a modern explanation of the working of the eye.

Finally, there is an appendix which endeavours to evaluate the way in which the discoveries and theories mentioned in the thesis were disseminated to the contemporary layman. This has been compiled from encyclopaedias published during the period covered by the thesis.

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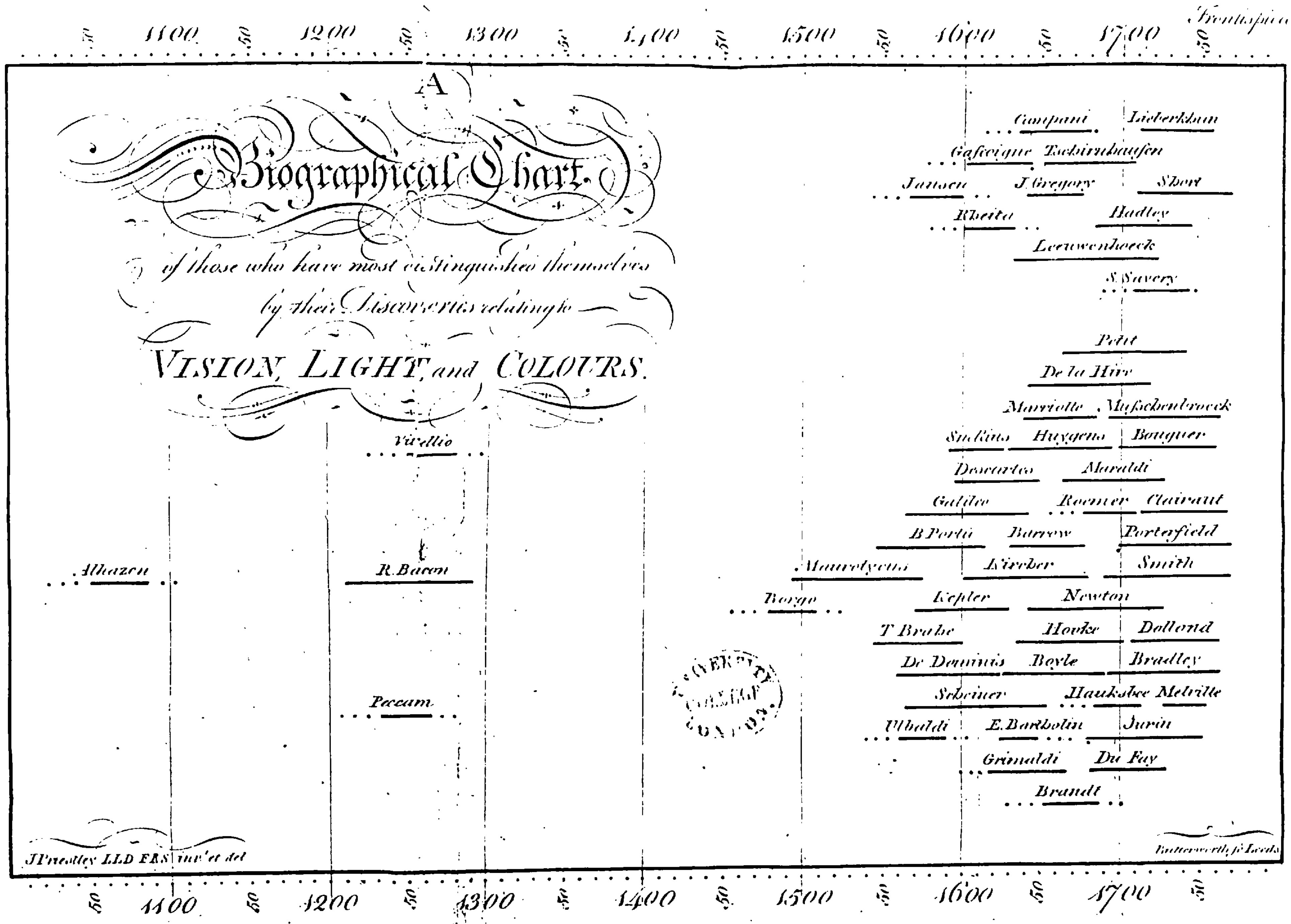

Frontispiece
1100 1200 1300 1400 1500 1600 1700
A
Biographical Chart
of those who have most distinguished themselves
by their Discoveries relating to
VISION, LIGHT, and COLOURS.
Campani
Lieberkhun
Gascoigne
Tschirnhausen
Jansen
J. Gregory
Short
Rheita
Hadley
Leeuwenhoeck
S. Savery
Petit
De la Hire
Mariotte
Muschenbroeck
Snellius
Huygens
Bouguer
Descartes
Maraldi
Galileo
Roemer
Clairaut
B. Porta
Barrow
Portarfield
Maurolycus
Kircher
Smith
Borgo
Kepler
Newton
T. Brahe
Hooke
Dollond
De Dominis
Boyle
Bradley
Scheiner
Hauksbee
Melville
Ubaldi
E. Bartholin
Jurin
Grimaldi
Du Fay
Brandt
Vitellio
Alhazen
R. Bacon
Peccam
J. Priestley LLD FRS inv.t et del.
Butterworth sc. Leeds
1100 1200 1300 1400 1500 1600 1700

INTRODUCTION

Of all the senses, vision has consistently attracted the most attention throughout history. Not surprisingly, numerous theories spring from the culture of Ancient Greece. Conjecture over the nature of how we saw produced the conflicting intromission and emission theories - that vision was caused by particles entering the eye, or by some emanation from the eye - as well as other hybrid theories. The appreciation of the inevitable imperfection of our senses gave rise to Plato's theory of Forms, which held that we arrived at approximate knowledge of the material world only through our senses. The true nature of any object he called the Form of the object, and our senses could give us only an approximate concept of what this was. The working of the eye itself attracted considerable interest, with the widely held view, carrying the authority of Galen, that the 'glacial humour', or crystalline lens, was responsible for vision within the eye.

The Muslims inherited Greek thought and continued to press ideas forward, although owing to a religious prohibition on dissection, they lacked the stimulus of experiment. During this time Alhazen did more than any other Muslim to emphasise the importance of vision; and it was he who introduced the concept of a point-by-point formation of the image in the eye, based upon the intromission theory. Thus at the end of the first millenium Alhazen had taken an important step forward in the search for an understanding of the true nature of vision.

Nevertheless, progress was slow, and even as late as the beginning of the seventeenth century, there remained one major stumbling block to the understanding of the internal operation of the eye. This was the lack of awareness that the image formed in the eye was inverted. To turn the collective image of the whole visible world upside-down was a gigantic intellectual feat, and yet until this was accomplished, the true seat of vision, the retina, could not be appreciated.

At the beginning of the seventeenth century, Johannes Kepler carried out the geometry necessary to demonstrate the inverted image on the retina, and shortly afterwards Christoph Scheiner verified its existence experimentally. Nevertheless, the position of the retina as the light-sensitive surface in the eye was still not firmly established. Descartes added his authority in favour of the retina, but almost immediately in the next generation Mariotte produced his controversial theory that the seat of vision was the choroid rather than the retina

Having faced the difficulty that the image on the retina was undoubtedly inverted, the task of explaining how we saw objects erect was dealt with skilfully and, by some, in an almost offhand manner. Basically the argument was put that our other senses interpreted the world correctly, and we learnt from experience to invert what was projected on the retina, and to view the world instinctively as upright.

The nature of the retina, which was often likened to a piece of plush, with the erect fibres corresponding to nerve endings standing out of its surface and facing the front of the eye, was well enough understood from the middle of the seventeenth century. It was now also understood that the point-by-point build-up of the image, which Alhazen had thought took place on the front surface of the crystalline lens, now took place on the retina. Messages from the nerve endings were then thought to be transmitted through the optic nerve to the brain.

Such was the almost instinctive prejudice in favour of the retina, that Mariotte's hypothesis in favour of the choroid as the seat of vision, did not receive widespread support. In many ways his hypothesis was extremely well founded, and his reasoning against the retina's being the seat of vision was logically based upon the widely held concept of the retina outlined above. The retina, however, is reversed, with nerve endings on its rear surface and not on its front; from its nature, which has been understood only during this century, we can now effectively answer Mariotte's difficulties in accepting it as the seat of vision.

While Mariotte did not receive support for his theory, his work was widely discussed throughout the greater part of the eighteenth century. During this time opinion moved slowly towards an acceptance of the retina as the seat of vision. In spite of this controversy and the undoubted importance of this subject in the field of physiological optics, the nature of the seat of vision was not the topic which produced most interest during the eighteenth century. Accommodation, the ability of the eye to focus on objects at different distances, produced far more speculation and experiment; this was probably because the number of alternative means by which accommodation could be achieved was far greater than the range of options for the seat of vision.

The possibility that the eye changes its focus for close and distant objects was first recognised by Kepler at the beginning of the seventeenth century; and the full range of alternative means of causing accommodation was frequently explored and re-explored during the next two hundred years. A minority, led by De la Hire, held that apart from the closing of the iris when close objects were viewed, no accommodating mechanism was required. Others held that the eyeball changed shape, or that the crystalline lens changed shape or position within the eye. A major difficulty in appreciating the correct cause of accommodation, a change in shape of the lens, was the back-to-front action of the ciliary muscles within the eye, which cause accommodation. When these muscles are relaxed they stretch the lens, whereas most early observers associated a stretching of the lens with a corresponding tightening of the muscles. However, there is no doubt that the major experimental contribution in this field was made by Thomas Young in the eight years at the end of the eighteenth century. It is worth emphasising his painstaking experimental work on accommodation, since it is easy for it to be overshadowed by his brilliant hypothetical theory in the field of colour vision.

The investigation of colour vision was stimulated almost entirely by one man, John Dalton. His masterly account at the end of the eighteenth century of his own colour-blindness highlighted at one and the same time a virtually unknown visual defect, colour-blindness, and also the then current lack of knowledge of colour vision. The time was obviously right for an investigation of this subject, since it had been briefly touched upon earlier, notably by Newton, but largely neglected. It is surely significant that eight years after the publication of Dalton's paper in 1794, Young put forward his three-colour theory, which was later to find greater authority through endorsement by Helmholtz in the middle of the nineteenth century. The confirmation of the Young-Helmholtz theory had to wait a further hundred years, for the development of the sophisticated electronic techniques required to make measurements involving single rods and cones.

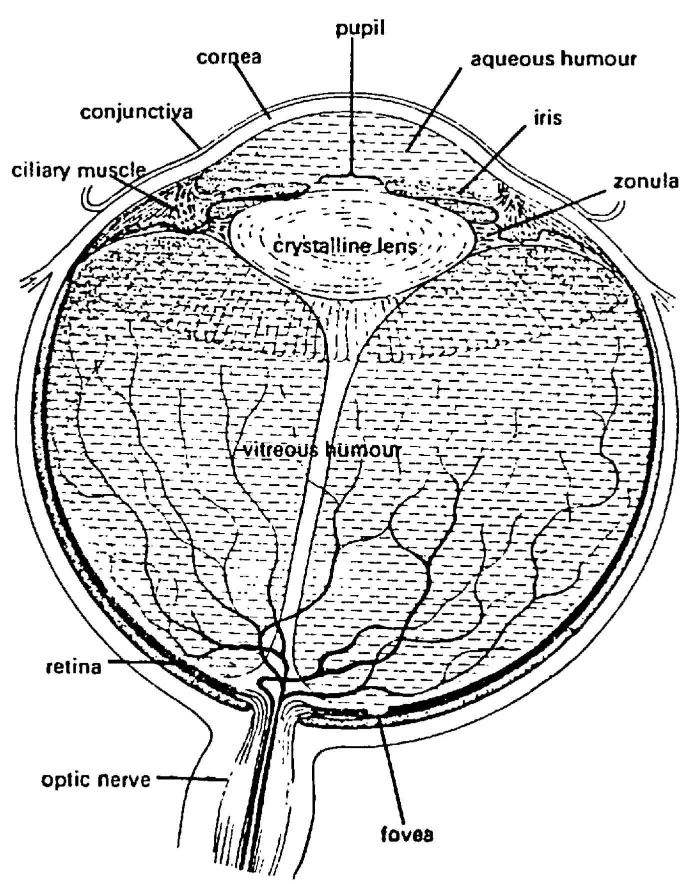
During the seventeenth and eighteenth centuries physiological optics was carried forward mainly on the basis of speculation with only a limited amount of experiment, since it was a subject which, apart from dissection, did not lend itself readily to empirical investigation. The scientists who involved themselves in this field were, like all scientists during this period, non-specialists.

In Kepler, Descartes, Newton and Mariotte we have men whose interests were as wide as knowledge itself. This generalised approach extended almost until the end of the eighteenth century, and Joseph Priestley's contribution is an example. A dissenting clergyman, he wrote widely on education, politics, history, English grammar and metaphysics. He was probably not in the first rank in any field, but his very versatility makes him a good example of the mind and spirit of the eighteenth century before the expansion of scientific knowledge made specialisation inevitable. Indeed, the arrival of the specialist in this field can be said to have occurred only with the emergence of Thomas Young right at the end of the eighteenth century. For although he was trained as a medical practitioner, his main contribution to science was in the single field of optics.

From the short list of names mentioned above, it can be seen that physiological optics was well enough served in the calibre of the scientists who contributed to its advancement. As a discipline its progress was in advance of many other areas of science, such as the study of heat, electricity, mechanics and chemistry. In fact, the sophistication of the discoveries made in the study of the eye were probably matched only in the fields of astronomy and physiology. Certainly the development of our knowledge of the eye was far more rapid than in the only field which could be considered to be in any way comparable, that of hearing.

A BRIEF MODERN EXPLANATION OF THE WORKING OF THE EYE

A modern diagram of the eye is shown below. The cornea, crystalline lens and vitreous humour act together as a compound convex lens to project an inverted image on the retina. The amount of light entering the eye is controlled by the iris diaphragm which is muscular and increases the diameter of the pupil in dull light, and decreases its diameter in bright light. The iris has one other function: for close objects it decreases the size of the pupil independently of the amount of light, and in this way improves the clarity of the image on the retina in the same way as stopping-down the lens on a camera improves the quality of the image on the film. The eye can alter its focus to enable it to see close or distant objects clearly. It does this by changing the convexity of the crystalline lens, making it more convex for close objects and less convex for distant ones. The change in convexity is achieved by the ciliary muscles acting in a rather unusual way. The crystalline lens



is normally held under tension by a membrane, the zonula; in this position it is focused for distant objects. In order to focus on a close object the ciliary muscles tense and in so doing release the tension in the zonula, allowing the natural elasticity of the lens to make it more convex. The ciliary muscles were not identified until the middle of the nineteenth century, and this had caused considerable difficulty in identifying the proper cause of accommodation. The theory which came closest to the truth proposed that the fibres of the "suspensory ligaments" - the zonula - were muscular and stretched the lens to view distant objects. Thus, an effort would have been required to view distant objects. Unfortunately this ran counter to experience, which held that the eye was at rest when viewing distant objects. It is the back-to-front nature of the working of the ciliary muscles, whereby the tenseness in the muscles relaxes the tension on the lens when viewing close objects, which explains how the eye is not in a state of rest when viewing close objects.

The main defects of vision are short-sight (myopia), long-sight (hypermetropia) and astigmatism. The deterioration of sight which takes place with age is called presbyopia, and is the progressive loss of accommodating power. In both short- and long-sight the eye possesses the normal amount of accommodating power, but the range over which this acts is different from the normally sighted. A short-sighted person can focus only over a range of distances close to the eye, while a long-sighted person can focus only on distant objects. Short sight is corrected by a simple concave lens, long-sight by a simple convex lens. Presbyopia occurs from middle-age on, and the increasing loss of accommodating power which occurs is due to the crystalline lens becoming increasingly harder; its symptoms are in many ways similar to those of long-sight. The correction of presbyopia often involves the use of bi-focal lenses, which enable close objects to be seen when the bottom half of the lens is used, and distant objects to be seen clearly through the top half. Astigmatism is the inability of the eye to focus in the vertical and horizontal planes. It is due to one of the curved refracting surfaces of the eye having different curvatures in the vertical and horizontal planes. Since the majority of the refraction takes place at the front surface of the cornea, unequal curvature here will be a likely cause of astigmatism. It is corrected by the use of lenses which have a complementary difference of curvature in the horizontal and vertical planes.

The retina contains light-sensitive receptors which react to the inverted image projected on its surface. The receptors are situated on the outer surface of the retina, and therefore light has to travel through the retina before reaching the receptors. For this reason there is a small area in the centre of the retina called the "fovea centralis", where its thickness is less, and where there are no blood vessels; this means that there are fewer hindrances to the formation of a clear image. The fovea is the part of the retina which is used for accurate vision. There are two main types of receptors, rods and cones. Rods create only monotone vision, but are considerably more sensitive to dull light than cones. There are three types of cone, sensitive to red, green and blue light respectively. The fovea contains only cones, and as the distance from the fovea increases, rods start to occur with increasing density. The sensitivity of the eye increases slowly on entering a darkened room, and after about thirty minutes the eye is several thousand times more sensitive than in bright light. This is not due to the action of the iris, which acts almost instantaneously, but is due to the photo-chemical action which takes place in the rods and cones. These contain pigments which are bleached in bright light. The bleaching absorbs light energy and this energy is eventually changed into electrical energy which is transmitted to the brain. In the dark, these pigments are manufactured, reaching their maximum concentration in about thirty minutes.

An explanation of working of the eye in greater detail is given in Appendix A at the end of the thesis.

THE ROLE OF SPECULATION AND EXPERIMENT

The history of physiological optics during the seventeenth and eighteenth centuriesshows that speculation played a far more important role than experiment in the formulation of hypotheses. The reasons forthis are easy to understand, for experiments designed to investigate the functioning of the eye are difficult both to devise and to execute. Dissections of the eye have always been comparatively easy, but these tend to lead to a knowledge of the structure of the eye rather than of its function; and, as we shall see later, they cannot be used reliably in formulating theories explaining the working of the eye.

There are three types of experiment available. The first uses dead eyes, either human or animal. The second uses other people as experimental subjects, and changes in their eyes are observed, such as the opening and closing of the iris, or their comments on what they see are noted. Not surprisingly, the third and most common type of experiment is that where the observer investigates his own eyes. This last type of experiment, however, posed problems of a special kind: namely, the observation of the function of the eye by the eye itself. It is fair to say that early observers largely ignored such philosophical niceties, and concentrated on a pragmatic approach, making observations and drawing conclusions when and where they could. From this extremely subjective base, many conclusions on the working of the eyes were drawn during the seventeenth and eighteenth centuries.

As has been pointed out above, dissection of dead eyes is obviously valuable in determining the structure of the eye and its measurements. One can cite here the painstaking experiments of Petit in the early eighteenth century, which provided other investigators with valuable information about the structure and dimensions of the eye. Such are the changes which start to take place, even shortly after death, however, - for example, the increasing opacity of some media - that it is difficult to place reliance on the results of these experiments when they are used in support of theories of vision. It could, however, be anticipated that dissections of the eye would normally assist in determining the nature of different parts of the eye. This, in turn, could be expected to assist in the understanding of how the different parts of the eye work. In general, however, this has not been the case, and one can say that, with one notable exception, dissections of dead eyes have not played a major part in helping us to understand the working of the eye. One can take as an example of this, the case of the ciliary muscle. It could be anticipated that careful dissections of the eye would have established its muscularity, and undoubtedly this discovery would have assisted the correct cause of accommodation to be found. In the event, the discovery of its muscularity was not made during the eighteenth century, and probably as a consequence of this, the correct cause of accommodation had also not been found by then. The ciliary muscle acts by relaxing the tension on the crystalline lens, when the muscle itself is in tension. The lens then becomes more convex, and in this state is accommodated for close vision. Thus the muscle is in tension for close vision. This explanation confirms everyday experience that it requires an effort to view close objects, and that the eye is therefore at rest for distant objects. All the early efforts to ascribe muscularity to any part of the ciliary body - the ciliary muscles and the suspensory ligaments - involved a pulling force at the periphery of the lens by the muscle. If this tension changed the shape of the lens, then its result was to make it less convex; and thus, tension in the muscle was associated with a flatter lens, and with distant vision. Thus the eye was imagined to be at rest for close vision, which was contrary to experience. It was probably the search for a solution to this difficulty which led to the formulation of those theories of accommodation based upon movement of the crystalline lens. In these hypotheses, the muscularity of the ciliary body was used to pull the lens forward for close vision. Thus the eye required an effort to be made for close vision, and was correctly deemed to be at rest for distant vision.

The opinion that experiments on dead eyes contributed little to our understanding of the functioning of the eye has one notable exception: the experiment carried out early in the seventeenth century, in which Scheiner dissected the outer layers of the back of the eye, exposing the translucent retina. On this he was able to see the inverted image produced by the optical system of the eye. This helped to establish at an early date a strongly held opinion among many scientists that it was the retina which acted as the seat of vision. This experiment also helped to establish a much clearer understanding of the optical working of the eye. In so doing, however, it also produced an additional problem, for it showed without any doubt that the image on the retina was inverted. This in turn led scientists to expend much philosophical ingenuity in explaining how, when the image was inverted on the retina, we saw an erect image. One can fairly say that many of them quickly arrived at the correct solution to the problem: that our experience of the external world gained through our other senses, soon teaches us to invert mentally the image which is projected on the back of the eye. Nevertheless, Scheiner's experiment can be said to have established at a remarkably early date the correct function of the retina. It would be interesting to speculate whether its function would have been as clearly understood, if its really detailed structure had been known at this time. The best picture which early experimenters devised was of the retina as a piece of velvet, with the pile facing inwards towards the vitreous humour. The pile was considered analogous to the nerve endings, which were sensitive to light, and which transmitted messages to the brain. This is a remarkably accurate picture, when one takes into account the fact that it was widely held even before the middle of the eighteenth century. It is inaccurate, however, in one important detail; the pile of the velvet - the nerve endings - is on the opposite side of the retina from the vitreous humour. This means that the light has to pass through the retina before reaching the nerve endings. It is difficult to avoid the conclusion that, had the early experimenters known this, they might have been less likely to understand the correct working of the retina.

It is easily possible, that, without the authority given to the function of the retina by Scheiner's experiment, Marriotte's theory might have had much greater acceptance. Marriotte discovered the blind spot in 1668, and the conclusions he drew from this discovery led him to put forward a theory in which the choroid, and not the retina, acted as the seat of vision. It is difficult to evaluate Marriotte's discovery. One could say that it was the result of inspired observation. On the other hand, one could equally well say that, since this small area of blindness is present in us all, its discovery was inevitable. Nevertheless, Marriotte was the first to marry his conclusions to other facts about the back of the eye and produce an hypothesis on the seat of vision. His argument was as follows. The blind spot was insensitive to light, and yet the retina was known to cover the area of the blind spot. If the retina was the light-sensitive surface, since it covered the blind spot, then the blind spot would also be sensitive to light. This he had shown was not the case, and therefore some other surface at the back of the eye must be sensitive to light. Since the choroid was the layer next to the retina, and since it did not extend over the blind spot, then the choroid must be the seat of vision. Nevertheless, in spite of this logical interpretation of his observations, Marriotte's view did not prevail and the retina continued to be held as the seat of vision. This opinion became even more strongly established during the eighteenth century, although it is still possible to find isolated articles expressing contrary opinion, even at the beginning of the nineteenth century.

Probably the most frequently repeated experiment of all, and one which was steadily refined during a period in excess of one hundred years, was the one extensively used by De la Hire at the end of the seventeenth century. This involved viewing objects through two small holes in a sheet of card and, although De la Hire did not devise the experiment, he was the first to use it in support of a theory which attempted to explain how the eye accommodated. Since his view was that the sole accommodating power of the eye was due to the closing of the iris, which took place when a close object was viewed, it would be easy to conclude that this experiment hindered the eventual discovery of the correct cause of accommodation. Such was the interest stimulated by De la Hire's work, however, that the experiment was repeated and refined over the years and eventually may well have assisted in the discovery of the correct cause. It was certainly a modification of De la Hire's experiment nearly one hundred years later that proved that the eye accommodated independently of the action of the iris. It can be considered extremely ironic that Priestley, using a development of De la Hire's original experiment, deduced that his results proved the existence of an accommodating power possessed by the eye in addition to that provided by the iris. Unfortunately, this deduction did not help Priestley, or any other experimenter before the end of the eighteenth century, to discover the true cause. In the closing years of the century there was a flurry of experimental activity at a far higher level of ingenuity and skill than anything that had been attempted before. Home and Ramsden working together, but above all Thomas Young, were responsible. Nevertheless, in spite of introducing new types of experiment, using advanced reflection techniques from the surface of the eye and other equally sophisticated devices, they still showed that they were, on occasion, just as capable of unjustified speculation as any of the scientists in this field who had preceded them. For example, Home and Ramsden based their opinion that the eye could still accommodate, even after the removal of the crystalline lens, upon some extremely doubtful results obtained from one subject. Their conclusion ran counter to a longstanding belief that removal of the crystalline removed the power of accommodation. Even Young, whose later work stands alone in this field, held an early view that the crystalline was muscular. This view was founded upon extremely poor experimental evidence and in fact could be almost described as pure speculation.

Nevertheless, it would be unjust to emphasis this aspect of the work of Home, Ramsden and Young, while neglecting the far more positive aspects of their other experimental work. I have mentioned it only to draw attention to the ease with which it is possible for even the best scientist in the field of physiological optics to fall into the trap of ill-substantiated speculation. Young in particular devised numerous ingenious experiments, which he carried out on himself, and which gave considerable authority to the conclusions he drew on the functioning of the eye. He even showed that rare quality in a scientist - or in any human being - of being ready to change his mind in the light of new evidence on a subject.

As a concluding thought on the role of experiment, one could fairly make the observation that a greater variety of experiments took place during the last few years of the eighteenth century than had taken place up to that time. The majority of these experiments were devised by Young and carried out by him, working alone and using his own eyes.

Summarising the role of experiment during the seventeenth and eighteenth centuries, one could say that, until the time of Young, it consisted of continued repetition and variation on a very few basic experiments. Young expanded the range of experiments considerably, but the basic pattern of the formation of hypotheses continued: the drawing of as many conclusions as possible from the inadequate experimental data available, and then the formulation of a theory, which usually represented the particular bias of the experimenter. In this way speculation was based, albeit tenuously, upon experiment. I am not certain whether one can say this about Young's most important theory: the three-colour theory of vision. It could be described as almost pure speculation, and to describe it as such would add a certain emphasis to this present discussion, especially when one considers the ingenuity shown by this experimenter in other fields of physiological optics. Indeed it is difficult to see how such a theory could be experimentally based, given the experimental facilities available at the end of the eighteenth century. It was an inspired guess, however, and serves to underline the importance of speculation in this particular subject.

THEORIES OF VISION BEFORE 1650

The thought and attention which the ancients lavished upon this most fundamental and intriguing of our senses ensured that virtually every possible theory had been put forward to explain the basic facts of vision by the early centuries of the Christian era. Although we have no direct writing from Pythagoras (c. 580-500 B.C.), subsequent works from his disciples make it reasonably certain that he favoured an intromission theory; thus he believed that vision was caused by the passage of particles from the object to the eye. He was later supported in this belief by the Epicureans. An alternative - the emission theory - considered that rays were emitted from the eye, and that we saw an object when it was touched by these rays. The luminescence often seen in the eyes of animals was one of the reasons put forward in favour of this theory by some observers. Empedocles (c. 440 B.C.) can be considered to have originated this theory; he thought that something corporeal issued from the eye and when it struck an object, it was reflected back into the eye, and the object was then seen. Euclid and Ptolemy were among the most distinguished of those who supported this theory. It was perhaps inevitable that a combined emission-intromission theory should also be put forward, and this was postulated by Plato, and later supported by Galen. Aristotle (385-322 B.C.) was unique in thinking that light was not corporeal, but was involved in some sort of qualitative changes in the medium through which it passed.

It is, however, necessary to put this brief summary of Greek thought into perspective, and it must be realised that Greek ideas about the nature of knowledge were very different from our own. No other civilisation has produced philosophers who have expended as much pure intellectual effort in the search for the real meaning of knowledge. Therefore it would not be correct to evaluate the bare bones of their theories on physiological optics, which are given above, without endeavouring to understand the importance they assigned to information which we acquire through our senses in general, and our eyes in particular. It is impossible in such a short summary to do justice to all the various shades of Greek philosophy, but an outline of the ideas of the two giants of the fourth century B.C., Plato and Aristotle, might serve to give some idea of the way in which the Greek mind thought, or perhaps the priorities which they gave to certain aspects of knowledge.

Plato believed that our senses could not give us perfect knowledge about objects. He called the idea of an object which our intellect created, the Form of the object; and he thought that our senses gave us information about objects which were approximations to the Form. He believed that the basis of objective reality lay in perfect or ideal Forms which were never discovered or realised completely in the world of sense perception. This has led some to a belief that Plato dismissed sense perception as being of no value. On the contrary, he regarded information and understanding gained through our senses as of vital importance as a means to the knowledge of the true realities of the Forms. In his allegory of the Cave in The Republic, he says that men who can see only the shadows of objects will suppose these to be the only reality. He is saying therefore, that not only can our senses give very distorted pictures of the real objects, but also, that without our senses we should have no idea at all of the object. This emphasis on the intellectual moulding of information gathered by the senses, by experiment as it were, into some other form of higher knowledge, has led some to believe that Plato's influence was unfavourable to the development of science. This is surely not true, since following Plato there was an era during which science developed considerably, helped, among others, by former pupils of the Academy. It would be easy to suppose that in pursuing his biological researches Aristotle was departing from the teaching of Plato; there is no evidence, however, that this was the case. It is true that Plato was not much interested in research in natural science. It is also true that Aristotle thought that Plato had placed far too much emphasis on the role of mathematics in the understanding of the material world. Aristotle rejected Plato's view that knowledge required the existence of forms which transcended the particular objects comprehended by our senses. He thought that form and matter were not separable; and it is this idea that the form of an object is inherent within it which reflects the difference between Plato's preoccupation with the pure abstractions of mathematics and Aristotle's greater interest in empirical science.

Nevertheless, in spite of his excellent empirical work in the field of biology, it would be incorrect to cast Aristotle as an experimental scientist with modern thought processes. When one compares him with Plato, the pure mathematician, Aristotle may well appear to be the natural scientist, but Aristotle's view of natural science was far less empirical and more rational than might be supposed. It may be significant that he appeared most anxious to emphasise the points of disagreement withPlato during the early years after founding the Lyceum as a rival institution to the Academy.

It may well appear that we have strayed far from Greek theories of vision in this section, but it is only proper to judge their theories in the light of the priority they gave to intellectual rather than empirical observation.

In the field of vision it was the very diversity of Greek thought which posed the greatest problems to the Muslims when they inherited their ideas unedited, as it were. In some ways, too, it was more than an inheritance, rather a dependence, since for religious reasons Muslims were not allowed to carry out dissections of their own, and had to rely upon the results obtained earlier by the Greeks. From this work two theories came to dominate Arabic thinking. Hunain ibn Ishaq (d. 877), who was the most prolific translator of scientific works into Arabic, argued for a combined emission-intromission theory in the tradition of Plato and Galen. Hunain had the support in general terms of al-Kindi (d. 873), although there was some disagreement as to the nature of what was emitted both by the eye and the visible object. Hunain thought in terms of rays, while al-Kindi couched his theory in terms of a general emanation of power, and gave greater weight to the geometrical approach to optics favoured by Euclid and Ptolemy. The alternative, which was a complete intromission theory, was supported by Avicenna (ibn Sina, d. 1037). None of these Arabic writers, however, attempted to do more than deal with the problems of vision in a piecemeal manner; they dealt with various aspects of sight as they arose, and in the space of a few paragraphs or pages.

This situation was soon changed, however, by the arrival of Alhazen (Ibn al-Haitham, d. 1039). He put forward the first comprehensive alternative to Greek optical theories. His main optical work was Kitab al-manazir, known in the West as De aspectibus, or Perspectiva; this was translated into Latin at about the end of the thirteenth century and dominated Western optical thought until early in the seventeenth century. Alhazen started from first principles by noting the effect of bright lights on the eye. He noted that the eye could be injured by very bright lights, and used this fact to support the contention that this could not happen from the emission of the eye's own ray; therefore the eye must be in receipt of something from the bright body. He argued that the phenomenon of after-images supported this position. His conclusion was:- "All these things indicate that light produces some effect in the eye." Rays from self-luminous bodies had been recognised from ancient times as they passed through mist or dust, but Alhazen maintained that every visible body emitted rays, and was seen by the emission of its own light; he held that the non-luminous bodies had light deposited on them from self-luminous bodies. Colour he considered to be a similar process to that of light:- "The form of colour of any coloured body, illuminated by any light whatsoever, always accompanies the light emanating from that body."

Alhazen also occupied himself at considerable length to demonstrate logically that rays emitted from the eye were not necessary for vision, or indeed, responsible for it. He therefore maintained that their existence was conjectural - "and nothing ought to be believed except through reason or by sight." The form of the only printed edition of Perspectiva (1572) gave rise to some uncertainty on his final position on emitted rays. The editor, Freiderich Risner, divided the work into sections and gave each a sub-title, and from one of these, which implies the acceptance of both emitted and received rays, has grown a belief that Alhazen may have backed down from his denial of emitted rays. In fact it would appear that Alhazen agreed to the existence of emitted rays, merely as a mathematical device to help mathematicians who were concerned with a mathematical account of the phenomenon rather than with the true nature of things.

When Alhazen came to consider the actual structure of the eye, he was, of course, considerably hampered by the Islamic prohibition of dissection. He obviously used descriptions given by Galen, and Rufus of Ephesus, and it is not surprising that he followed Galen closely in arguing that the glacial humour (crystalline lens) was the light - sensitive organ. He specified further that it was the front surface of the lens which was the sensitive part. When he came to consider the explanation of the actual formation of the image on the front surface of the lens, Alhazen was in some difficulty; each part of the object radiated light and colour in all directions, and consequently every part of the glacial humour should receive light and colour from every part of the object, producing a totally confused image on the lens. He overcame this difficulty by considering only the rays which entered the cornea at right angles and which did not suffer refraction. Alhazen pointed out that only one of the rays from each point on the object, entered the cornea perpendicularly, and hence was unrefracted. The rays which were unrefracted were the most efficient in vision; the refracted rays formed only an indistinct impression. This simple device enabled Alhazen to say:- "the form will be arranged on the surface of the glacial humour just as it is on the surface of the visible object." This was the first time that a satisfactory explanation of all aspects of vision, including the formation of the image in the eye, had been given, based upon the intromission theory; the theory had now become a viable alternative, both in geometrical as well as physiological terms, to the emission theory based on the concept of visual rays.

The first Latin translation of Alhazen's work reached the West early in the thirteenth century, but its dissemination was not rapid enough to influence the optical work of Robert Grosseteste. We first see his influence in the writings of Roger Bacon, John Pecham, and Witelo at about 1260 to 1270. The range of sources available to these writers was considerable, including Greek, Latin and Muslim authors, but in spite of this it can be said that Alhazen exerted by far the dominant influence in the field of optics. All three writers held that vision occurred through rays entering the eye from the visible object, and striking the cornea perpendicularly. They all agree on the basic structure of the eye, with only minor differences from Alhazen. They all agree upon the glacial humour as the light-sensitive organ. There is no doubt, therefore, that Alhazen's work had a considerable influence initially upon contemporary Western writers on physiological optics. However, opinions on subsequent trends of thought tend to differ.

One view has been put forward by Vasco Ronchi; following the initial reception of Alhazen's work in the West, his ideas were not developed, and there was a regression of thought as scholars attempted to combine the new with the classical. Ronchi says:- "The merger was a monstrosity, with which the philosophers and mathematicians of the later Middle Ages tried to reason when confronted by optical problems." This trend was only reversed when the first printed edition of Perspectiva was published at the end of the sixteenth century. The other view is perhaps summarised by David Lindberg[1] who maintained that the initial impact of Alhazen's work was continued by the numerous copies made of the works of Bacon, Pecham and Witelo, and this led later scholars firmly along the paths they took. Lindberg says that even if later writers did not preserve Alhazen's ideas on vision in their works, it was not that they had failed to understand, merely that they were asking different questions.

One can best understand these two points of view if one first establishes what innovations Alhazen brought to the field of vision, and then considers how his views were absorbed and developed by later workers. Alhazen's main contribution to the theory of vision had undoubtedly been to suggest a point-by-point build up of the image by rays coming from the object. This had been in place of the Epicurean concept of the whole form of the object being transferred as a sort of 'skin' called an "eidolon" into the eye, which thus gained an impression of the exterior of the object. It is Ronchi's contention that Western thought tended to return to this concept after the initial reception of Alhazen's ideas. The term which came to be used in the West during the time under discussion was 'visible species' and Ronchi feels that this is analogous to the ancient "eidolon". Lindberg feels that 'visible species' is a similar alternative to Alhazen's 'forms of light and colour', and that the confirmation of this is to be found in the fact that the concept of 'species' still allowed the image to be built up in a point-by-point way - whereas "eidola" were the entire image of the body.

It is perhaps significant that the one area where a reconciliation of ideas had to take place between Alhazen and later workers in the West had its origin in the work of Grosseteste, whose work predated the dissemination of 'Perspectiva' in the West. Grosseteste thought vision could only be complete if rays of light were both received and emitted by the eye. Within the framework of such a general summary as this, it is probably sufficient to say that, in broad terms, Alhazen played the most significant part of any man in moving the study of physiological optics forward during the time preceding the seventeenth century; and that, while there might have been modifications made to his original ideas during the period between the reception of 'Perspectiva' in the West and its printing at the end of the sixteenth century, the process of printing his work placed his original ideas back in the centre of Western thought.

The stage was now set for a series of discoveries made in a comparatively short space of time. Leonardo (1452-1519) was probably the first to think that the image was formed on the retina, although he thought that the image was erect, and therefore he failed to determine the course of the light rays in the eye. Giambattista della Porta (1535-1615) added a convex lens to the camera obscura, but even so failed to realise that the image was formed on the retina; he thought that the image was formed on the surface of the lens, and that the retina merely acted as a reflector. Felix Plater (1536-1614) was the first to demonstrate experimentally that the lens was not the receptor. He disconnected a lens from its suspensory ligaments, which, it was supposed, carried the visual impulses to the brain, and showed that vision was nevertheless preserved. He thought that the retina was the true photo-receptor, the lens merely transmitting the image. He said in "De corporis humani structura", published in Basel in 1583:- "The principal organ of vision (is) the optic nerve dilated into the grey hemispherical retina after it enters the eye; which catches and discriminates the forms and colours of external things that flow with the illumination into the eye through the aperture of the pupil and are presented to it by its lens...." Nevertheless, it would be wrong to suppose from this that Plater had anticipated Kepler(1571-1630) in the way in which the image was formed on the retina. It is also true to say that the correct dioptric nature of the lens was not accepted until the formation of retinal images was explained by Kepler, and it would probably be accurate to say that his work on the eye marked the beginning of the modern era. His demonstration of the formation of the retinal image was mathematical, but it was followed fairly rapidly by experimental demonstrations by both ChristophScheiner and Rene Descartes. Kepler's theory was that every point of an object radiated or reflected light in all directions; those rays falling on the pupil of the observer's eye were projected on the retina to form the point on the image corresponding to that point on the object emitting or reflecting the ray. He said, in "Ad Vitellionem paralipomena", published in Frankfurt in 1604: "vision is brought about by a picture of the thing seen being formed (by the lens) on the concave surface of the retina. That which is to the right outside is depicted on the left on the retina... that above, below..." Thus, he realised that the eye acted as a sort of camera obscura, and, as he stated, formed an inverted image. His knowledge of lenses also enabled him to explain the defects of long and short sight.

Kepler continued his work in "Dioptrice" published in 1611. He appears to be the first to have recognised the need for the eye to be able to accommodate. He thought that this was achieved by a lengthening or shortening of the eyeball, so as to increase or decrease the distance between retina and lens. He supposed this distortion of the eyeball to be produced by the web-like structure - the ciliary processes - which supports the crystalline lens. On contracting, this would elongate the eye, and on relaxing, the natural elasticity of the eye would allow it to shorten.

Christoph Scheiner (1573 - 1650) in "Oculus sive fundamentum opticum" 1619, which was probably the first formal treatise on physiological optics, initiated a number of interesting experiments.

His most famous was undoubtedly the one in which he showed the formation of the inverted image on the retina, by removing the outer layers of the eyeball. He did this with both animal and human eyes. He also measured the curvature of the cornea by an ingenious but simple means. He compared the size of the image of a window or some similar object, in the cornea, with the image of the same object in a glass sphere, held at the side of the head by the temple. A number of glass spheres of different sizes enabled the experimenter to select the one which showed the image as nearly as possible to the one in the cornea. Scheiner also noted the minute forward displacement of the pupil during the act of accommodation, and also the tendency for the iris to close when viewing close objects. Like Kepler, he thought that accommodation was caused by the ciliary processes elongating the eyeball for close vision; but it is likely that he also thought that this process might produce a change in the shape of the crystalline. If this is so, he was probably the first person to suggest that a change in shape of the crystalline lens might be involved in the act of accommodation. Scheiner was also probably responsible for inventing a simple but ingenious experiment, which was used a great deal in the next two centuries, in the search for the true cause of accommodation. A card which contained two pin-holes, very close together, was held in front of the eye. If the object which was seen through the holes was at the normal viewing distance of the eye, then a single image was seen; if the object was not at the point where the eye was focussed, then two images were seen. This was because the two pin holes, which lay within the area of the pupil, allowed two pencils of rays into the eye, and these would only unite on the retina if the object was at the point of focus of the eye at that moment.

It is clear from what has just been written that Scheiner made a number of extremely significant experimental observations, but it would be wrong to attribute the same significance to his work as that which should be given to the work of the last scientist to be considered in this introductory section, René Descartes (1596-1650). He had an almost modern idea of the detail of the eye, and his work introduced a number of hypotheses which were as significant as those made by Kepler. In particular he improved our knowledge of the working of the eye and especially how it accommodated. The following is a translation of his description from La Dioptrique, 1637, and reprinted in Renati Descartes Opera Philosophica 1656. Chapter III of the section on dioptrics has the following description: [2]

> "If it were possible to cut the eye across a plane through the pupil in such a manner that none of the contained liquors could escape or any of the parts become displaced, the section would appear as represented in the diagram, Fig. I. ABCB is a membrane somewhat dense and hard, having the form of a vase which serves as a receptacle for all the interior parts. DEF is a thinner membrane which extends like a curtain over the inner surface of the former. ZH is what is commonly termed the optic nerve. It consists of a vast number of small figures, the extremities of which are spread over the whole surface GHI, where they are mixed with innumerable fine veins and arteries, thus forming a species of very tender flesh which appears as a third membrane covering all the base of the second. K, L and M are three very pellucid liquors which distend all these membranes and have the particular forms indicated in the diagram.
>
> Experiments have taught me that the medium L, which is called the crystalline humour, produces

nearly the same refraction as glass or crystal, and the two remaining humours a somewhat smaller refraction nearly equal to that of water, and hence the former medium (crystalline humour) transmits the rays of light more readily than the other two, and still more readily than air.*

In the first membrane the part BCB is transparent and more sharply curved than the remainder BAB. In the second the interior surface of the part EE which faces the base of the eye is all black and opaque and has at the centre of its anterior portion a small round hole called the pupil which appears very black when viewed from without."

"This aperture is not always open to the same extent, but the part EF of the second little membrane to which it belongs, floating perfectly freely in the very liquid humour K, has a kind of fine muscle which extends or contracts according as the objects which are viewed are near or far away, or are more or less strongly illuminated, or when it is the wish to view them more or less distinctly....."

EN, EN are numerous black filaments embracing on all sides the humour L. They originate in the second membrane at the place where the third ends and evidently form a species of minute tendons by means of which this humour (the lens), through becoming more curved or flatter according as it is desired to observe near or more distant objects, alters somewhat the whole figure of the body of the eye. This may be proved by experiment. For, if while looking intently at a distant tower or mountain a book is placed at a short distance before the eyes, none of the letters will be seen except indistinctly until the configuration of the parts alters slightly.

Finally O and O are six or seven external muscles attached to the eye by means of which it may be turned in all directions and, incidentally, also by pressure or retraction they may alter the figure."

From this account we see that Descartes associated the act of accommodation with 1) a change in the aperture of the pupil, and 2) a change in the shape of the lens, effecting a change in the shape of the whole eye. Thus, the tendon EN in the diagram would, at the same time, flatten the crystalline

*The paragraph here was written before the wave theory of light had been put forward.

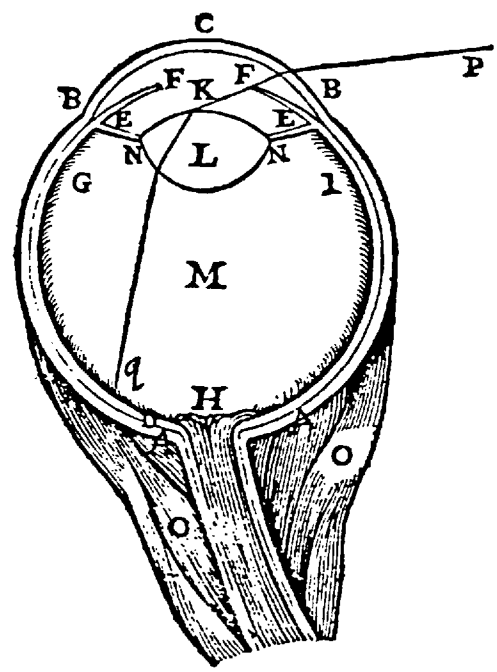


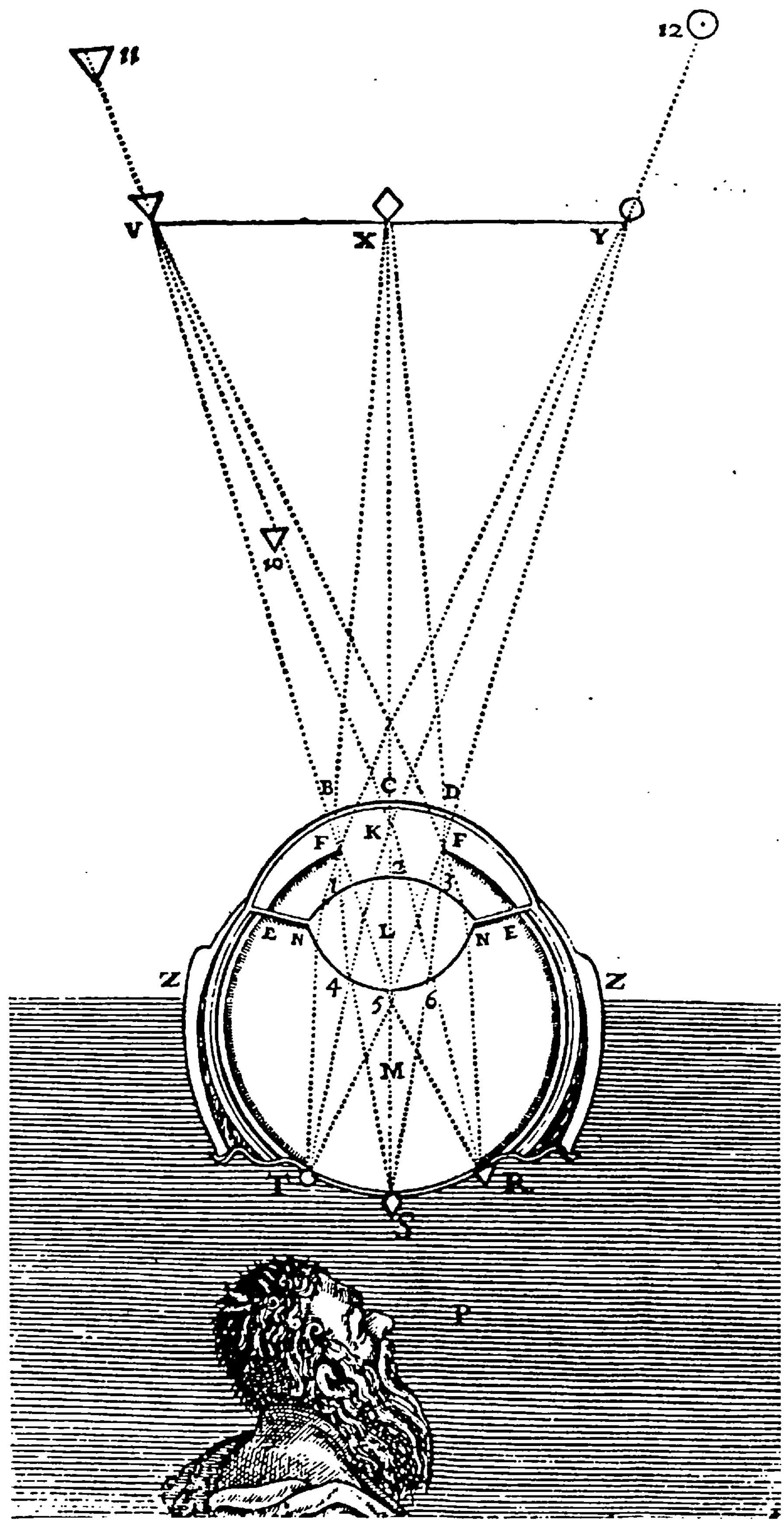
Fig. 1.

Diagram of a section of the eye from René Descartes'
"Treatise of man."
(The labelling of this diagram seems to be identical to that of fig. 1 referred to in the text.)

and also reduce the diameter of the eyeball and thus increase the axial length of the eye through the compression of the internal humours.

Later in his work, however, he seems to place more emphasis on the part played by the crystalline lens in accommodation:[3] "You must know that the shape of the humour is accommodated to refractions occurring elsewhere in the eye and to the distances of different objects"; and again:[4] "The change of shape that occurs in the crystalline humour permits objects at different distances to paint their images distinctly on the back of the eye."

Descartes, therefore, had been responsible for significant advances in the knowledge of the eye. The structure and relative refractive indices of the optical media were well defined by him, and he firmly indicated the correct method of accommodation. Unfortunately his theory was not immediately developed, since, as can be seen in the section on accommodation, his work was closely followed by De la Hire's experiments and hypothesis which concluded that no means of accommodation, apart from the contraction of the iris, was actually required.



OPTICAL ANALYSIS BY DESCARTES included an experiment in which he removed the eye of an ox, scraped the back of the eye to make it transparent and observed on the retina the inverted image of a scene. The illustration is from Descartes's essay *La Dioptrique.*

The formation of the inverted image, from Rene Descartes' "La Dioptrique".

References.

1. Isis, 1967, 58,321-341.

2. Mainly derived from Trans. Opt. Soc., 1919, XX, 209 - 36, and T.S. Hall trans. Rene Descartes: Treatise of Man, 1972, 50 - 58.

3. T.S. Hall, R. Descartes: Treatise of Man, 1972, 54.

4. Ibid., 56.

THE SEAT OF VISION

By the middle of the seventeenth century the commonly held opinion was that vision was caused by visible objects emitting rays or streams of corpuscles which entered the eye, forming an image on the retina; it was also accepted that this image was inverted and it was the retina which contained the nerves which transmitted the visual message to the brain.

The experimental work which had led to these conclusions, apart from contributions of Scheiner and Descartes, consisted mainly of dissections of the eye and examinations of the optic nerve. Scheiner and later Descartes, however, carried out an experiment which had a far greater significance than any other, and which, in addition to dissection, also contained an optical element. This experiment followed Kepler's mathematical demonstration of the inverted image on the retina, and was carried out first by Scheiner and later by Descartes. In it, the outer layers of the back of the eye were carefully removed until the retina, appearing like oiled paper, was exposed. The inverted image could then be seen projected on the retina. A second experiment which could also be considered to be of an optical rather than an anatomical nature was carried out by Edmé Mariotte (c. 1620 - 1684), Prior of St. Martin sous Beaune in Dijon and co-discoverer of Boyle's Law. This was the experiment which led him to the discovery of the blind spot. In itself this was a significant enough achievement, but Mariotte drew conclusions from his experiment which threw into question the role of the retina. He argued that because the retina covered the blind spot, it could not be responsible for vision. The passage of the optic nerve through the choroid (the membrane next to the retina) created the blind spot, and therefore the choroid was the light-sensitive surface within the eye. Mariotte was, of course, wrong in his deduction, but his experiment caused the subject of the seat of vision to be discussed and examined; ironically the discussion and examination may have helped ultimately to strengthen the belief in the retina as the light-sensitive surface.

The controversy caused by Mariotte's discovery and hypothesis was considerable, and the arguments for and against his theory will be examined in detail in the next few pages. Indeed the discussion of the choroid-retina argument occupies a major part of this section; and because of this it would be easy to over-emphasise the importance of Mariotte's theory. In fact there was never any widespread support for the choroid as the light-sensitive surface in the eye, and opinion never really wavered from a general acceptance that the retina was responsible for the reception of light and for the transmission of the resultant impulses to the brain. The maximum influence which Mariotte could be said to have achieved was on the hybrid theory put forward by De la Hire, which attributed vision to a composite effect produced by the retina and choroid combined.

Mariotte recorded the results of his experiments in a letter written in 1668 to Jean Pecquet (1622 - 1674), a physician. His letter, entitled "Nouvelle Découverte Touchante la Veue", was published in Paris and an extract was given in English in Philosophical Transactions.[2] Mariotte noted that the optic nerve in man and animals does not enter the retina at the place where the image of an object is made when we look directly at it. The entry of the nerve in man is somewhat higher and towards the nose, and there had been a long-standing curiosity whether sight is weaker or stronger at the entry of the optic nerve. In order to find out what happened when the rays from an object fell on the optic nerve, he carried out the following experiment:[3]

> "I fastened on an obscure wall, about the height of my Eye, a small round paper, to serve me for a fixed point of Vision; and I fastened such an other on the side thereof towards my right hand, at the distance of about 2 foot, but somewhat lower than the first, to the end that it might strike the Optic Nerve of my Right Eye, while I kept my Left shut. Then I placed myself over against the First paper, and drew back little by little, keeping my Right Eye fixt and very steddy upon the same; and being about 10 feet distant, the second paper totally disappear'd."

Mariotte pointed out that other objects surrounding the paper were easily visible, and the paper itself could be made to reappear by the slightest movement of the eye. He repeated the experiment at different distances with each eye:

> "This experiment I made often, varying it by different distances, and removing or approaching the Papers to one another proportionally. I made it also with my Left Eye, by keeping my Right shut, after I had fastened the Second paper on the left side of my point of vision; so that from the Site of

> the parts of the Eye, it cannot be doubted
> but that this deficiency of Vision is upon
> the optick Nerve."

He carried out other experiments in the Library of Académie Royale des Sciences, where it was discovered that the size of paper which could be made to vanish varied from person to person; a result which Mariotte attributed to the varying size of the optic nerve in different individuals. He concluded by saying:[4]

> "This Experiment hath given me cause to doubt whether Vision was indeed performed in the Retina (as is the common opinion) or rather in that other Membrane, which at the bottom of the Eye is seen through the Retina, and is called the Choroides. For if vision were made in the retina, it seems that then it should be made wherever the retina is; and since the same covers the whole nerve, as well as the rest of the bottom of the eye, there appears no reason to me why there should be no vision in the place of the optic nerve where it is; on the contrary, if it be in the choroides that vision is made, it seems evident that the reason why there is none on the optic nerve, is because that membrane (the choroides) parts from the Edges of the nerve and covers not the middle thereof, as it does the rest of the bottom of the eye."

Pecquet disagreed with Mariotte's hypothesis and replied[5] in detail quoting the points with which he took issue and giving his reasons. There was also a paraphrase of part of his reply translated into English and published in the Philosophical Transactions.[6] (It may be observed that not all the points which Pecquet attributed to Mariotte occur in Mariotte's 1668 letter, and it would appear that he wrote another letter to Pecquet, since one was mentioned in Mariotte's "Œuvres").

In order to be able to appreciate fully the exchange of opinion, it will probably be most convenient to assume that Pecquet was correct in his statement of Mariotte's reasoning. Thus Pecquet quoted Mariotte's writing on the retina as follows:

> "It is transparent and receives only a very small impression of the light, no more than transparent substances such as water and air, while on the other hand black and opaque substances, such as the choroid are easily illuminated by light."

Pecquet disagreed on two grounds. First, he queried the comparison of the transparency of the retina to air and water; he said it was more like that of oiled paper. With a fresh eye, if the retina was carefully exposed by cutting away the sclerotic and choroid, "the opacity of the retina can still be seen." Secondly, the blackness of the choroid that Mariotte judged necessary for vision was not found in all sorts of eyes: it varied in degree from man to man, and in animals many different colours were found. Pecquet also made the point that the retina was in front of the choroid and therefore an obstacle to the light falling on it.

He attributed to Mariotte the opinion that the retina did not penetrate into the brain, as did the choroid, which went on to envelop the optic nerve as far as the centre of the brain. Pecquet did not find this surprising, since the retina had its origin in the extremity of the optic nerve which ended at the back of the eye. It was made up of very fine filaments which could come only from the nerve, and which were rendered visible when the membrane of the retina was put in water.[5]

> "It is easy to see what continuity the retina must have with the brain, since by the means

> of the Optick Nerve from which one can say it is produced, it takes its origin from the main part of the brain."

In fact, he said the retina was made up of a spreading out of the filaments which come from the nerve:[5]

> "My conception is that at the exit of the nerve the spreading out of the filament is as the fibres which leave the stem of a plant and stretch to all parts and so form a flower above the stem."

Pecquet again quoted Mariotte's opinion that for clear vision, rays must meet at a point, and since the retina has a thickness of half a line (1/24th of a French inch) the rays would fall on it in different parts. However, the choroid, being opaque, would allow light to fall on a single point. Pecquet countered by stating that the rays did not have to unite in a mathematically exact point and in any event the opacity of the retina would prevent the formation of images on the choroid. He put forward an alternative reason for the loss of vision at the blind spot. At this point there are in the retina the trunks of the veins and arteries of the retina, which would certainly be sufficiently large to prevent vision. Pecquet also made the point that the smaller vessels which issue from the main trunks were sufficiently small not to hinder vision on the parts of the retina used for distinct vision.

The modern view of the retina is that it is like a transparent carpet lying upside down on the floor of a room, with the pile of the carpet corresponding to the rods and cones lying underneath. It is this back-to-front nature of the retina that results in the blind spot. If the optic nerve, on entering the eye, spread out in all directions, it would be possible for there to be a continuous layer of rods and cones with their endings facing the light, so that there would be no blind spot. In fact, the optic nerve pierces the retina and the nerve fibres which are insensitive to light, run on the surface of the retina next to the vitreous humour and then turn away towards the outside of the eye to connect with the rods and cones. Thus, Mariotte's reasoning that the retina could not be the light-sensitive membrane, since if it was, there would be no blind spot, would have been valid, but for the reversed nature of the retina. Therefore we "see", i. e. light reaches the light-sensitive nerve endings, after light has passed through the retina, and Pecquet was correct in assuming that the blood vessels of the retina were too fine to hinder vision; though he could not have been aware of the reversed nature of the retina, and perhaps not of the fovea centralis, the area of most acute vision which contains no blood vessels. Mariotte, however, was aware that the area of the retina on the optical axis had no blood vessels, since he later used this fact to answer an objection to his theory by Perrault.

Mariotte's reply to Pecquet's letter was published in English in Philosophical Transactions[7] where it was given in greater detail than in his original letter. He emphasised again the point mentioned above, that he could not see any reason for the absence of nerve endings at the end of the optic nerve, and if there were nerve endings, the blind spot would be sensitive to light. It would need only a simple direct continuation of its fibres into the anterior part of the retina. He thought that the vessels which proceed from the base of the nerve were very small and if the rest of the nerve were sensible to light, we should not have such a large insensible area as would cover the image of a 2" diameter circle of paper at a distance of 10 feet. The degree of transparency of the retina was largely a matter of opinion. whether, as previously stated by Mariotte, akin to water, or as Pecquet would have it, like oiled paper. However, Mariotte persisted with his view in this second letter, giving further observational evidence to emphasise its transparency. Thus, he said that we can see the choroid through the retina in a dissected freshly-killed eye, and for this, light has to pass twice through the retina. We could also see light from a candle reflected from the choroid of a dog or cat which have light coloured choroids, but not from a man and birds and other animals with black choroids.

Thus, he said that light does pass through the retina to the choroid and the retina receives very little impression. He demonstrated that this light was reflected from the choroid by using a round glass bottle full of water with white paper close behind. A candle was placed in front of the bottle so that an image was formed on the paper. If one placed an eye near the candle, the bottle appeared full of light, which disappeared if the paper was moved too close to or too far from the bottle. Mariotte said that the light from a dog's eyes came from a like cause, and while the image on black choroid, such as in human beings, was obscure and could not be seen, the impression made by the image was much stronger. In order to link the choroid to the brain, a connection denied by Pecquet, Mariotte said:[8]

> "The nerves are all coated with the Pia Mater (which envelops the spinal marrow) and have with it the same continuity of fibres; so that if these nerves never so little be moved, the impression is carried to the brain."

The choroid was an expansion of the Pia Mater which enveloped the optic nerve internally - so that any impression on the choroid was easily transmitted to the brain.

For the retina:[9]

> "there must be a little channel in the optic nerve through which the retina in its proper substance extends itself to this tuberosity by a continuity of its fibres; which is not seen, and you are constrained to say that there are little filaments of nerves which come from the interior of the optick, and expand themselves through the retina which have this continuity; but if there were these filaments, they should spread themselves through the retina, as from a centre to a circumference, and should lye closer together near the optic nerve, than a good way further in the retina; which nevertheless we do not observe to be so."

Thus Mariotte denied that he could see any nerve filaments coming from the optic nerve as Pecquet held (P. 43.) In any case he said that this would not give proper vision:[10]

> "since they would leave the parts of the retina too great intervals empty; and it is necessary that every point of the object find a sensible point in the origin of sight to unite his rays there; which is found in the choroids, which is an expansion of the sensible part of the nerve into a continued membrane."

Mariotte added a last argument. He said that it was not known what caused the pupil to dilate and contract with variation in the intensity of light; but if the choroid was sensitive to light according to his hypothesis:[11]

> "It is easy to conceive that it being hurt by too strong vision may cause it to dilate or contract its fibres which have one continuity with those of the fore part of the uvea so that it can contract its aperture, and when it is not hurt, relax it again; whereas if the retina be supposed to be the organ of sight it will be very difficult to explain how this contraction is made."

In 1682 the argument on the seat of vision was re-kindled by Claude Perrault[12] (1613 - 1688) who wrote to Mariotte stating his objections to his theory. Perrault thought that the blood vessels in the retina would hinder the choroid in its function as the seat of vision; he thought the choroid too rugged, shiny and dirty, and also lacking any connection with the optic nerve. Mariotte denied that the blood vessels in the retina would interfere with vision, since there were no blood vessels in the part of the retina lying on the optic axis of the eye and thus our direct view of an object would not be hindered. He described an ingenious and discerning experiment by which he located the optic axis of an eye by measuring half way round the outside from the centre of the cornea, and marking this place on the retina. When the eye was dissected, he noted that there were no blood vessels near this spot on the retina. For oblique rays, the defects of part of an image caused in one eye would be compensated for by the other eye. Also, since our eyes are in a constant state of motion, the image would be constantly projected through slightly different areas of the retina. Mariotte stated that the choroid did connect with the optic nerve, not with the marrow, or inner part, which he maintained was insensible, but with the membrane which encompassed it (part of the Pia Mater) and which was the true organ of sense both for eye and ear. Perrault's explanation of the lack of vision at the optic nerve was that the entry of the fibres of the retina at this point formed a bundle which gave a rough surface which was insensitive. Mariotte denied the fibrous nature of the retina, saying that he had looked at many through excellent microscopes and found only a uniform mucousness, without any fibres, only veins and arteries. This was a surprising oversight in an observer who was sufficiently skilled to have noticed the lack of blood vessels in the fovea, a fact which had escaped the majority of Mariotte's contemporaries. In conclusion Mariotte said that although his theory had few followers, he was nevertheless unwilling to abandon it. The lack of impact made by Mariotte's theory can indeed be considered most surprising. The theory was soundly based upon excellent experimental evidence, while opinion in favour of the retina relied upon more indirect evidence - such as its fibrous nature which was interpreted to show that it contained nerves. During the next hundred years, opinion in favour of the retina slowly became confirmed; although as we shall see, even Joseph Priestley was considerably impressed, but eventually unconvinced, by the arguments in favour of the choroid as the seat of vision.

Apart from the series of letters between Mariotte, Pecquet and Perrault there was little contemporary writing on this subject. Isaac Newton (1642 - 1717), outlined his opinion that the light sensitive membrane was the retina in a letter in 1675 to Henry Oldenburg (1615 - 1677) although it should be added that the letter was not primarily about the eye, but about the production and transmission of colours. He supposed that bodies of different colours gave off vibrations to the air of different sizes, and these in turn excited vibrations in the æther. He then continued:[13]

> "And therefore the ends of the Capillamenta of the optic nerve wch (sic) pave or face the Retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations wch vibrations (like those of sound in a trunk or trumpet,) will run along the aqueous pores or Crystalline pith of the Capillamenta through the optic nerves into the sensorium (wch Light itself cannot doe,) and there I suppose, affect the sense with various colours according to their bigness and mixture."

In 1682 William Briggs (1642 - 1704), Fellow of Corpus Christi College, Cambridge, presented a paper to the Royal Society entitled "A New Theory of Vision".[14] He also sent a copy to Newton who was politely unimpressed by Briggs' theory, although later - probably because of the Cambridge connection - he was prevailed upon to write a more favourable opinion. Briggs later presented another paper on the subject.[15] He held a contrary opinion to the majority and thought that neither the retina nor the choroid was primarily concerned with the process of seeing. He endeavoured to show that the fibres of the optic nerve were more concerned with vision than the cornea, humours or retina, and that sensation was performed purely in the brain.

His argument was that if the optic nerve was damaged, then blindness followed, even though the eye might be perfect. It was also observed that the optic nerves from each eye, although being apparently united before reaching the brain, did in fact keep their separate order. Within the eye the white colour of the retina was thought to be more suited to receiving the images of coloured objects than the dark choroid. He also noted that it had a fibrous nature which became apparent when it was put in warm water and that, since it was a medullary expansion of the optic nerve, it could more easily transmit impressions to the 'meditarium' of the brain; the choroid, being a continuation of the Pia Mater, did not meet the brain.

Another scientist in favour of the retina was James Keill[16] (1673-1719), a physician: "The impression of the object is made on the retina. The choroides is tinctured black, that rays of light which pass through the retina may not be reflected back to confuse the image of the object."

A scientist of much greater influence was Phillipe de la Hire (1640 - 1718)[17] who held the opinion that the retina was the principal organ of sight, though he was impressed by Mariotte's experiments; and it was Mariotte who probably led him to propose his own ingenious variation on the theory of the retina as the principal organ of vision, which will be mentioned shortly. He noted the fact that it was the almost universal opinion of those who had written on the subject that the retina was the sensitive membrane. He could not imagine that there would be sensitivity to light other than where there were nerves, and he said that the retina was a tissue of the nerve fibres of the.optic nerve which lie over the whole of the bottom of the eye. He was also aware that there was a place on the retina where vision was most sensitive, and that we have a habit of turning our eyes so that the focus of the rays from objects that we wish to see clearly fall on this spot. That he was influenced by Mariotte is shown clearly in the following extract:[18]

> "I do not think that one can attribute the defect of vision to other matters than the defect of the choroid; but I do not think that because of that one must regard the choroid as the principal organ of sight. In order to find some explanation of this difficulty, it is necessary to consider what happens at other senses, and it seems to me that by comparison, one can very well prove that the retina is the principal organ of vision, while having an opening which is not sensible to exterior objects. I say then that the retina is the principal organ of sight, as being an expansion of the optic nerve; because we must not seek for sensations anywhere else but in the nerves. But yet this organ must receive the impression of light through the medium of another organ which would receive them from the object itself; from which it is clear that it is necessary that this intermediate organ is the choroid since it touches and supports the retina, and which is of an obscure colour more appropriate to be disturbed by the impressions of light, than if it was white and transparent."

De la Hire pointed out that it was the colour and transparency of the retina which was used as a principal argument against it by Mariotte. He concluded the first part of this dissertation by saying:[19]

> "I say also that it was necessary that there should be in the eye a part which could receive easily all the different impressions of light, so that it could transmit them to the principal organ, where they would be made sensible by a proper modification to that effect; and that this is found in the choroid. Thus the retina will not be touched by light, as is necessary to be aware of objects, when it does not receive impressions from the choroid and consequently there will still be a defect of vision at the entry of the retina which is not supported by the choroid."

Thus he held that the retina, containing the nerves, was responsible for transmitting the impressions of light to the brain, but in itself it was too transparent to be affected by the light, and thus it relied on the choroid, immediately behind it and opaque, to receive the impression of the light and transmit it to the retina. It was a necessary corollary to this hypothesis that there should be no impression of light sent from the part of the retina which had no choroid behind it, i.e. the blind spot.

This was an ingenious compromise hypothesis, supporting the current view in favour of the retina as the seat of vision, but explaining a previous difficulty: the non-sensitivity of the blind spot, even though the retina (supposedly sensitive to light) covered the spot. It also incorporated the transparency of the retina into the theory where there had previously been a point of difficulty. However, no attempt was made to explain the way in which the image was transferred from the choroid to the retina, and particularly how or what stimulated the nerve fibres which he stated were contained in the retina. The most likely explanation would be that the choroid somehow reflected light back to the retina; although it would be difficult to argue this case when considering the black choroid of men.

In 1738 Robert Smith (1689 - 1768), Professor of Astronomy and Master of Trinity College, Cambridge, published his large work on optics.[20] In this he included extensive quotations of translations of Mariotte's, Pecquet's and Perrault's letters and also summarised de la Hire's view given above. Indicating his own belief, he wrote:[21] "...the retina, being like a fine net composed of the fibres of the optick nerve woven together"; and more definitely:[22]

> "This account of the eye and the cause of vision is farther confirmed by these arguments; that anatomists when they have taken off from the bottom of the eye that outward and thickest coat called the dura mater, can see through the thinner coats the pictures of objects lively painted thereon. And these pictures propagated by motion along the fibres of the optick nerves into the brain, are the cause of vision."

This was a very clear statement in favour of the retina, although, as we shall see in the conclusion to this section, the image seen on the exposed retina is not a decisive argument in favour of the retina as the light-sensitive surface.

However, opinion at this time was clearly coming to favour the retina as the seat of vision. This can be seen in a treatise by Claude Le Cat (1700-1768), which was published in 1740, in which he emphasised that the current view in favour of the retina was strongly influenced by its links with the brain through the optic nerve:[23]

> "The prevailing opinion that the sensations are conveyed to the very substance of the

> brain has been the reason why the immediate organ of sight has hitherto been placed in the retina, which is an expansion of the substance of the brain contained in the optic nerve."

Repeating Mariotte's experiments, he determined the angle between the optic axis and the axis of the optic nerve of the eye. He noted that the circle of paper vanished for him at eight feet and not ten, as found by Mariotte, and at eight feet the size of paper to vanish was nine inches in diameter (much larger than Mariotte's two inches.) He confirmed this at proportionately larger distances: at sixteen feet the paper size to vanish was eighteen inches, and at twenty-four feet the size was twenty seven inches in diameter. From simple geometry, Le Cat calculated that the size of the blind spot was no larger than a small pinhead, or a third or even fourth part of a line.

Le Cat was the first for many years to consider the cause of erect vision. He was of the opinion that the inverted image on the retina was seen erect owing to our sense of touch. He thought that the sense of touch was the only sense which could arbitrate over the situation of bodies, whether they were erect or inverted; and therefore this over-rode any impression we might obtain visually. Thus Le Cat believed that we learnt from our sense of touch, to see objects erect.

The next major work on the eye was published in 1759 by William Porterfield (1696/7 - 1763). This was a work devoted entirely to a discussion of the eye and the way in which vision was effected, and yet the author touched only briefly on the major topic of the seat of vision and reached no definite conclusion. He contented himself with a description of the retina, [24] saying that it was composed of small fibres, not unlike a piece of plush with the ends of the thread turned towards the crystalline and the other ends terminated in the brain. He was of the opinion that there could be no more distinct sensations than there were distinct threads to convey them. It is possible to infer from this sensible description of the retina that Porterfield had a good understanding of the retina and its working. This makes his neglect of this subject all the more surprising, since his style as an author tended to be lengthy and repetitive, and one wonders why he suffered this attack of brevity when dealing with such an important topic.

When Joseph Priestley (1733 - 1804) published his conclusions on this topic in 1772, [25] he adopted a somewhat vacillating position. While giving an impressive list of reasons in favour of the choroid, he came to a decision in favour of the retina. However, this was obviously a topic which exercised his mind considerably, and he returned to it later in his book, [26] when he favoured a solution which was similar to that of De la Hire, and involved both the retina and choroid in the process of vision. Priestley thought that the retina had considerable thickness and was uniformly nervous throughout this thickness, and that it was difficult to imagine rays being focused clearly and sharply throughout this depth; and that consequently vision would be confused. If the seat of vision was the near surface of the retina, images and objects formed would be considerably confused by light reflected from the choroid in animals with white or coloured choroids. On the other hand, it was impossible for light reflected from the choroid to come back to the retina because in several animals it was perfectly black, and reflected no light. He supposed that in whatever manner vision was effected, it was the same in the eyes of all animals. An argument which he felt was in favour of the choroid being the organ of vision was that it received a more distinct impression from the rays of light than any other membrane, whereas the retina was a substance on which light makes an extremely faint impression, and perhaps no impression at all. In addition, the retina was exposed to many rays which did not terminate in it. Priestley said that this was not the case with the choroid which was in no case transparent and had no reflecting substance behind it.

In spite of this impressive list of reasons in favour of the choroid as the seat of vision, Priestley, in this section of his book, still favoured the retina as the seat of vision.

In the section of his book which has just been considered, Priestley gave a comprehensive historical survey of the theories favouring both the retina and choroid, which indicated the importance which he attached to the subject. The fact that he returned to the topic after the first part of his book had been printed emphasises this fact; and it is clear that Priestley was far more uncertain of the nature of the light-sensitive surface than many earlier scientists. In view of the conflicting evidence on this topic, this uncertainty was not to Priestley's discredit. He was impressed by De la Hire's argument in favour of the retina, that it contained many nerves:[27]

> "M. De la Hire's argument in favour of the retina, from the analogy of the senses, is much strengthened by considering that the retina is a large nervous apparatus, immediately exposed to the impression of light; whereas the choroides receives but a slender supply of nerves, in common with the sclerotica, the conjunctiva, and the eyelids, and that its nerves are much less exposed to the light than the naked fibres of the optic nerve."

Priestley even went back to first principles, as it were, giving experimental evidence that the optic nerve was in fact principally involved in the process of vision. He said that in certain cases of blindness the optic nerve, which went on to form the retina, was found to be smaller and harder than usual, while the nerves which went to the choroid were found to be normal. He came to a final opinion that the retina and choroid might both be involved in the process of vision, a conclusion, as he said, which was not far from the hypothesis of De la Hire:[28]

> "I shall conclude these remarks with observing that, if the retina be as transparent, as it is generally represented to be, so that the termination of the pencils must necessarily be either upon the choroides, or some other opaque substance interposed between it and the retina, the action and reaction occasioned by the rays of light being at the common surface of this body and the retina, both these mediums (supposing them to be equally sensible to the impression of light) may be equally affected; but the retina, being naturally much more sensible to this kind of impression, may be the only instrument by which the sensation is conveyed to the brain, though the choroides, or the black substance with which it is sometimes lined, may also be absolutely necessary for the purpose of vision. Indeed, when the reflexion of light

> is made at the common boundary of any two mediums, it is with no propriety that this effect is ascribed to one of them rather than the other, and the strongest reflexions are often made back into the densest mediums, when they have been contiguous to the rarest, or even to a vacuum. This is not far from the hypothesis of M. De la Hire, and will completely account for the entire defect of vision at the insertion of the optic nerve."

The final word in this section will be left to Andrew Horn who introduced yet another theory, in spite of the evidence to the contrary summarised above and in spite of the three-colour theory introduced earlier by Thomas Young, which will be discussed later, and which assumed the retina as the light sensitive membrane. Horn produced a comprehensive historical survey[29] of the theories on the seat of vision, and then introduced his own theory. He examined numerous retinas from oxen with a magnifier and could find no area which he felt would give rise to superior sensibility; he drew the conclusion that the retina is insensible to light. His opinion was that it is an expansion of the scepta, or membranous substance that pervades the optic nerve, and that its sole use was to produce reflection in the same way as the glass of a mirror, with the choroids behind serving the purpose of the metallic coating on the convex surface of the mirror.

Horn was impressed with the size of the optic nerve, and the way it did not terminate in branches, but had a well-defined circular base fringed with the choroid and covered with the retina. He put forward his idea of the way vision is accomplished:[30]

> "Rays from all points of such objects as are opposed to the organ pass through the pupil, and, after refraction in the different humours, form delicate and perfect, but inverted pictures on the retina at the bottom of the eye; these pictures are instantly reflected in their various colours and shades on the anterior portion of the concavity; and another reflection from thence raises images of the external objects near the middle of the vitreous humour, in their natural order and position; these images make due impression on the opposite base of the nerve, which are transmitted by it to the brain; thus the sensation is produced and vision perfected."

Horn maintained that the image formed in the vitreous humour was upright, and thus overcame the problem of the inverted image on the back of the eye which had not yet been satisfactorily resolved. He went on to say:[31]

> "However, having demonstrated that neither the retina nor the choroides is the immediate seat of vision; and having restored the optic nerve to that dignified function in the theory, which it naturally possesses in the organ, all the inferior instruments will be found harmoniously co-operating with it in producing the various phenomena of vision."

He said the retina's covering the optic nerve protects it and moderated the impression of the rays. It is perhaps a pity that this section could not have ended on a more positive and accurate note, but wrong as it was Horn's theory does serve the purpose of emphasising the continuing difficulty which the nature of the seat of vision presented to scientists.

It is surprising that, during the seventeenth and eighteenth centuries, the examination of the nature of the seat of vision produced far less comment and discussion than did the process of accommodation, in spite of the added interest injected into the topic by the choroid controversy arising from Mariotte's discovery of the blind spot. Although it was not the primary subject for investigation during this period, as might have been expected, much of the work done in the investigation of the light-sensitive surface was of the highest calibre.

Kepler's mathematical demonstration of the inverted image formed on the back of the eye clearly established that the eye functioned in the same way as the camera obscura; it was also reasonable to deduce from his work that the image was formed on the retina. Scheiner's experimental demonstration of the inverted image on the retina not only confirmed Kepler's geometry, but also apparently pointed clearly to the retina's being the seat of vision. Therefore, by the time Mariotte came to question the function of the retina, opinion had largely been influenced by the work of Kepler, Scheiner and later, Descartes, in favour of the retina. Scheiner's brilliant experiment, however. had one inherent defect. It purported to show the internal operation of the eye to an outside observer. The fact that it was possible to see the inverted image projected through the retina, was generally held to show that it was the retina which received the image and transmitted it to the brain. However, since an outside observer could see the image, it could equally have been said that the image passed through the retina and could have influenced the choroid. This was a view held only by Mariotte, De la Hire and later, by Priestley; and of these three only Mariotte believed that it was the choroid which contained the nerves which transmitted the image to the brain.

The probable reason why opinion was so consistently in favour of the retina, in spite of Mariotte's excellent work, was the fortunate early discovery and understanding of the nature of nerves. It was probably this which gave the many writers who favoured the retina an almost instinctive prejudice in its favour, based in many cases upon little more than the discovery that the retina was fibrous - the fibres being correctly interpreted as nerves.

Therefore, apart from Mariotte, there was no scientist of repute between 1600 and 1800 who doubted that it was the retina which received the image and transmitted its details to the brain via the optic nerve. This belief has been verified experimentally only in very recent years with the discovery of the action of rods and cones. (P. 315).

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VISUAL ACUITY

The ability of the eye to resolve fine detail was a subject which might be expected to lend itself readily to experimental investigation; yet it was an area in which scientists who had shown their ingenuity in other experimental fields seemed only to touch the surface. Visual acuity depends upon the nature of the retina, and it would have been understandable if an inability to appreciate the nature of the retina had somehow inhibited the ability of scientific workers to explore the subject experimentally. This was not the case, however, since the nature of the retina was apparently well understood as early as the middle of the seventeenth century. At that time it was considered to be analogous to a piece of velvet with the pile facing into the eye, with each filament of the velvet being equivalent to a nerve ending. In fact we now know that the nerve endings are on the outside of the retina, and that light has to pass through the retina before it can reach them; nevertheless the analogy of the velvet pile was sufficiently accurate to enable workers to understand well the working of the retina.

By the middle of the seventeenth century Kepler's mathematical demonstration of the inverted image on the retina, and Scheiner's and Descartes' experimental verification of his calculations, had clearly established a foundation on which the study of physiological optics could be developed in many directions. For the first time the actual image formed within the eye had been seen, and it was not surprising, therefore, that the degree to which this image could reproduce the detail of the original object should come under consideration.

Robert Hooke [1] (1635 - 1703) was the first writer to attempt to define the clarity with which we could see. He postulated that the eye could differentiate between two points which subtended an angle of one minute at the eye.

> "Now whereas most eyes distinguish not a less angle than a minute, or the 60th part of a degree, or the 21600 part of a circle, therefore whatever is sensated or seen by it, is seen of that bigness or under that angle; and so if there be two or three or 10 or 100 small stars so near together as that they are all comprised within the angle of 1 minute, the eye has the sensation of them all as if they were one star. Likewise if the light be strong and powerful so as to affect the eye, it always appears of the bigness of 1 minute; though possibly its real angle be not 1 second."

This basic measure of the acuity of the eye became established as the starting point for many scientists who wrote on this subject during the next hundred years. In fact the acuity of the eye is about twice as good as this under ideal conditions, but in normal daylight, one minute of arc is considered to be a reasonable measure of the eye's resolving power. In view of Hooke's overall grasp of this subject, it is probably fairer to ascribe the accuracy of his result to observation, rather than to luck.

Hooke also showed a clear appreciation of the way the retina worked; he even understood the different effects that dull and bright objects would have on the retina. For dull objects he maintained that the eye was incapable of distinguishing the parts of any picture which were smaller than the filaments of the optic nerve. For a very bright object he maintained that:[2]

> "For a very bright radiation the whole filament is moved by having one part acted upon and the sensation of the object is the same as if it were bigger - this is why stars appear to our naked eye many thousands of times larger than they really are, and even as big as through a long telescope."

Unfortunately, De la Hire's contribution to this subject was not as significant as Hooke's, and it did not have the stature of De la Hire's work in other areas of vision. He took his example from experience, and estimated that he could see clearly the blade of a windmill 6 feet long at a distance of 4, 000 fathoms. Without any explanation he deduced from this that the image on the back of the eye would be 1/8000 inch, which is less than 1/666 of a line. He estimated a line to be equivalent to 10 average hairs. Thus the image of the blade of the mill was 1/66 of an average hair, or, he said, 1/8 of a strand of silk. He concluded that a strand of the optic nerve could not have a breadth greater than 1/8 of a strand of silk. De la Hire found the product of his own reasoning to be almost inconceivable, especially as he maintained that each strand must be a tube containing "spirits". He also said that birds, with their keener sight, must have strands of the optic nerve even finer than those of humans.

In his "Essay upon Distinct and Indistinct Vision", Jurin investigated the limits of vision more thoroughly than any other previous author. He took as his starting point Robert Hooke's view that the majority of people could distinguish objects to a limit of 1/60 of a degree; but he pointed out that the nature of the object affected the minimum size which could be seen. In the case of a round object, such as a black circle on a white ground, or a white spot on a black ground, his experiments led him to agree with Hooke that it was difficult to perceive them at an angle much less than a minute. In other cases, however, he maintained that a smaller angle could be discerned by the eye. For this to happen, the impression made on our senses must come with a certain degree of magnitude. He used the analogies of a drop of dew or rain which could fall on the back of the hand without it feeling wet; or a small particle of sugar on the tongue which does not give the impression of sweetness. However, a spark of a fire, of the same size as the drop of rain, falling on the hand will give a very definite sensation, because the impression of the spark of fire is of a greater force than the drop of water or speck of sugar. Jurin said that for the same reason we could see a star as a clear point through a telescope, even though it subtended an angle of only one second, though a white or black spot of 25 - 30 seconds is not perceived. A line of the same breadth as a circular spot would be seen, even though the spot would be invisible, because the "quantity of impression" from the line is greater; also a longer line is visible at a greater distance than a short line of the same breadth:[4]

> "The learned Dr. Hooke asserts, that when an object subtends a less angle than a minute, it is to the generality of eyes wholly invisible. And by the experiment related in art. 97 of this book, and one that I have made myself,

within the limits of Perfect Vision,* I am inclined to think, that when the object is round, as a black circular spot upon a white ground, or a white spot upon a black ground, an eye must be exceeding good to perceive it under an angle much less than a minute.

160. But there are other cases, in which a much less angle can be discerned by the eye, some of which we shall here consider, after premising one observation, which seems necessary for explaining the reasons of them.

In order to our perceiving the impression made by an object upon any of our senses, the impression must be either of a certain degree of force, or of a certain degree of magnitude.

For instance, a very small drop of dew, or rain, may fall upon the hand without our feeling it wet; and a very small particle of sugar may be laid upon the tongue without our tasting it sweet; but a spark of fire of the same magnitude with the drop of rain, by falling upon the hand will sensibly affect it, because the impression of the spark of fire is of a greater force than that of the drop of water, or particle of sugar.

And for the same reason, a star which appears only as a lucid point thro' a telescope, not subtending so much as an angle of one second, is visible to the eye, though a white or black spot of 25 or 30 seconds is not to be perceived.

161. Also, though one very small drop of water will not sensibly affect the hand, yet a number of such drops falling together, or one larger drop falling alone, will affect the hand with a sense of wet, because the quantity of the impression is greater."

*(Jurin described "Perfect Vision" on p. 116 of his essay as follows: "Perfect Vision , is that in which the rays of a single pencil are collected into a single physical, or sensible point of the retina").

Jurin then went on to consider the nature of the retina. He stated that some people held the opinion that the impressions of objects were received on "villi" of the optic nerve which stood erect on the surface of the retina, although he did not specifically embrace this opinion himself.[5]

> "162. It has been by some persons supposed, that the impressions of visible objects are received upon certain villi of the optick nerve, imagined to stand erect upon the retina, like the pile on velvet. And from the experiment that a spot less than one minute in diameter cannot be perceived by the eye, it would follow that the thickness of one of these villi is about 1/5000 or 1/6000 part of an inch."

From this it is clear that a remarkably accurate idea of the working of the retina had been postulated by this time, and that the concept of individual nerve endings sending messages to the brain, which then built up a composite picture of the object, was well understood.

Jurin went on to consider experiments with fine wire and with silk which gave the limit of distinct vision as considerably less than one minute, and indicated that the size of a single "villus" of the optic nerve could be much smaller than the 1/5000 or 1/6000 of an inch mentioned above:[5]

> "163. But admitting the supposition, the diameter of one of these villi will be found to be much smaller.
>
> For, I find, that a bit of silver wire, of the thickness of 1/485 of an inch, laid upon a white paper, is visible at the distance of 10 feet. Hence the angle subtended by the diameter of the wire is about $3''\frac{1}{2}$. Consequently, the thickness of a villus must be 17 times smaller, than in art. 162.
>
> 164. I took a single filament of silk, and laid it close to this bit of wire, then viewing them with a deep magnifying glass, I judged the diameter of the wire to be equal to 4 diameters of the silk. The diameter of the silk must therefore be about 1/1940 part of an inch.

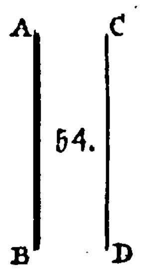
> This silk and wire, being laid upon a white paper, were both visible at the distance of 40 inches from the eye, and the silk appeared plainly less than the wire.
>
> Here the silk subtended an angle of $2''\frac{1}{2}$ only. Consequently, the thickness of a villus is 24 times smaller than that determined in art. 162."

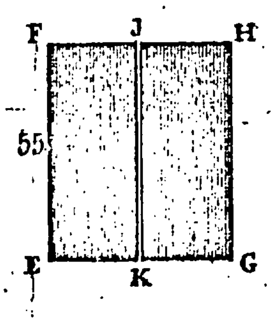
This last experiment was cleverly used by Jurin to answer an argument which was sometimes put forward that the villus need not be as small as the image falling on it:[5]

> "165. It will be objected perhaps, after Des Cartes's way of reasoning, that there is no necessity of supposing the diameter of the villi to be so small as the diameter of the silk. For if the diameter of a villus were 24 times as great as that image, so that the image took up only 1/24 part of each villus, yet the whole of every villus the image fell upon, would be affected by the impression upon that 1/24 part. Consequently, the silk must be equally perceivable, as if its image had taken up the whole of each villus. We answer, if this be the case, then the silk ought to appear equal in breadth with the silver wire, whose image takes up only a sixth part of each villus. But, in fact, the wire appears broader than the silk.
>
> 166. This greater visibility of a line, than of a spot of the same breadth, within the limits of Perfect Vision, seems to arise only from the cause we have here laid down, viz. the greater magnitude or quantity of the impression upon the retina by the line, than by the spot."

So far Jurin had shown that visual acuity not only varied from person to person, but also varied according to the nature of the object seen. Thus a line of a certain width would be visible, while a spot of the same diameter as the width of the line would not been seen; and an intense spot of light would be seen, while a spot of lesser intensity but identical size would not be discerned. The next step he took in his logical exploration of the limits of distinct vision led him towards a modern method of testing the resolving power of an optical system. Jurin noticed that a single black line of a given width could be seen clearly at a distance, while two identical black lines with a white line of equal width between them could not be resolved at the same distance:[6]

> "170. For in Fig. 54, let AB be two black lines drawn upon white paper with a space between them equal in breadth to each of the black lines; and let CD be another black line drawn at some considerable distance from the two former, but equal in breadth and length with either of them. Then, if the paper be set against a wall, and you retire backwards from it, you will find that at some certain distance suited to your eye, even within the limits of Perfect Vision, the white space between the two lines; AB will not be distinguishable, the two lines appearing as one broad line only; but at the same distance the single line CD will be manifestly perceived, and will continue to be so, though you retire to a considerable distance farther backwards."





This can be considered to be a very rudimentary example of the grids of alternate black and white lines of increasing fineness which are used in modern optical testing.

Jurin's explanation of this phenomenon, however, was of considerable interest. He argued by analogy that it was difficult to distinguish fine detail in an object when a person was moving, and from this postulated that if the eye were to fluctuate slightly then this would have the same effect:[7]

> "175. From the same cause of the instability of the eye it must be, cæteris paribus, more difficult to perceive and distinguish the parts of any compound object, when each of those parts subtends a very small angle, than to see a single object of the same magnitude as one of those parts.
>
> For instance, the hour I upon a dial-plate may be seen at such a distance, as the hours II, III, IIII are not to be distinguished at, especially if the observer be in motion, as in a coach, or on horseback, or even in a boat upon the water. This may easily be experienced in looking at a dial where the intervals between the black or gilt strokes are equal to the breadth of those strokes; and much more easily where the intervals are of a less breadth, which is a defect in large dials that are to be seen at a great distance. For in these, the intervals ought to be considerably broader than the strokes.
>
> Likewise, AB, in Fig. 54 is a compound object consisting of three parts, viz. the two black lines and the white line lying between them: But CD is a single object consisting of one black line only upon a white ground: And IK, Fig. 55, is to be considered as a single object consisting of one white line only upon a black ground.
>
> Now in viewing either of these single objects, if the eye be imperceptibly moved, all the effect from that motion will be only, that the object will be painted upon a different part of the retina; but wherever it be painted, there will be but one picture, single and unconfounded with any other.

> But in viewing the compound object AB, if the eye be supposed to fluctuate ever so little, the image of one or other of the black lines will be shifted on to that part of the retina, which was before possessed by the white line; and this must occasion such a dazzle in the eye, that the white line cannot be distinctly perceived and distinguished from the black lines, which by a continued fluctuation will alternately occupy the space of the white line, whence must arise an appearance of one broad dark line without any manifest separation."

The slight fluctuation proposed by Jurin was most significant for it can be said that he had discovered the "saccadic" movements of the eye. These are flicks of the eyeball, of several minutes of arc, occurring at regular intervals of about one second. These movements are necessary to allow the image to fall on a new set of receptors in the retina; it has been shown experimentally that if this did not happen, the image disappears after a few seconds. Therefore, not surprisingly, Jurin's explanation was incorrect; the saccades would not cause dazzle in the eye but prevent the image from vanishing. Nevertheless, his discovery of the movements was an indication of the extremely accurate and painstaking observation which he devoted to this aspect of vision.

Jurin had therefore shown that a single body was more easily seen than the parts of a compound body which have the same magnitude as the single body; and he now attempted to reach some measure of the difference between the visual acuity obtainable for a single and that for a compound body. To do this he used square pieces of white paper on a black background. He discovered that the least angle under which the space between the spots could be seen was at least a quarter greater than the angle under which a single spot could be seen:[7]

> "179. And if two black spots be made upon white paper with a space between them equal in breadth to one of their diameters, that space is not to be distinguished, even within the limits of Perfect Vision, under so small an angle, as a single spot of the same size can be distinguished. To see the two spots distinct, therefore, the breadth of the space between them must subtend an angle of more than a minute. It would be very difficult to make this experiment accurately within the limits of Perfect Vision, because the objects must be extremely small; but by a rude trial with square bits of white paper placed upon a black ground, I judge that the least angle, under which the interval between the two objects can be perceived, is at least a quarter part greater than the least angle under which a single object can be perceived. So that an eye, which cannot perceive a single object under a less angle than one minute, will not perceive the interval between two such objects under a less angle than 75"."

Another author who took Hooke's work as the basis for his own comments was Robert Smith, but the quality of his investigation of the subject was trivial, when compared with that of Jurin. Having summarised Hooke's conclusions on the minimum angle which a body could subtend before becoming invisible, Smith went on to recount an elementary experiment which he carried out:[8]

> "I have been present at making the experiment, when a friend of mine, who had the best eyes of all the company, could scarce perceive a white circle upon a black ground, or a black

> circle upon a white ground, or against the skylight, when it subtended a less angle at the eye than two thirds of a minute; or which is the same thing, when its distance from the eye exceeded 5156 times its own diameter: which agrees well enough with Dr. Hooke's observations. Hence I find, by a rule in the next book, that the diameter of the picture of that circle upon the retina was but the 8000 part of an inch at most: and this may be called a sensible point of the retina. That this point is very small any one may perceive from hence that the breadth of the finest hair is visible at the length of one's arm."

This was the sort of elementary comment which Smith sometimes made in the first volume of his 'Opticks'. However, on occasion he added considerable depth to his discussion in the section in the second volume entitled 'Remarks', and it is from this section that the quality of Smith's background knowledge often becomes apparent. On this particular topic, however, Smith stayed silent in the 'Remarks' section, and one can only feel surprised at his neglect of such a significant subject.

The next major work on vision was published just over twenty years after Smith's 'Opticks' by William Porterfield, and once again we find this major topic of visual acuity strangely neglected. In the following passage[9], Porterfield showed that he had a clear understanding of the working of the retina:

> "The optic nerve is a bundle of very small fibres or threads of a certain determinate bigness. These fibres at one end arise from the brain, and at the other, terminate in the retina, upon the anterior surface of

> which they may be supposed to stand erect, like the pile on velvet; whence it comes to pass that when an object is so small or so far removed from the eye as to form a picture on the retina less than one single fibre, that object will not been seen, because of the weakness of the impression, unless it be very bright and luminous; in which case the whole fibre will be moved by having one part of it powerfully acted upon; and therefore the sensation will be the same as if the object were much bigger."

He did, however, coin a useful term for an object at the limit of vision - "the minimum visible"; and he came to an important conclusion which had been appreciated before, but which had not been expressed as clearly:[10]

> "The magnitude of an object, however small it may be, will be estimated not by the magnitude of the picture but by the magnitude of the nervous fibre."

Thus a small bright body, whose image occupied only part of a nerve fibre, would appear as large as a duller body whose image was as large as the entire fibre. Porterfield said that this seemed to be the reason why stars appeared to be all the same size, and why they appeared to the naked eye to be many thousand times bigger than they really were, even as big as when they were viewed through a long telescope.

In spite of this excellent appreciation of the subject Porterfield was not moved to carry out any significant experiments of his own, and as the majority of those before him had done, he used Hooke's conclusion as the basis for his calculations on the size of individual nerve fibres, although he did maintain that some could see to an angle of $\frac{1}{3}$ minute.

His reasoning was an amalgam of the arguments used by Hooke and De la Hire before him. He based his calculations on a "minimum visible" of 1 minute, and then considered the implications of an increase of acuity to $\frac{1}{3}$ minute:[11]

> "1 minute is a 60th part of a degree or 21,600 part of a circle and if the eye is 1 inch in diameter or 3 inches in circumference, the diameter of a nervous fibre would be the 21,600th part of 3" or the 7,200th part of 1 inch, which is a 600th part of a line. If 10 hairs' breadths make a line, the diameter of the nerve fibre would not exceed the 60th part of the diameter of a hair. Thus their cross sectional area will be no bigger than 1/3600 of the area of a hair. Taking $\frac{1}{3}$ of a minute as the "minimum visible" the area of a nerve fibre would be 1/32,400 of the area of a hair."

As De la Hire before him, Porterfield was amazed at the delicacy of the nerve fibres, since he supposed each of the fibres to be a hollow canal in which "spirits" flowed to the brain, which he took as the true seat of vision.

Unfortunately, the subject of visual acuity was not carried significantly forward in the next major work on vision to be published, Joseph Priestley's (1733 - 1804) "The History and the Present State of Discoveries Relating to Vision and Light and Colours".[12] The importance of the topic was clearly acknowledged, since more space was allocated to it than in any publication previously mentioned, except in Jurin's "Essay". However, Priestley added no new conclusions of his own and merely gave extensive summaries of the ideas of earlier workers, particularly emphasising the work of Jurin. In view of the nature of the book, however, it would perhaps be unfair to expect Priestley to have included new experimental data in an account which was essentially an historical review of the subject of light.

Some years later Thomas Young(1773-1829) took this topic considerably further, within the context of an interesting comparison of the way in which the eye and ear functioned.[13] Young felt that the eye and the ear were unique among our organs of sense, since with the organs of taste, smell and feeling the perceived objects come into immediate contact with the actual nerve endings:[14]

> "But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound, to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propogation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive hard bodies, obeying the motions of the air nearly in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes."

Young's description of the action of the three bones, the hammer, anvil and stirrup, in the ear was excellent for its accurate brevity. It is also interesting to note that the ear was not as well understood at this time as was the eye, since Young also said: "It cannot indeed be denied, that we are capable of explaining the use and operation of its (the eye's) different parts, in a far more satisfactory and interesting manner than those of the ear."

Young then went on to consider the directional capabilities of the ear and eye, taking as a rough measure the fact that the eye could distinguish between two points one minute apart. He concluded that the retina contained not more than 10 million, or less than 1 million, sentient points, and the optic nerve consisted of several millions of distinct fibres. Gone, apparently, was the idea that the optic nerve consisted of minute tubes containing "spirits":[15]

> "Supposing the eye capable of conveying a distinct idea of two points subtending an angle of a minute, which is, perhaps, nearly the smallest interval at which two objects can be distinguished, although a line, subtending only one tenth of a minute in breadth, may sometimes be perceived as a single object; there must, on this supposition, be about 360 thousand sentient points, for a field of view of 10 degrees in diameter, and above 60 millions for a field of 140 degrees. But, on account of the various sensibility of the retina, to be explained hereafter, it is not necessary to suppose that there are more than 10 million sentient points, nor can there easily be less than one million: the optic nerve may, therefore, be judged to consist of several millions of distinct fibres. By a rough experiment

> I find that I can distinguish two similar sounds proceeding from points which subtend an angle of about five degrees. But the eye can discriminate, in a space subtending every way five degrees, about 90 thousand different points. Of such spaces, there are more than a thousand in a hemisphere: so that the ear can convey an impression of about a thousand different directions."

The directional properties of the ear, however, rely on the combined response of both ears (as used in the electronic reproduction of stereo records and radio) and Young appeared to be unaware of this. However, his estimate that the retina contained between one and ten million receptors, the range being due to the falling-off of the sensibility of the retina away from the fovea, was remarkably percipient.

The modern view of the acuity of the eye is that it is very close to the limits set by the spacing of the receptors; it is also close to the limits placed by the optical system of the eye. Away from the fovea, acuity is much worse and, strangely, it is worse than either the optics or the fineness of the pattern of rods and cones would lead one to expect. The reason probably lies in the discrepancy between the number of receptors which is about 100×10^6 and the number of nerve fibres 1×10^6. Thus Young's estimate of no more than 10×10^6 and probably less than 1×10^6 sentient points was remarkably accurate. It is also of interest to note that the historical idea of the nerves being hollow tubes carrying fluid to the brain has a modern counterpart in the rods and cones in the retina which contain photochemical substances, for example, the rhodopsin found in the retina which absorbs the light energy and allows it to be converted into electrical energy in the receptors, which in turn set up the nervous impulses to the brain.

VISUAL ACUITY REFERENCES.

1. R. Hooke. Posthumous Works, 1705, 97.
2. Ibid., 12.
3. Mém. Acad. Sci., 1730, 9, 566.
4. R. Smith. Opticks, 1738, Vol. ii which also contains:

   J. Jurin. An Essay upon Distinct and Indistinct Vision, 148.
5. Ibid, 149.
6. Ibid, 150.
7. Ibid, 151.
8. Ibid, vol. I, 31.
9. W. Porterfield. A Treatise on the Eye, 1759, I 384.
10. Ibid, I 385.
11. Ibid, II, 62.
12. J. Priestley, The History and the Present State of Discoveries Relating to Vision and Light and Colours, I, 1772, 673-688.
13. Thomas Young, Natural Philosophy, II, 1807, 573-600.
14. Ibid., 574.
15. Ibid., 575.

ACCOMMODATION

It has already been mentioned in the section which dealt with vision before 1650, that Kepler was probably the first to recognise the need for the eye to be able to accommodate. He came to this conclusion at the beginning of the seventeenth century, and within the next half century many of the possible methods of achieving accommodation had been postulated and discussed.

Kepler thought that the eyeball itself was elongated when it viewed close objects, and that the compression of the eyeball necessary to make this happen was achieved by the ciliary processes. Scheiner was the next to make a discovery which was significant in this field. He noticed the closing of the iris and its minute forward displacement which occurred when a close object was viewed. He agreed with Kepler that the eyeball was elongated to view close objects, but thought that the ciliary processes might consequently also produce a change in shape of the crystalline lens. It is Scheiner, therefore, who has the distinction of being the discoverer of the actual change within the eye which is responsible for accommodation, although it cannot be claimed that he had, therefore, satisfactorily explained the mechanism of accommodation. Descartes' work on the eye in general was of even greater significance than that of Scheiner. In fact, he made no new discoveries, but he did place greater emphasis than Scheiner on the means by which accommodation took place. He acknowledged the part played by the contraction of the pupil in accommodation, but stated clearly for the first time that, in addition to an elongation of the eyeball, the crystalline lens changed shape in order to view objects at different distances.

Thus, the fact that the eye, and particularly the lens of the eye, suffered some change when it viewed objects at different distances was widely and even generally accepted very early in the study of physiological optics. That it was the supports of the crystalline lens that somehow caused this to happen also had considerable favour, either by changing the shape of the eye, or the shape of the crystalline, or both. There seems to have been little experimental evidence in favour of these hypotheses available at the time, apart from the indirect evidence that the crystalline was elastic and could, if necessary, change shape, and also the fact, recognised by Descartes in 'La Dioptrique' 1637, that we could focus on only one object at a given distance at a time[1]: "For, if while looking intently at a distant tower or mountain a book is placed at a short distance before the eyes, none of the letters will be seen except indistinctly until the configuration of the parts alters slightly." It is perhaps strange that the much stronger external muscles attached to the sclerotic coat, which could, if acting in opposition, be imagined to squeeze and elongate the eye, were not seriously considered to be responsible for the act of accommodation, their correct function of determining the direction of the eye being generally accepted.

It was left to De la Hire to investigate the major experimental observation so far noted during the act of accommodation, namely the closing of the pupil when viewing a close object. In 1685 De la Hire published a paper entitled 'Dissertation sur la conformation de l'Œil' in which he put forward a hypothesis that the contraction of the pupil was the sole means by which accommodation was carried out; he denied that any other changes in the eye took place and supported his contention by an ingenious series of experiments.[2]

De la Hire supposed that the accommodation of the eye for objects at different distances was achieved solely by the opening and closing of the iris. He maintained that we would (instinctively) know if the eye changes its conformation in order to see close and distant objects. His introduction to his experimental work was as follows:-

> "If one were able to measure exactly the strength or weakness of an eye, at different ages or at different times and when looking at a close object or one which is further off, there is no doubt that one would be able to know if there was a change in conformation in order to see objects at different distances, since the strength or weakness of the eye depends absolutely on the general form of the humours, and of that of the crystalline in particular, as many have supposed.
>
> Without stopping here to inquire whether, if it is possible that the eye can compress itself by means of the muscles which surround it, or in what manner the crystalline can be flattened and regain its natural figure which must be of a certain convexity. I will show in the first part of this dissertation how we can know the strength or weakness of an eye with a very great accuracy in order to make a comparison with the same eye at different times and in different situations; and I will see later by a very certain experiment that the eye does not change its conformation at all in order to see close or distant objects."

De la Hire's experiment to show that no change in conformation of the eye was necessary was based on a simple optometer which was made from card which had several small holes pierced through it close together within a space less than that of the pupil.

Through this he viewed a luminous object such as a candle. He said that if the candle was at a medium distance, such as three feet, then "Presbitae" (people suffering from long sight) and "Miopes" (people suffering from short sight) would see as many objects as there were holes. Those who had good sight would see only one object. De la Hire explained this phenomenon by stating that if the eye was too flat, that is, it had the rays of light from the candle coming to a point behind the retina, then when one looked at the candle through two holes in a card within the area of the pupil, it would be seen double. This happened because the pencils of rays would have separate paths through each of the holes and these would not meet until they were behind the retina; the pencils would cut the retina in two places and two objects would be seen, though with less brightness. He said that the apparent distance apart of the two images seen through the holes would be proportional to the distance apart of the holes, and they would also appear further apart for an eye which was flatter, since this type of eye would bend the rays less, and they would thus strike the retina at two points which were further apart.

If an eye was too convex, De la Hire said that the two pencils of light from the holes in the card would intersect within the eye and each one would strike the retina behind this point. Thus the retina would again see two images. He said again that the two images would appear further apart if the holes were further apart, and also if the eye was more convex. In this small point he is incorrect in that, since the rays cross before striking the retina, the images will appear further apart if the holes are <u>closer</u>, and closer if the holes are further apart.

This method was then put forward as a method of determining whether an eye was slightly long- or short-sighted. Presumably he assumed that it was easy to diagnose the defect in extreme cases. His method was to view an object through the two holes, and in the case of long-sighted eyes, which he called "too flat", he chose a convex lens of the correct strength, so that when it was held just in front of the card, the two images appeared as one. He said that by this method it was possible to evaluate the amount of the defect of the eye, and also by comparing readings taken at different times, to see how an eye changed its strength with time, or the effect that an illness would have on it. An identical method using concave lenses was mentioned for short sight.

De la Hire commented that this experiment could be used to prescribe the type of convex or concave lens that would be needed to correct a given person's sight, or even to persuade someone who thought that they had good vision, that they in fact needed to wear glasses.

He then continued in the light of his experimental observations to investigate whether the globe of the eye itself, or the crystalline lens, changed its shape in order to see objects at different distances. In order to do this he assumed that there was some change, either of the crystalline or shape of the eye, and then showed that this assumption was incompatible with his experimental results.[3]

> "Now, let us see if it is possible that the globe of the eye, or the crystalline, changes its conformation in order to see objects at different distances, and let us suppose, for example, that

an eye can change its form to the extent that it is necessary, in order to see with the same clarity an object at a distance of one foot, and another at a distance of six feet. Let us suppose further that this eye, by its nature, or by the help of a glass, can see an object distinctly at the distance of one foot; it follows from the supposition that we have just made that it will be able to see another with the same clarity at six feet; that is to say that this eye having been disposed to receive on the retina the point of the pencil of rays from an object which is only at a distance of one foot, can then change its form in such a way that it can also receive on the retina the point of the pencil of rays from an object which is at a distance of six feet. It is thus clear from what we have shown above that if one puts in front of the eye a card pierced with two holes, it will only see a single object at a distance of one foot, if it is disposed to see an object clearly at a distance of one foot and, in the same way, if it were disposed to see a different object at six feet distance, it would see single, like that which is only at a distance of one foot. But as one cannot say that the eye changes its conformation in an instant and since it judges very well the distance of objects through a small opening (which is the only thing which would cause it to change its conformation when it is engaged upon the examination of an object at a distance of one foot), if one quickly puts in front of the eye a card pierced with two holes, through which it can see the same object, it will see single; and if one does the same thing for the object at a distance of six feet, it must appear also single, following this hypothesis.

However, it is very certain by experiment, that if the eye with such a disposition as one is able to give it, sees the object single at a distance of one foot through the holes in a card, it will assuredly see it double at six feet; or, on the contrary, if it sees it single at six feet distance, it will see it double at one foot, whatever effort that can be made in order to change its first conformation.

That of which I say of six feet and of one foot

> distance must likewise apply to other distances which are lesser or greater, and this is why one can assuredly conclude that the eye does not change its conformation in order to see objects at different distances, since however small the change, one would notice it in this experiment, and there is no one who, thinking himself to have good sight, does not feel convinced that he sees an object equally distinctly at one foot or two feet distance as at five feet or six feet."

The thread of his reasoning in the above passage is clear. His experimental basis stemmed from his definition of clear vision; he maintained that in order to be sure that we were seeing an object clearly, it had to appear single when viewed through a card containing two small holes close together. Later in this paper he maintained that even objects which appear double through the card seem clear enough when looked at normally. However, here, he was trying to establish an experimental basis for clear vision. He maintained that a given eye, seeing an object single at one foot through two holes, inevitably saw any object at six feet, double. Again if it saw an object single at six feet, then at one foot it would appear double. He said that the eye could not make one and then the other object appear single when viewed through the holes. The only way this could be done was by means of some auxiliary lens. This being so, he deduced the eye could not change its conformation to enable it to see clearly objects at more than one distance.

However, his argument was obscured by being couched in difficult language and he also confused his line of thought by using arguments based on his experimental results with holes in cards, and then changing to arguments based on pure instinct - "But as one cannot say that the eye changes its conformation in an instant..." In fact, De la Hire's original and interesting experiments with holes in cards are largely rendered invalid, since he is dealing with the act of accommodation, a process which, though it can be controlled, is largely instinctive, and thus takes place normally without an act of will, and without our knowledge. Thus, his original statement, one of the foundations of his reasoning - "when looking at a close object, or one which is further off, there is no doubt that one would be able to know if there was a change in conformation in order to see objects at different distances" - is incorrect,which in turn, vitiates his reasoning from his experimental results. It even undermines the results themselves, since one cannot avoid changing the focus of the eye when cards are placed between the eye and the viewed object.

In part II of this Mémoire, De la Hire turned his attention to an interpretation of what was meant by clear vision. . He did this again in order to refute the opinion he said was common that the eye must change its conformation in order to see objects at different distances:[4]

> "After what has been shown in the first part, it appears that it will not be necessary to refute the commonly held opinion that the eye must change its conformation in order to see objects at different distances, which is mainly founded only on the belief that in order to see an object well, it must be necessary that the point of the pencil of rays falls exactly on the retina. In the meanwhile, in order to leave no doubt about that which I have put forward, I will examine in order the reasons which one employs in order to support the necessity of this change of conformation.
>
> It is said first that it is not possible to see an object distinctly if the point of the pencil of its rays does not meet exactly on the retina. I grant that vision is the more distinct as the

> point of the pencil falls more exactly on the retina, but I reply that one does not cease to see an object distinctly, though this point is a little displaced from it (the retina). I say further that it is impossible to perceive this error without availing oneself of the method that I have proposed before; for it is not to be supposed that the rays which would come, for example, from a point which might be only the thousandth part of a line, after having passed through the eye, could reunite in a point which would also be only a thousandth part of a line, seeing that the rays after refraction intersect at different points, although we suppose them to come to a geometric point; that is why they form a focus which is not determined by a point, but which has always a slight breadth - that is to say that the focus is equally sharp a little further away or a little closer to, as a trial with a telescope will show, since one can shorten or lengthen them a trifle without the object appearing less distinct."

Thus, De la Hire maintained that we could see clearly under every-day conditions, even when the rays of light from an object did not meet at a geometrical point on the retina. Moreoever, his opinion was that unless we availed ourselves of an experiment such as the one with holes in the card, we would not even be aware of any lack of clarity in viewing objects which were not geometrically in focus.

He had also carried out experiments with a small convex lens, with the same focal length as the diameter of the average eye, and an opening corresponding in size to that of the iris, to satisfy himself that objects from 50 pouces (53. 3") to infinity were all pretty well in focus.

Continuing his experiments with convex lenses, De la Hire closed the aperture of the lens, so that light was allowed through only a small portion. He came to the conclusion that images of close objects were not as clear as those from objects much further off, when projected on a screen at the focus of the lens. Nevertheless, he was still led to draw the conclusion that the normal eye, with a small pupil, could see close and distant objects reasonably well, without any act of accommodation.[5]

> "One is in no doubt that when one looks through a small hole, the point of the pencil (of rays) from a close object is not sensibly as distinct as for one which is further off, as one can see by putting a white paper at the focus of a convex lens to receive there the image of any object, there being only a small portion of the lens uncovered. From which it comes that those who have a very small iris, and who have an eye of average roundness, can see close objects, as those at 8 pouces distance, easily and reasonably distinctly, without it being necessary for the eye or crystalline to change its shape."

De la Hire passed to a consideration of the opening and closing of the iris. He pointed out that its main purpose was to limit the amount of light entering the eye, but that the fact that it contracted when we viewed close objects would enable us to see them more distinctly. However, it does seem that he thought of this contraction not as a separate act, but merely as an extension of the contraction for bright light. He indicated in the quotation below[6] that the pupil contracted because light from a close object was more vivid than from a distant one. It will be noted again that he mentioned the lack of clarity of images of close objects, a comment that would seem to be somewhat at variance with his theory that close and distant objects were seen with reasonable clarity without the necessity for accommodation:

> "Light from close objects will be much more vivid than that from an object far off, we must close up the opening of the iris, and thus although these objects send out rays into the eye, the pencils of which cut the back of the eye near their point, this section becomes so small that the image is not allowed to be very distinct."

One of the reasons put forward in support of the existence of an act of accommodation was our inability to see simultaneously and clearly with one eye a close and a distant object which are almost in a straight line. In answer to this, De la Hire commented that as well as our inability to see objects situated as above, we were also not able to see clearly, at the same time, objects not in the same straight line from the eye, but at the same distance. He maintained that in order to see clearly, the images would have to fall on the point of the retina where the axis of the eye cuts it; and thus in order to see the second object, the eye must change direction, so that its image should fall on this spot on the retina. He said that "one must not be surprised if one finds it a little more difficult to change attention from a close to a distant object, than in order to see another at the same distance, since the different light of these objects affects the eye differently." This change in colour of distant objects and the small change in the direction of the axes of the eyes were put forward as sufficient means of judging distances. Thus, he maintained that we did not need the power of changing the conformation of the eye in order to judge distances.

De la Hire returned to this topic in his wider-ranging paper "Dissertation sur les Differens Accidens de la Vue"[7]. In this paper his experimental work in support of his theory was again largely based on his hole-in-card experiments, and followed very closely that outlined above. However, he did dwell more thoroughly on the possible ways in which accommodation could take place, and he did question the experimental work based upon models of the eye, which was put forward in favour of accommodation. The fact that in models of the eye it was necessary to change the position of the retina, or lens, in order to produce clear images of objects at different distances, in his opinion had led scientists to the view that some similar change was necessary in the eye, and this led him to query the validity of such experiments.

The main objection he had to the existence of an act of accommodation (apart from the closing of the iris for close objects) was the difficulty he found in reconciling the type of change which would be necessary in the eye, with his knowledge of the structure of the eye. This had led him to support his prejudice with his interesting experimental work with holes in cards. He expressed his own prejudice most succintly as follows:[8]

> "Those who know the structure of the eye and the nature of all the parts which compose it, will have difficulty in persuading themselves that the changes can happen to it which one has been obliged to suppose in order to justify the way in which vision makes use of them."

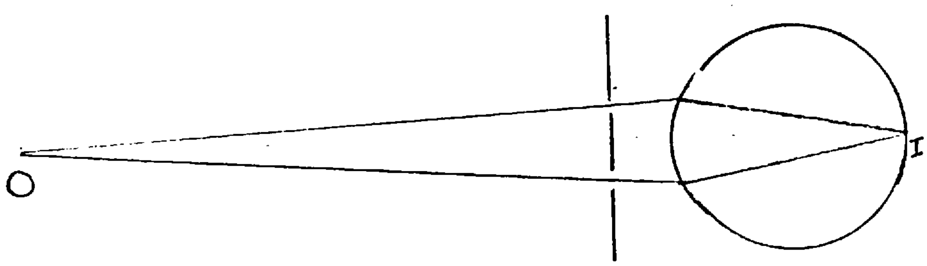
He listed the type of changes which would have to take place if accommodation did occur. The eye must stretch for close objects and flatten to see distant objects. This would require the cornea to become more or less convex. This he felt could best be achieved by the muscles attached to the sclerotic (the exterior muscles responsible for changing the direction of the eye) and he thought that the muscles were not strong enough to do this. He thought that a more plausible explanation would be the change of shape of the crystalline lens, since it was held by the ciliary ligaments, which were muscular.

The fibres of the ligaments travelled towards the crystalline and would make it flatter when the muscle swelled up, since it held the lens equally around the whole circumference. However, he said the best anatomists held an opinion contrary to his, and maintained that the ciliary ligaments were not muscular. In any case, he thought that the crystalline was very solid, being made up of layers, and could not change its shape easily.

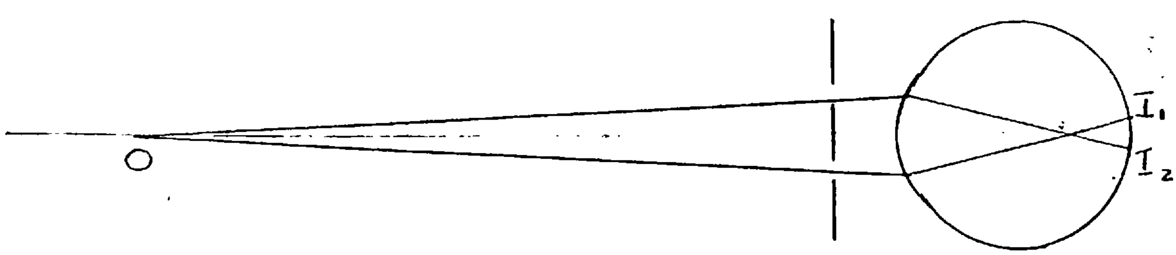
In conclusion, De la Hire again dwelt upon the inability of eyes to focus simultaneously on two objects at different distances. He marshalled all the arguments mentioned in his earlier paper, but in addition introduced the action of the iris, in viewing close objects, as a reason for this defect. He said that when an object was far off, only a few rays strike the retina; therefore the iris, which was a muscle, made every effort to have as big an opening as possible. The closing of the iris for close objects served to cut down the range of rays entering the eye, allowing through only those which would be sharply focused on the retina. Thus he maintained that the effect reported to be due to the different conformations of the eye, was, in fact, due to the different openings of the iris; this enabled the retina to see close objects with the same "force" as those which were far off.

De la Hire"s hypotheses and the reasoning with which he supported them are difficult to follow, and since he was advocating a theory which is incorrect, it is tempting to diminish the importance of his work. Therefore, before attempting to evaluate his work, I propose to endeavour to draw all the threads of the work mentioned in previous pages together in a detailed summary of his experiments and the conclusions drawn from them.

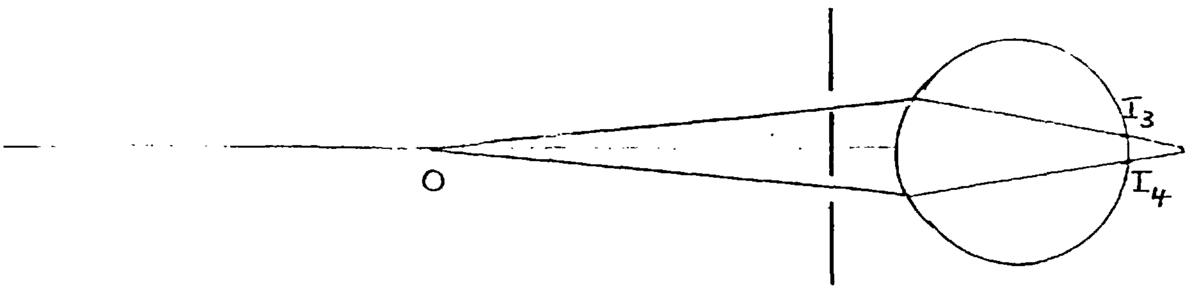
De la Hire held that if rays from an object united on the retina, then we saw that object "distinctly". He used his experiment with two holes in a card to demonstrate this. Using a spot "O" as an object, if we looked through two holes at the spot, and the rays united on the retina, then one spot was seen.



De la Hire 's experiments led him to maintain that, for a given person, using this experiment, a single spot was seen only at one given distance. If one looked at the spot when it was further off, then two spots were seen - I_1 and I_2



If one looked at the spot when it was closer, then two spots were seen - I_3 and I_4



He maintained it was impossible to unite these two images by any act of accommodation of the eye. Colleagues and I have performed the experiment and have verified completely De la Hire's findings.

Thus, when looking through two holes in a card, it is possible to make the two pencils of rays from an object unite on the retina for only a single distance. However, De la Hire maintained that under normal circumstances we saw things clearly over a whole range of distances and this, of course, is common knowledge. Thus, we could see the spot clearly at ten feet, six feet, or one foot. Therefore, since he had shown by his experiment that the rays of light united on the retina for only one distance, then he maintained it could not be necessary for the rays to unite for us to see an object clearly. He agreed that the image on the retina would be blurred for all but the ideal distance of distinct vision, but maintained that this blurring could not be significant, since we saw clearly over a whole range of distances. (It should perhaps be mentioned that De la Hire was considering objects which could be seen clearly over a large range of distances, such as spots of light, etc; it would not be possible to maintain that we could, for example, read a printed page at anything but comparatively close distances.)

Therefore, since it was well known that we could see clearly at different distances, and since he said his experiments showed that we could not make pencils of rays unite on the retina by an act of accommodation, De la Hire maintained that no act of accommodation(apart from the narrowing of the pupil for close objects) took place.

The obvious weakness in his reasoning was clearly pointed out by Smith in his "Opticks" and whose comments on De la Hire's work will be reviewed later (P. 141). Smith stated that it was not possible to maintain the focus of the eye on the original spot during the time that the card with two holes was placed in front of the eye. During this instant of time we must assume that the eye reverts to its natural viewing distance, and it would not be possible to re-focus on the spot through the two holes. If one attempts this experiment, it is easy to come to the conclusion that this is the correct explanation, and the inability to reduce the two images to one could well be because of the presence of the card, with the blurred image of the two holes dominating the field of view.

De la Hire's work on this subject was of considerable distinction, in spite of the fact that it supported a theory which was not accepted at the time, and has since been shown to be incorrect. He introduced into an area of study, which had previously relied almost solely upon theory (which was not far removed from conjecture), an element of experimental work which showed considerable ingenuity, although Scheiner was probably the originator of the experiment where objects are viewed through holes in cards. To what extent he assisted the correct cause of accommodation to be found is difficult to assess. Since his theory was not accepted, he probably did not cause a delay, and by stimulating interest in the subject, and by causing others to repeat his experiments, he may well have promoted the discovery of the correct cause of accommodation; and in the distinction he made between clear and distinct vision he had a distinguished disciple, as we shall see when we come to study the work of Jurin published many years later.

He closed his work with a simple summary of his theory:-[9]

> "Thus the latitude that one sees in all sorts of eyes comes only from the differing openings of the iris and not from the different conformations of the globe of the eye or the crystalline, as one has believed until the present."

De la Hire's hypothesis was so much at variance with current opinion on the subject of accommodation that it is not surprising that it gained little acceptance. The common opinion that some form of accommodation took place was based on little, if any, experimental evidence, but there was available to every interested scientist the evidence available using his own eyes. It was probably this, and perhaps most of all the fact that only one distance at a time could be seen clearly that persuaded them against De la Hire's opinion, since it was here that his reasoning was perhaps weakest.

The immediate opposition to his theory was published in 1685.[10] The author, who was anonymous, was unconvinced by De la Hire's experiments, being of the opinion that he had carried out insufficient experiments to establish that the eye lacked accommodating powers apart from the contraction of the iris. A few years later, Keill[11] merely stated an opinion that the ciliary ligaments pressed the crystalline nearer to the retina when it viewed objects at a distance from the eye, and made no reference to De la Hire's work.

In 1719 Henry Pemberton published a much more significant dissertation on the means by which the eye might accommodate.[11a] From the title of the work it is probable that Pemberton can be given the distinction of coining the word 'accommodate' to describe the focussing action of the eye, since I have not found it used in this way by any earlier writer. Pemberton said that there were two main opinions on the way accommodation was achieved. The most common opinion was that the crystalline humour was responsible for the ability of the eye to focus at different distances. Another opinion, however, was that the eye was compressed more or less into its orbit, by a muscular action which changed the length of its axis:[11b] "so that the retina is now nearer to, now farther from, the anterior humours of the eye." Pemberton favoured the former opinion, since pressing the eye caused confused, not clearer, vision, no matter at what distance the object was set. He thought that the muscles could not exert a perfectly uniform action on all parts of a body as soft as the retina; therefore they would cause the same kind of distortion as the finger pressing against the eye. In any case, he thought that the sclerotic was far too stiff to allow the change of shape of the eye to be caused by the weak muscles of the eye. He also made the point that, after a cataract operation, the natural accommodation of the eye was lost, a fact which clearly linked accommodating power to the crystalline lens.

Pemberton then went on to state that there were two ways in which the crystalline could cause a change of focus of the eye. The first was that the crystalline moved; the second was that it changed shape. In order to assist his choice between these two theories, he attempted an experiment. Black lines $1\frac{1}{4}$in. wide were drawn at $\frac{1}{4}$in. intervals on white paper. He fixed a bi-convex spherical lens 30in. from the paper and put his eye close to the lens. As he drew his head back, the blurred image became more confused, until near the focus of the lens, the distinction between the black and white spaces was lost. This was a very inconclusive experiment, but it must be assumed that Pemberton used it to show that a change in shape of the lens, rather than its movement, was the more likely cause of accommodation.

Pemberton then turned to a lengthy geometrical discourse to show that the crystalline lens possessed a variable shape, and that its change in shape would not affect the shape of the retina. Having satisfied himself that the lens could change shape, he then disagreed with the commonly held opinion on the cause of the change in shape. Rather than support the usual view that the change in shape of the lens was caused by the suspensory ligaments and ciliary processes, which he thought were too weak to change the shape of a body as stiff as the lens, he put forward the new but not quite incredible conjecture (as he put it), that the lens itself was muscular, somewhat like the tongue, and containing two opposed muscles capable of altering the shape of the surfaces. He said that the fibrous nature of the lens had long ago been found by Antonie van Leeuwenhoek (1632-1723), a microscopist, and that its transparency did not mean that the fibres could not be muscular, since not all muscle fibres were red. Pemberton thought that the fibres of the lens, as described by Leeuwenhoek, would be capable of flattening the lens on one side, while making it more convex on the other. Unfortunately he seemed unaware that, in order to shorten the focal length of the lens so that the eye could focus on close objects, it was probable that both surfaces of the lens would require to have their convexity increased. The idea of lens muscularity was, however, not new. In 1684[11c] Leeuwenhoek had put forward the idea that the capsule that held the lens was muscular, and then in 1704[11d] he called the crystalline humour the 'cyst. muscle'. In fairness to Pemberton it should be added that Leeuwenhoek mentioned these ideas briefly, and made no attempt to relate the ideas of lens or capsule muscularity to the problem of accommodation. The idea of a muscular crystalline lens was later also put forward by Thomas Young, who, while mentioning Leeuwenhoek's work, made no reference to Pemberton's theory.

At about the same time as the publication of Pemberton's dissertation another book emerged, also from Leyden. This was "Mathematical Elements of Natural Philosophy" by William-James's Gravesande (1688-1742), Professor of Mathematics and Astronomy at Leyden, translated into English by Desaguliers in 1721.[11e] The book was subtitled "An Introduction to Sir Isaac Newton's Philosophy" and was clearly intended as a text book covering the major aspects of Natural Philosophy. In it Gravesande devoted a chapter to the subject of vision. His ideas on the subject of accommodation were sensible and clearly stated. He had no doubts that the ciliary ligaments were muscular.[11f]

> "This crystalline Humour is sustain'd by small Fibres or Threads, which are fix'd to all the Points of its Circumference, and likewise to the Inside of the Eye: They are inflected in the Form of an Arc, and every one of them is a Muscle; they are call'd the Ligamenta Ciliaria, ..."

Gravesande was the first to postulate the idea that the ciliary ligaments were curved, and the idea played an important part in his explanation of how accommodation was achieved:[11g]

> "But when, according to the different Distance of the radiant Point, its Focus is brought

nearer, or removed farther off, it is necessary that there should be a Change in the Eye, lest the Place in which the Picture is exact, should fall short of, or beyond the Retina, and so the Vision should be confused.

But it is very difficult to determine what this Change is, and Philosophers are divided in their Opinions about it: I shall only observe in general, that it is not very probable that the Figure of the whole Eye is changed, in order to put back or bring forward the Retina; and therefore we must expect to find this Change within the Eye.

For if the Figure of the Eye was changed, as this Change must be equally necessary in all Animals, the Eyes of all Animals would undergo the same Changes; for the same natural Effects cannot have different Causes. Now in the Whale the Sclerotica is too hard to be subject to any Alteration of Figure. Besides, if there was such a Change in the whole Eye, it would arise from the external Pressure of the Muscles, which would be different in different Positions of the Eye, and only regular in one Situation of it.

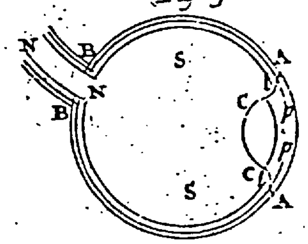
If now we examine the Eye within, it will appear necessary that there should be a Change in the Crystalline; which by changing its Place or Figure in the Eye, will produce the desired Effect; for the Rays that fall upon the Retina before they are united, will be made to unite just upon the Retina, if the Crystalline becomes more convex, or if (its Figure remaining the same) it be brought forwards towards the Cornea.

That the Position of the Crystalline Humour is easily changed, and that it is brought nearer to, or farther from the Retina, its Axis remaining the same, is plain, because the ciliary Ligaments are muscular: When these Muscles are swell'd, and become shorter, the Hollow which their Inflection makes at Cl, Cl, becomes less; by which means the vitreous Humour is compress'd, and therefore it presses upon the Crystalline, and pushes it forwards farther from the Retina; which is necessary when we look at near Objects.

From an Experiment that we shall hereafter mention, it has been demonstrated, that there is another Change in the Eye that acts contrary to this; and we shall shew what is the Occasion of it. The second Change is also to be referr'd to the Crystalline; which (when it is drawn by the ciliary Ligaments, to make it recede from the Bottom of the Eye) becomes also flatter, and therefore it must recede farther than if its Figure was unchangeable; that is, the Change becomes more sensible; which we shall shew to be of Use."

This is a very clear statement of Gravesande's opinion that accommodation was caused by a change in the position of the lens. It is perhaps strange that he adopted a rather complicated method of moving the crystalline forward (the ciliary ligaments became less curved (inflected) and that this caused the vitreous humour to push the lens forward), when as can be seen from his own diagram (fig. 3 below) it would have been possible to imagine the ciliary ligaments pulling the lens forward by direct action. Gravesande was again unusual among scientists advocating the movement of the lens to produce accommodation, in that he acknowledged that the pull of the ciliary ligaments would tend to flatten the lens, and thus make it necessary for the lens to move further than would have otherwise been necessary to achieve a given amount of accommodation.

Fig. 3



Some years later, François Pourfour Petit[12] (1664-1741), a Physician, put forward his own theory of accommodation. His work, as befitted a doctor, was largely physiological, and was based on his extensive observations and dissections of human and animal eyes. He did not hold to the view that the ciliary ligaments changed the shape of the lens, stating that he thought them too weak. His opinion was very tentative: that the oblique attachment of the fibres of the ciliary ligaments to the front and back of the capsule which held the crystalline made it possible for them to move the crystalline forward. He did not permit himself to elaborate on this observation.

However, the greatest contribution to the subject of physiological optics in the first half of the eighteenth century came from two authors whose works were published between the same covers. Robert Smith's 'Opticks' published in 1738 should obviously be counted as the major work of the two, but in the field of vision James Jurin's 'Essay on Distinct and Indistinct Vision' can easily rank as its equal. 'Opticks' was published as two volumes, and Jurin's essay was appended to the end of the second volume, which happily ensured that it had the wide readership that it undoubtedly deserved. Since Jurin's approach to the problem of distinct vision followed ideas first put forward by De la Hire, it is more logical to consider his work before that of Smith. It is probable, however, that Smith's work was the earlier since Jurin does on occasion use Smith's calculations to support hypotheses of his own.

Jurin[13] did mention De la Hire's work and also in his introduction, followed a train of reasoning on what was meant by "distinct vision" which was similar to that which De la Hire had put forward in his earlier papers. However, he still maintained that some form of accommodation was necessary and did take place. Jurin started his essay with a clear statement of his understanding of distinct vision, and followed this with a simple experiment which enabled him to distinguish between distinct and what he called "perfect" vision.[14]

> "An object is said to be seen distinctly, when its outlines appear clear and well defined, and the several parts of it, not too small, are plainly distinguishable, so as that we can easily compare them one with another, in respect to their figure, size and colour. For instance, the words of this book are distinctly seen when the letters appear well defined, and their shape and the intervals between them are plainly perceived and distinguished, so as that the book may be read with ease. A single letter also is distinctly seen, when the several parts of the letter, the connexion of those parts, and the intervals between them are clearly perceived and distinguished. In order to such distinct Vision, it has hitherto been commonly thought, that all the rays of a pencil flowing from a physical point of an object, must be exactly united in a physical, or at least in a sensible point of the Retina.
>
> But that such an exact union of the rays is not necessary to distinct vision will manifestly appear upon making the following trials.

> Take a title page of a book, in which there is a print of three or four different sizes; and first, place the book at such a distance, as that every sort of print may without any straining of the eye, appear perfectly distinct. In this case it may reasonably be presumed, the rays of every pencil flowing from the letters are accurately collected into so many several physical, or at least sensible points upon the Retina.
>
> Afterwards bring the book by degrees so near, as that the letters of the smallest print may now begin to appear a little confused, and cannot by any endeavour or straining of the eyes be rendered so distinct as they were before. Then, keeping the book at that same distance, look at a print somewhat larger than the former, and that larger print shall seem perfectly distinct without any the least appearance of confusion.
>
> Here, it is manifest from the less distinct appearance of the smaller print, that at this distance the rays of each pencil are not accurately united in a sensible point of the Retina, notwithstanding which the larger print appears distinct.
>
> If the book be brought still nearer, the smallest print will now be quite confused, and the larger print will begin to appear indistinct. But keeping the book at this same nearer distance, a print still larger will appear distinct."

. Thus Jurin's view was similar to De la Hire's, that we could still see distinctly even though the pencils of light did not necessarily meet at a point on the retina; though his argument has greater authority, since it is supported by the simple but persuasive experiment outlined above.[15]

> "Distinct Vision may therefore not unfitly be divided into the two following sorts or

> species: namely Vision perfectly distinct, or Perfect Vision; and Vision imperfectly distinct, which I shall usually call, simply, by the name of Distinct Vision.
>
> Vision perfectly distinct, or Perfect Vision, is that in which the rays of a single pencil are collected into a single physical, or sensible point of the Retina.
>
> Vision imperfectly distinct, or simply Distinct Vision, is that in which the rays of each pencil are not collected into a sensible point, but occupy some larger space upon the Retina, yet so as that the object is distinctly perceived, as the larger point."

He then went on to define further, by means of diagrams, the distinction between these two types of vision.

In spite of agreeing with De la Hire's concepts of perfect and distinct vision, Jurin next set out to refute his hypothesis that the eye had no other accommodating power except that due to the contraction of the iris. To do this he drew upon the optical calculations contained in the body of Smith's 'Opticks'. Unfortunately, it is not easy to follow his method since the details he gave were sketchy; but the general line of his argument is clear. He assumed De la Hire's hypothesis to be true, drew certain conclusions from it, and showed that calculations based upon these conclusions were contrary to observation. In other words, he used a variation of the "reductio ad absurdum" type of proof.

Jurin made the initial assumption that the greatest distance from which rays could be collected to a point on the retina (i. e. perfect vision) was 27 inches; from this he calculated that we could just separate two point sources (e. g. stars) subtending an angle at the eye of 26'. Even making the aperture of the iris as small as possible, it was not possible, by calculation, to reduce this angle substantially. However, he said that observation showed that we could separate two stars as close together as 4'.

Therefore, in order to enable us to separate distant objects, such as stars, the eye must have a distance of perfect vision greater than 27", and Jurin calculated that the distance of perfect vision necessary to separate stars subtending an angle of 4' must be 14 ft. 5 inches. However, with this distance of perfect vision, he said we should not be able to read a book, such as Smith's 'Opticks', at a distance of $13\frac{1}{2}$"; this again was contrary to experience.[16]

> "And if instead of 27 inches, a larger distance be pitched upon for the invariable distance of Perfect Vision, this will a little help the matter with regard to the intervals of the stars; but will increase the confusion at the distance we usually read at. If a smaller distance be pitched upon, we shall read more easily at our usual distance, but shall not see the interval between the two stars, unless they are more than 13' asunder."

In this way Jurin argued that in order to separate objects such as close stars at an angle of 4' we needed to have perfect vision at a distance in excess of 14 feet; and that in order to read small print we needed perfect vision at less than 27". Thus, some form of accommodation was necessary. Having established to his own satisfaction that accommodation did take place, Jurin then summarised the various ways by which it could be achieved.

The first hypothesis to be mentioned was that which supposed the eye to be at rest for distance objects, which would thus be in focus; and to be compressed by its external muscles, so that it elongated when viewing close objects. Jurin objected to this explanation of accommodation on a number of grounds. He thought that the sclerotica was too hard easily to change its shape, but his main objection lay in the effect that such a change of shape would have on the retina. He thought that its fibres would be unevenly pressed together by this action, but even more, it would be extremely difficult for the image to be focused clearly over the area of the retina since its shape would have to change from spherical to oval He was also of the opinion that the change in shape of the retina would be considerable, since he estimated that the increase in length of the eyeball would have to be ten percent in order to allow the eye to focus clearly from 6" to 14'. Another hypothesis, and one which I have not seen mentioned before Jurin's paper, was that the external muscles pulled the eye back into its socket, so that by pressing against the orbit, it was shortened. In this way distant objects would be focused; and when at rest and not pressed back, it would see close objects clearly. This again was dismissed for the same reasons as above.

Another option which was mentioned, and which had been frequently put forward over a long period, was the use of the ciliary ligaments in order to move the position of the crystalline lens. However, Jurin investigated this on geometrical grounds, and dismissed it.[17]

> "A third opinion is, that the eye, when at rest, is suited to the most distant objects, and that in order to see the nearer ones distinctly, the crystalline humour is by

> means of the ligamentum ciliare drawn forwards, so as to increase the distance between its back surface and the Retina sufficiently to unite the pencils into points upon that membrane.
>
> But to see objects with Perfect Vision from 14 feet 5 inches to 6 inches, it would be necessary that the crystalline should be drawn forwards by about 0. 87 (tenths of an inch) which the uvea will not permit, there being no more than the distance of 0. 22 (tenths of an inch) at the most between the uvea and the crystalline."

A major obstacle delaying the discovery that accommodation is caused by the change in shape of the crystalline lens, must have been the difficulty of reconciling two properties of the eye. The first was that, as observations progressed, it must have become increasingly obvious that the eye is at rest when viewing distant objects, since it is easy to feel that an effort is necessary to adjust one's eyes for close objects. The second was that the obvious agents which could cause the change in the lens were the ciliary ligaments, and the natural mode of operation of these would have appeared to be to stretch the lens and make it less convex for distant objects, with the assumption that the natural elasticity of the lens would make it more convex when viewing close objects, i. e. it would be at rest in this latter position and thus one would expect to feel an effort when viewing distant objects. As has been indicated in the introduction to this paper, the ciliary muscles and ligaments act on the lens in such a way that when they are tense, they relax their effect on the lens, and it becomes more convex under the action of its natural elasticity. When they are relaxed, the result is to pull the lens so that it becomes less convex, i. e. the ciliary muscles are at rest for distant objects.

Jurin considered the above method of accommodation next. He assumed that the eye was at rest for close objects, and that the ciliary ligaments stretched it for distant objects, making both its surfaces less convex. His main objection to this hypothesis was that it demanded a considerable change in the convexity of the lens. He estimated that in order to accommodate the eye between the limits of 14'5" and 6" the lens would need to have the radius of each of its surfaces increased by 2/5, and he thought that the texture of the lens was too firm, and the strength of the ciliary ligaments seemed to him to be too weak to do this.

Having discussed the current theories, Jurin then put forward his own,[18] which was a compromise, based upon the eye being at rest for viewing objects at about 15" - 16" and using one method of accommodation for viewing closer objects, and another for objects further off.

He chose the distance of 15" - 16" for the following reasons, indicating that he was convinced that the eye was at rest for viewing at a certain distance and that this was the distance he felt was the most likely one:[19]

> "When the eye is perfectly at rest, no force, strain or effort of any kind being used by any of its parts, it is then suited to see with Perfect Vision at some one determinate moderate distance. This distance, I suppose, is for most eyes about 15" - 16", the usual distance for reading print of a middle size. For it is likely we usually read at that distance where vision is perfect without any straining of the eye."

The choice of a middle distance position of rest for the eye was ingenious, and the justification he put forward in support of his hypothesis was a blend of biology and mathematics at an intellectual depth which I have not encountered in any paper published earlier. As with De la Hire, then, Jurin may be considered to have increased the intellectual rigour with which the subject of accommodation was studied, though apparently his theory did not necessarily lead in the right direction.

Jurin's hypothesis for viewing close objects was, as far as I have been able to gather, original; he first established that the uvea was muscular, containing both straight (radial) and circular fibres. He maintained that the uvea was attached extremely strongly to the inside of the cornea, around its outer edge. He was also of the opinion that the cornea was "a compressible and springy membrane, easily giving way to any force external or internal, and easily restoring itself to its former figure by its own spring assisted by the pressure of the aqueous humour within it." Thus, when viewing close objects, the iris would contract as commonly observed, and this would also have the effect of pulling the outer ring of the cornea inwards, and rendering it more convex. It was admitted by Jurin that the muscular ring on the outer edge of the uvea, where it was attached to the cornea, had not yet been discovered; but he argued for its existence as a necessary balance to the inner ring, which was responsible for the contraction of the uvea (reducing the pupil), and also from the strength of its attachment to the cornea. By virtue of the strength of the attachment he thought that it was capable of exerting the necessary force to make the cornea more convex. A major disadvantage of this theory (not mentioned by Jurin) must be the effect of viewing a distant object in bright light. The intense light would cause the uvea to contract, and, following the theory, this would render the cornea more convex, making the distant object out of focus. However, one could postulate an immediate compensation using the mechanism he proposed for viewing distant objects.

Again, the explanation given for distant vision was original as far as I can discover. It also shows considerable ingenuity of thought. Jurin was of the opinion that the crystalline lens was too firm to lend itself easily to a change of shape; however, he noted that the lens was enclosed in a capsule, and he maintained that there was water between the capsule and the lens. The ciliary ligaments were attached to the front surface of the capsule at the edge. To view a distant object the ligaments were supposed to contract and this would result in the front surface of the capsule being drawn a little forward and out, forcing the liquid it contained from the middle portion towards the edges. Thus, the whole front surface of the capsule would become less convex. When the ciliary ligaments relaxed, then the natural elasticity of the capsule would restore it to its former convexity. In this way, Jurin overcame the necessity for there to be a change in shape of the crystalline lens, which was something which many scientists found extremely difficult to accept. Anticipating opposition to his novel theory, Jurin explained that it was a necessary part of his hypothesis that the ciliary ligaments should not be sufficiently strong to change the shape of the lens itself. He assumed that it would be said that if nature had wished for accommodation to be achieved in this manner, then it would have seen that the ligaments were sufficiently strong to change the shape of the lens, and thus avoid the necessity for the device involving the change in shape of the capsule. He said that the ciliary ligaments were attached to the end of the cornea, and that if they were sufficiently strong to stretch the lens, this would result in an equal and opposite force which would render the cornea more convex, and counter the effect of the lens being less convex due to the stretching.

Jurin's theories are summarised above. However, the depth of his thought in this matter can best be appreciated from his own account of his hypotheses, which is in Appendix I to this section

Having put forward his theory in general terms, Jurin then endeavoured to verify some of his assumptions mathematically.[20]

It is clear from the figures given below that Jurin had little concept of significant figures. His measurements were given in tenths of a London inch, and were obviously arrived at by taking the average of a whole series of results. None of the individual results could have been measured to the accuracy of .00001 of the London inch, but he used average results which contained four places of decimals, and which therefore purported to give this order of accuracy.

He took the following average constants of the eye:

Radius of cornea	3.3294
Radius of front surface of crystalline lens	3.3081
Radius of rear surface of crystalline lens	2.5056
Distance from cornea to front of lens	1.0358
Thickness of lens	1.8525
Refractive indices of aqueous & vitreous humour	1.3
Refractive index of lens	13/12 (relative to aqueous humour)
Size of average eye	9.4

(in this he disagreed with Petit's figure of 10.0578 which he converted from the original measure in French lines.)

He calculated that the distance for perfectly distinct vision for an eye of the above dimensions was 33". This would be the distance at which there would be no straining or effort of any of its parts. Jurin called it the natural distance of the eye. He felt that it was unfortunate that these calculations did not agree with his observations of the distance at which we naturally read books, supposing that 33" was too great a distance for relaxed vision. Confidence in the accuracy of his observations led him to believe that the distance at which we usually read fairly large print must be our natural distance of relaxed vision, and this was about 15" or 16". To support this argument he used a calculation developed by Smith [21] that it required nearly as great a change in the conformation of the eye to lessen the natural distance to one half as to increase the natural distance to infinity. (Details of Smith's work are given later on Page 135.

This concept would fit very well into Jurin's theory of accommodation which was that we see most clearly at 15" - 16", the eye being perfectly at rest; and then to view at infinity or to view at the near point requires an equal change in the conformation of the eye in either direction.

However, Jurin now needed to modify his calculations, which had provided him with a natural distance of 33", roughly double that which his reasoned arguments had provided. He achieved this modification by supposing a very slight increase in the refractive index of the aqueous humour, which he had previously considered to be the same as that of water. He increased it from 4/3 to 81/60, from which he calculated the natural distance to be 14. 7", which agreed closely with the figure that his reasoning had led him to. One is led to wonder that such a small change from 4/3 to 81/60 should produce such a considerable change in natural viewing distance, but no details of the calculations are given.

The experiments which led to the "constants" of the eye which Jurin used in his calculations were necessarily crude. It is therefore perhaps surprising that he considered it necessary to make a minute change in one figure, which he must have realised was not known to a very great degree of accuracy, in order to bring his calculations into line with his very reasonable assumption that the eye was at rest at the "naturally adopted" distance for reading.

Jurin next considered whether he could calculate the effect that the changes he proposed would have on the conformation of the eye. He first attempted to prove that the change in the radius of curvature of the cornea would produce an ability to see clearly to a distance of 5", a distance which he took as a typical near point. He considered the greater muscular ring of the uvea to lessen its circumference by about 1/47, i.e. from 4.4392 to 4.3462 and he said that this would change the radius of curvature of the cornea from 3.3294 (as it was for the natural distance) to 3. This would increase the distance from the lens to the cornea from 1.0358 to 1.1193.

From these figures he calculated that rays from a point on the principal axis 5" from the eye would meet on the retina; i.e. that the near point was 5"; this agreed with his observations. Jurin was at pains to point out that the eye was able to withstand changes of this order. He said:[22]

> "Nor can any just objection be drawn against the change of conformation we have here supposed in the eye, as being greater than can reasonably be admitted. For the radius of the cornea alters only a tenth part, and this arises from the contracting of the greater muscular ring of the uvea only 1/47 part, which is vastly less than the contraction of the lesser muscular ring, that being able to contract into half its dimension, when the eye is exposed to strong light."

However, Jurin did admit that there was a more important difficulty stemming from his theory:
If the outer ring of the uvea contracted, then so also must the circumference of the circle where the cornea joins the sclerotica. This can best be understood from diagram I overleaf. The diagram shows the eye from the front:

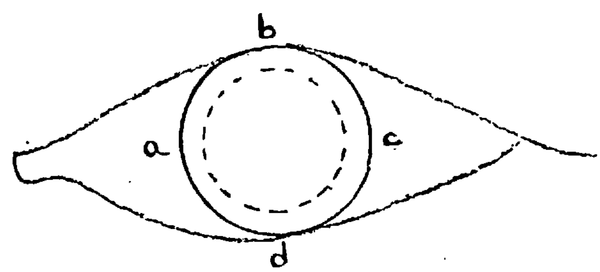


Diagram I

The Eye Viewed from the Front

abcd is the ring where the cornea joins the sclerotica. When close objects are viewed, the greater muscular ring of the uvea contracts and decreases the radius of curvature of the cornea, so that the ring where it joins the sclerotica becomes smaller and is indicated by the dotted circle.

Jurin explained this as follows:[23]

> "But possibly some doubt may arise about that circumference of the cornea into which the uvea is inserted, whether by reason of its union with the sclerotica, it can comply with the contraction of its muscular ring, so as to be drawn inwards towards the pupil, and likewise to contract itself into a less circumference. To this, therefore, we reply that the space by which it approaches the pupil, is by our supposition very small, being less than ...* part of an inch; and this small motion is favoured by the obliquity of its junction with the sclerotica observed by Monsieur Petit; and that space, by which that circumference shortens its length, is less than 3/100 of an inch, which in a compressible and dilatable membrane is not hard to conceive."

*This number was not printed in my copy of Smith's 'Opticks'. In another copy 1/200 has been inserted in ink. The omission does not occur in the Errata.

Thus Jurin was of the opinion that the natural elasticity of the materials of the eye were sufficient to allow the cornea to take on a lesser radius of curvature.

Lastly, Jurin considered the effect that the change in radius of the cornea would have on the ciliary ligaments and the crystalline lens:[24]

> "And lastly, as we have above taken notice that the elasticity of the capsula and the tone of the ligamentum ciliare antagonise each other, it follows that, when the edge of the cornea is drawn inwards by the contraction

> of the uvea and consequently the ligamentum ciliare is relaxed, the capsula will then grow more convex. On which account somewhat less convexity of the cornea and a less contraction of the uvea will be necessary, than is above supposed."

Thus, as a by-product, almost, of his theory, Jurin did incorporate a change in convexity of the crystalline lens as playing a part in accommodation, though only a very minor part. Having satisfied himself that his theory for accommodation at close distances would work, Jurin turned to a closer examination of the theory he proposed for focusing on objects at distances between about 15" and infinity.[24]

For this he used measurements of the eye published by Petit.[25] He treated the crystalline, capsule and the fluid between the lens and capsule as a compound lens, and stated that the attachments of the ciliary ligaments to the capsule must be far enough apart to allow light to enter un-interruptedly, even when the iris was fully dilated. During the act of accommodation for distances greater than 15", Jurin supposed that the ciliary ligaments drew the front of the capsule forward by 1/400 of an inch, increasing the radius of the central portion dd (figure 51, over) between the points of attachment of the ligaments from 3.3081 to 4.200; the thickness ce of the crystalline would also be reduced from 1.8525 to 1.8270. From these figures Jurin calculated that rays from an object 14'5" from the eye would be brought to a focus on the retina. Thus he stated that by means of the very slight movement of the front of the capsule by a maximum of 1/400 in. which caused the fluid between the lens and the capsule to create a compound lens of less convexity, he had shown how the eye could accommodate itself between 15" and

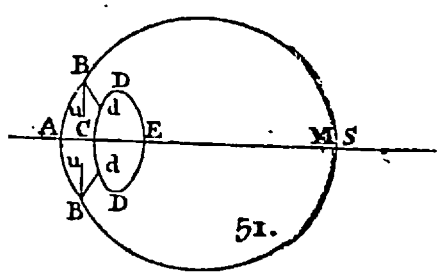


Figure 51 from James Jurin's "Essay on Distinct and Indistinct Vision."

14'5" with "perfect vision". However, he felt that this involved undoubted strain on the parts of the eye causing the alterations necessary for this change in focus, namely the uvea and ciliary ligaments, and thus the eye probably merely adjusted itself just sufficiently to obtain "distinct vision." This, he said, could not normally be distinguished from "perfect vision". No explanation was given for the choice of 1/400 of an inch for the movement of the front of the capsule. Obviously by its nature it would not be possible for it to change its shape very much, but by choosing a slightly larger figure, Jurin would have been able to establish a greater range of accommodation at a distance (larger than 14'5") and so have avoided some of his explanation of "distinct" rather than "perfect" vision at a distance, which on occasions appears unconvincing.

Jurin spent some time elaborating his theory that the eye accommodated itself only sufficiently to view an object with "distinct vision", thus avoiding the additional effort required for "perfect vision."[26]

> "For instance, if a young adult person in reading holds his book at 10" distance it will not be necessary to contract the greater muscular ring of the uvea so much as to procure perfect vision at the distance of 10": for a middling print it may be enough to contract the uvea so much only as would procure "perfect vision" in case the book were at the distance of 13" or 14"; or at the distance of 11" or 12" if the print be small; and in this conformation of the eye he may see with sufficient distinctness at 10" for the book to be easily read. And this lesser contraction, being less laborious, will be used instead of the greater contraction, which is more fatiguing, especially if he reads a great while."

This led him to state that the conformation of the eye was not always the same when looking at objects at the same distance. He said:[27]

> "So that in looking at very distant objects the eye will not always have one and the same conformation, namely the flattest possible conformation of the capsula; but that conformation will be different for different objects at the same distance, as well as for the same objects at different distances."

Jurin used the distinction between "perfect" and "distinct" vision to explain how we could see clearly beyond the limits of 5" and 14'5". Perhaps strangely in such a well-documented and clearly argued paper, he confined himself to just a single example, showing how we might see clearly beyond the limit of 14'5" for "perfect vision" at a distance;[28] and this was merely to explain how we might well be able to read a poster at a distance of 16':

> "Again, let another object, as a playhouse bill pasted against a wall, be presented to the eye at a distance of sixteen feet. Then as soon as we attempt to read this bill, the anterior surface of the capsula will be rendered flatter, so as to accommodate the eye to some distance not exceeding its utmost limit of 14'5"; and though by this means vision at the distance of 16' cannot be rendered perfectly distinct, yet will it be rendered less indistinct than before, and perhaps distinct enough to read the bill with ease."

This seems a remarkably unenthusiastic statement to explain how his basic theory of accommodation might be extended beyond the limit of 14'5". This was essential if the theory was to be considered seriously, since it also had to be able to explain how we saw clearly at much greater distances than 16'.

In view of this it was perhaps surprising that Jurin made no effort to modify his original calculations by proposing a slightly greater forward movement of the front of the capsule than the 1/400 inches. This additional forward movement would have been minute and would have enabled him to have achieved a distance greater than 14'5" for perfect vision. The explanation perhaps lies in the concern he showed to minimise the strain on certain parts of the eye which he thought would be caused when the eye was accommodated to his limits of perfect vision:[28]

> "And this (strain) must happen chiefly near the limits of perfect vision, where the straining either of the greater muscular ring of the uvea, or the ligamentum ciliare, to the utmost they are capable of, must be somewhat laborious and uneasy."

Therefore, he was perhaps reluctant to modify his theory even very slightly if it would increase the strain on the ciliary ligaments following the increase in the distance of perfect vision.

It is difficult to avoid a conflict of opinion in assessing the calibre of Jurin's work. On the one hand, his theory of accommodation was original, well-conceived and presented in restrained scientific language. In it he clearly showed himself capable of taking up a position in advance of contemporary thinking. On the other hand, he allowed himself to be circumscribed by rigidly adhering to constants which were essentially approximations, though appearing to possess a degree of accuracy which was impossible with the measuring techniques available at the time. It seems certain that such meaninglessly precise conclusions that the limit of distinct vision was 14'5", which he allowed to be a difficulty in his theory, could have easily been avoided if he had been able to appreciate the true nature of the figures which produced such results.

The part played in accommodation by the contraction of the iris was strangely not emphasised by Jurin. He devoted very little space to it, and gave details of only one elementary experiment. It would perhaps be true to say that he was far from clear in his own mind about the cause and effect of this contraction. He stated initially that the closing of the iris made the image clearer. However, in dull light, he thought that accommodation must be effected entirely by the changes in convexity of the cornea or capsule, since the pupil would be dilated under these conditions.

However, in bright light, he thought that the contraction of the iris might be sufficient to make any other form of accommodation unnecessary -a sweeping conclusion:[29]

> "But in strong light the contraction of the pupil is chiefly made use of (in accommodation.) For then that contraction answers two purposes: one, to exclude an overgreat quantity of light which would be offensive to the eye: the other to lessen the indistinctness. And when the light is very intense, the pupil may contract so much as of itself to cause distinct vision, and so render other means altogether unnecessary. So that these two several means of procuring distinct, or less indistinct vision, may sometimes be used jointly, that is each in a moderate degree, and sometimes single.

> The degree, to which the pupil contracts, does not absolutely depend either upon our will, or upon the sensation of confusion in the object; but partly upon the degree of light.
>
> This is easily proved in the following manner: By daylight take any book, and standing about the middle of a room, with your back to the window, hold the book so near as the letters may appear indistinct, and yet not so much but that you can read, though with some difficulty; then turn your face to the light and the book will be read with more ease. Again, holding the book at the same distance from your eye, go into the darkest part of the room, and standing with your back to the light you will find the book not at all legible; but upon coming to the window, with your face to the light you will be able to read, especially if the sun shines, with great ease and distinctness."

One can perhaps fairly conclude from the above that the contraction of the iris was an area of study which Jurin had neglected. The experiment he described above used contraction caused by bright light only. He mentioned in passing that contraction could be caused by "the sensation of confusion in the object" but did not elaborate on this at all. He also omitted to mention the contraction of the pupil which took place when viewing close objects, independent of the intensity of light, which had been well-known for many years. In view of the fact that his essay concerned itself with distinct vision, this omission, and the lack of emphasis on this whole subject, is perhaps puzzling.

Jurin next concerned himself with the changes in the eye which he felt were caused by custom or habit. To explain the view, which was at the time widely held, that persons, such as sailors, who were accustomed to look at distant objects, were able to see more distinctly at great distances, Jurin reasoned that additional strength was obtained by the ciliary ligaments in constantly pulling forward the crystalline. He thought that this led to a corresponding difficulty when they attempted to look at close objects. Persons who habitually tended to look at close objects, such as watchmakers and students, had the opposite strengths and weaknesses. Since the development of other bodily muscles by constant use was well-known, it is perhaps not surprising that it was thought that similar development would take place in the muscles of the eye if they were extensively used. This theory has re-appeared even during the present century, notably Aldous Huxley's support of eye exercises to overcome defects of vision,[30] but it is now known that one cannot change the eye's accommodating power by exercising its interior muscles.

In his last section on the accommodating power of the eye, Jurin dealt with the changes caused by age. He noted first that the pupil in children was usually more dilated than in adults, and ascribed this to the greater flexibility of the cornea. This enabled the eye to be focused more completely by means of the change in curvature of the cornea, and thus there was no requirement for additional focusing by means of the iris. Jurin explained the increasing deterioration in accommodating power with advancing age as being due to a progressive stiffening of the cornea.

It was commonly held that eyes tended to become long-sighted with age, i. e. eyes tended to become hypermetropic. In fact they become presbyopic. Jurin said that this was generally attributed to the shrinking of the coats (outside layers) and humours of the eye. While agreeing with the premise, he disagreed with the explanation, saying that if the eye shrank, then it would tend to become short-sighted rather than long-sighted. This, he explained, was because the distance between the cornea and retina would be less. Jurin held that the explanation was as follows:[31]

> "The cornea, as it is of a rarer texture, and is more exposed to the air than the sclerotica, will in length of time shrink a little more than the sclerotica, and will by that means grow a little fatter than it was before."

Jurin estimated that the shrinking of the cornea combined with its increasing rigidity with age caused the nearest point of distinct vision to change with age as follows:

Young children	3 - 4"
Young adults	5 - 6"
Old age	20 - 30 - 40"

In the last case the only assistance that the eye had in viewing close objects was from the contraction in the pupils, and this was only sufficient in strong light. He estimated that if the arc of the cornea shrank by 1/200 inch, the natural distance of viewing would be removed from 15" to 77" (no explanation was given of choice of these figures); and that when the cornea had, through ageing, lost its flexibility, this distance could not be reduced to less than 38" or 39" for viewing close objects. The effect of age on the refractive indices of the refracting humours of the eye was briefly considered; Jurin felt that these could well suffer an increase in the refraction, and that this would tend to delay the progress of the tendency to long-sight with age.

We can now turn our attention to the far larger work of the two, that of Robert Smith, whose 'Opticks'[32] was, for its time, an extremely comprehensive book on all aspects of light.

Smith started his explanation of the working of the eye by explaining its basic function, and in particular by deducing the reason for its shape.[33] (see figs. 153&154.)

> "One might contrive a tolerable eye in this manner, by placing a pellucid hemisphere ABC to serve for the fore part, and another concentric one DqE, opposite to the former, to serve for its bottom or back part; making the semidiameter, Oq, of the latter triple the semidiameter, OB, of the former; and then by filling the whole cavity of both with water. By this means rays of light flowing from the points P, Q, R, etc., of remote objects, after refraction at the surface ABC will be collected to as many other points, p, q, r, of the cavity DqE and paint an image upon it. And because a spherical surface does not accurately refract all the rays of a large pencil to a single point, but only those that go pretty near its axis; this imperfection might be remedied by covering the base AC, of the lesser hemisphere, all but a moderate hole about the center O; which would answer the purpose much better than if the surface itself was covered, all but a hole in the middle about B. For in this latter case the surface ABC would not receive rays from the lateral points P, R, so directly as those from the middle of the object, to all which it is exposed alike when the hole is left open at the center O.
>
> Though this construction of the eye appears not amiss at first sight, yet we shall see presently that the author of nature has wisely varied some things for the better, and added others absolutely necessary, though in everything we cannot perceive his designs. In the first place he would not make use of an entire hemisphere ABC, but retaining the middle part, has taken off pretty much from the sides, and yet without contracting the compass of objects taken in at one view.

The reason of this was to bend inwards the edges of the larger hemisphere about D and E, thereby reducing the shape of the eye to a rounder figure, for the convenience of its motion every way in the cavity that contains it. He has therefore given it such a shape, as is expressed in this other figure, representing an human eye dissected through its axis, all the parts being twice as big as in the life to render them more conspicuous.

Here the transparent parts of the coat called the cornea is ABC, the remainder ATYC being opake, and a portion of a larger sphere. Within this outward coat anatomists distinguish two others; the innermost of which is called the retina, being like a fine net composed of the fibres of the optic nerve YVT woven * together, and is white about the parts p, w, r, at the bottom of the eye. The cavity of the eye is not filled with one liquor, but with three of different sorts. That contained in the outward space ABCOEGFDO is called the aqueous humor, being perfectly fluid like water; the other contained in the inward space EpqrDFG is a little thicker like the white of an egg, and is called the vitreous humor; the third humor FG is shaped like a lens of unequal convexities, lying between the two former, and fixed to the side coats by filaments or threads extended all round it, and is called the crystalline humor, being hard like the white of an egg boiled, but as clear as the other two, and differs from them in a greater degree of refractive power; whereby the rays that came from the points P, Q, R, having received a degree of convergence by the refraction of the cornea ABC, are made to converge a little more by other refractions at the surfaces of the crystalline FG; *so that uniting in as many other points p, w, r, upon the retina, they represent the points of the object P, Q, R, from whence they came. And perhaps the rays are so directed by these secondary refractions at the crystalline, as to fit the cavity pqr intended to receive them; which otherwise must have been a portion of a larger sphere, according to the fictitious design in the former figure.

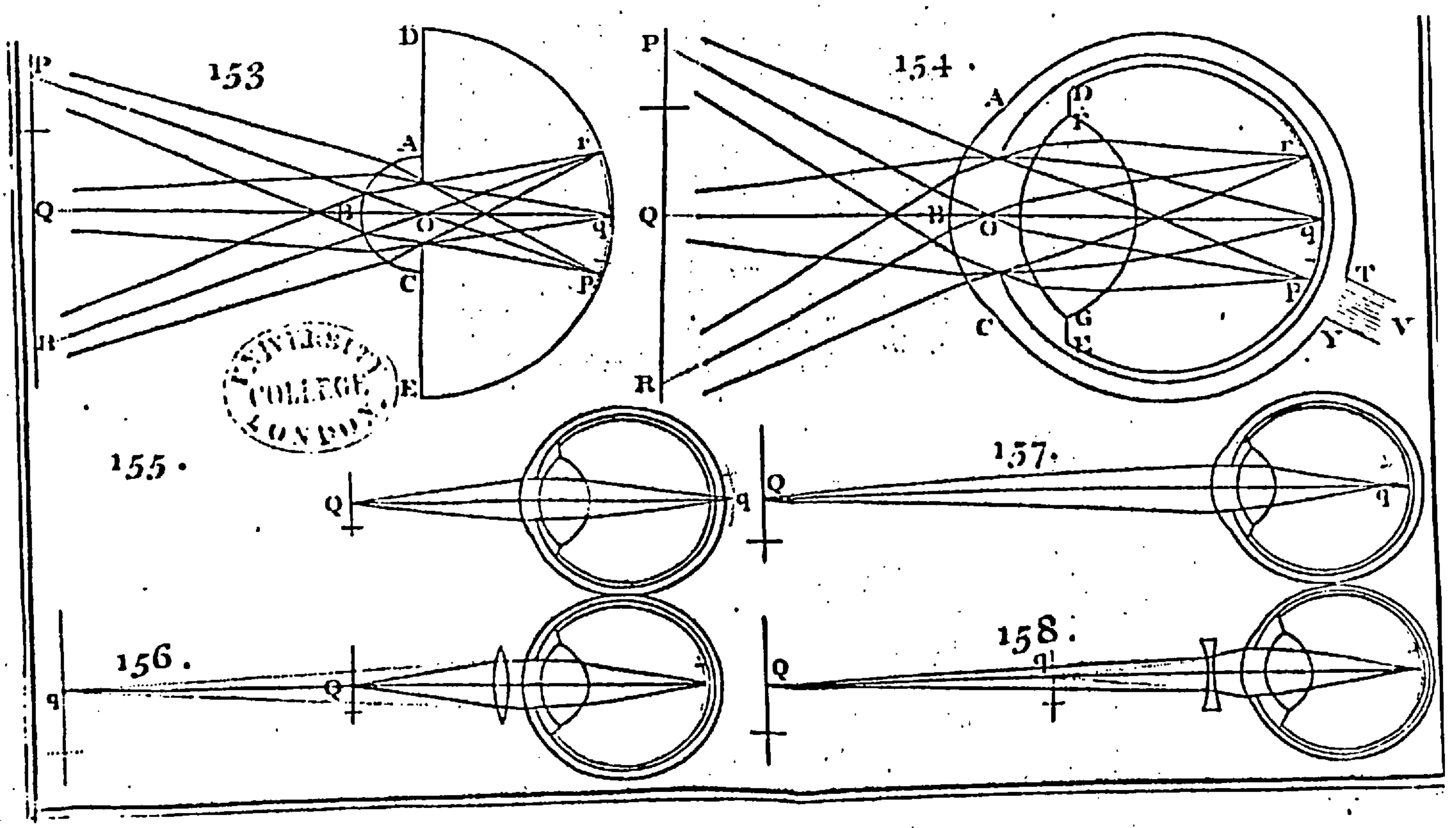
Besides this there was a greater need of the lens FG upon another account; namely to help

*should read 'q'.

the eye to conform itself for seeing objects distinctly at all distances, which was wanting in the fictitious eye. There are two ways of doing it by the help of this lens FG, in order to see things near at hand; either by moving it nearer to the outward cornea , or by increasing its convexity, or perhaps by doing both at once. If it is moved towards the cornea, this may be effected by the pressure of the muscles against the sides of the eye, and consequently against the vitreous humor; but if the crystalline alters its figure and becomes rounder for seeing near objects, the filaments DF, EG, whose greater tension helps to flatten it, may perhaps be slackened by the lateral pressure aforesaid; and possibly both these alterations are made at the same time. The hole or pupil O is not placed in the center of the cornea ABC, as in the fictitious eye, but somewhat nearer to its front. The reason is uncertain, unless this also may contribute to make the images coincide with the cavity of the retina, (in all their parts,) which otherwise must have been shaped according to a larger sphere."

He called his first diagram (fig. 153) in which he showed merely the basic production of the image on the retina, a "fictitious eye." This device enabled him to show more clearly the way in which the actual eye improved on the elementary working of this fictitious model. For instance, the need for a protuberant cornea to catch rays from a wide angle could be easily appreciated. As could the way in which a pupil, preferably set back from the front surface of the eye, would help to produce a clear image on the retina by limiting the size of the pencil of rays allowed into the eye.

Smith also gave a function to the crystalline lens; he said that it enabled the eye to focus on close objects either by moving closer to the cornea, or by increasing its convexity, or by doing both at once. Smith said that the movement of the lens was caused by the pressure of the muscles against the sides of the eyes. It is not clear what he meant, since the only muscles which could exert such pressure would be the muscles external to the eye. It is conceivable that if they flattened the eyeball, this would force the front of the vitreous humour forward which, in turn, would push the lens forward. The change in shape of the



Figures 153 - 158 from Robert Smith's "A Compleat System of Opticks."

lens Smith correctly attributed to the suspensory ligaments (which he called 'filaments'.) He said that increased tension in these filaments helped to flatten the lens, and when they were made slacker (perhaps by the action of the external muscles flattening the eyeball), the lens would become more convex. Thus, it was simple for Smith to postulate that accommodation was caused by a combination of movement of the lens, and a change in its convexity, both caused by the external muscles. It may also probably be supposed that he regarded the suspensory ligaments as inextensible, and therefore not muscular. Smith did not elaborate further on his theory in the first part of his book, which was sub-titled 'A Popular Treatise'. However, he did deal very thoroughly indeed with other theories of accommodation in the last part, entitled 'Author's Remarks on the Whole Work'.[34] In this critical summary of other scientists' theories, he showed a detailed knowledge of the constants and functioning of the eye, and incidentally showed that his own theory must have been arrived at by a careful process of elimination from other hypotheses.

Smith first considered the three ways in which he thought that accommodation could be obtained, and then investigated geometrically the effect these changes in the conformation of the eye would have on the paths of the rays. In fig. 2 overpage, he imagined accommodation to be produced by some change in the shape of the refracting surface only.

C was an object situated at the least distance of distinct vision. D was an object at double this distance, and in the third case the object was considered to be at infinity, and marked E in the diagram, with the ray EA in the diagram parallel to the principal axis. For this exercise, Smith considered the retina to be unmoved;

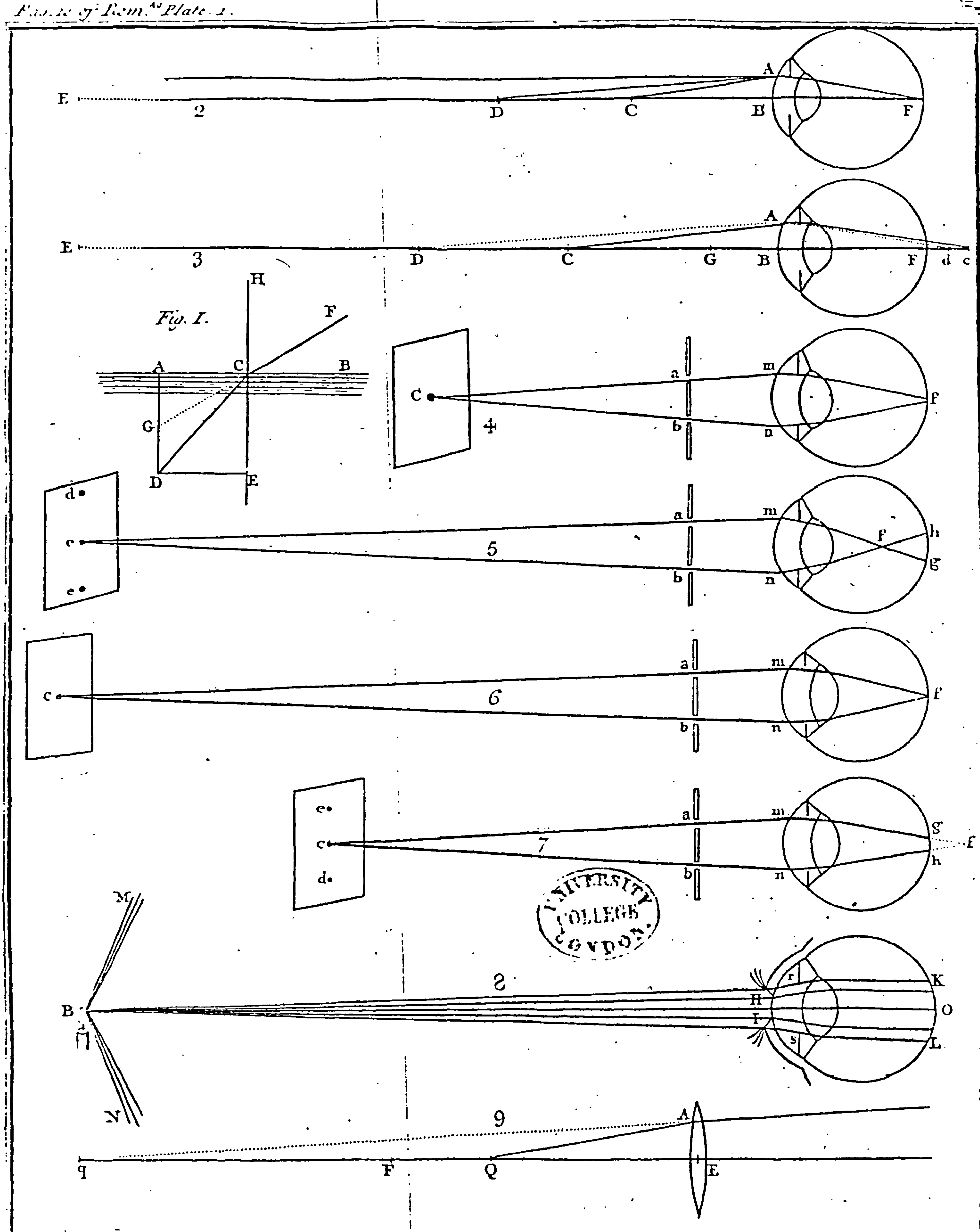


Plate I from Robert Smith's "A Compleat System of Opticks."

therefore BF was a constant. The rays from C, D and E were successively focused on the retina by some change in the refracting surfaces. He considered three such rays, all striking the cornea at A, CA, DA and EA.

From his initial premise CD = CB
and also CB = CA since AB is very small
Therefore CDA = CAD
also CDA = DAE

Therefore as the object moved from C to D it is refracted less by an angle CAD; and as it moved from D to infinity it was refracted less by an angle DAE (which was equal to CAD.) Thus, Smith had proved the point which is best stated in his own words:

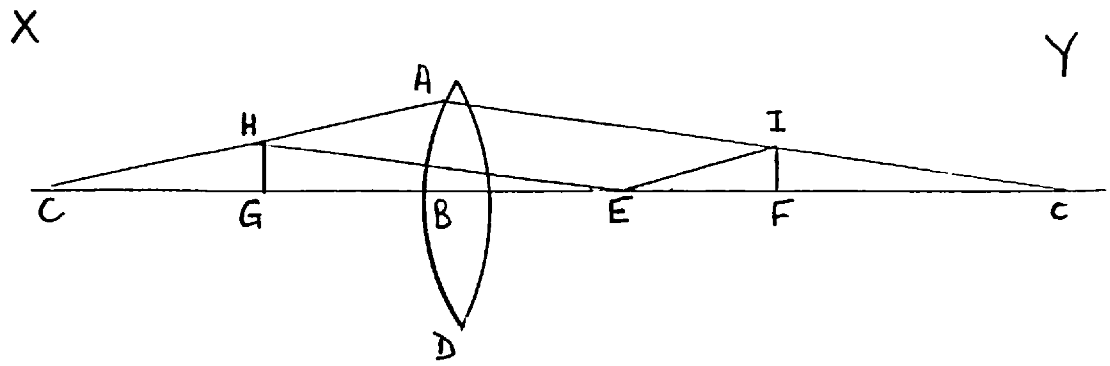
> "If an object be viewed distinctly and successively at three different distances from the eye; the first of which may be the least distance at which it may be viewed distinctly, the second double the first, and the third infinite; it is remarkable that as great alterations in the figure of the eye are necessary for seeing the object distinctly at the first and second distances, whose difference is small, as at the second and third, whose distance is infinite."

Smith next considered the refracting surfaces to remain constant and calculated the movement that the retina would have to make in order that the images from C, D and E (fig. 3) should fall on the retina. The ray from E which was parallel to the principal axis was considered to strike the retina at F. G was the point at which rays coming from inside the eye, i. e. right to left, parallel to the principal axis falling on the rear surface of the lens, would cut the principal axis. Smith said that he had computed the distance GB to be no more than 5 or 6 tenths of an inch. (He gave no experimental justification and showed no calculations which led him to this result.) He made the distance GC equal to CD saying that since GB was small, this case was very similar to the former, when distances were measured from the cornea at B. The pencil of rays flowing from D was refracted to d on the principal axis; the pencil from C was refracted to c. Smith then stated that:

Fc:Fd : : GD:GC : : 2:1 (since GC had been made to equal CD initially) (1)

Therefore, as the object moved from C to D, the retina would have to move from c to d, and as the object moved from D to infinity, the retina would have to move from d to F, (equal to cd.) Thus, again, the change in the eye,- in this case, the position of the retina,-was the same for the object moving from the closest position of distinct (C) vision to double this distance (to D) as it was when the object moved from D to infinity.

The justification for equation (1) above, was given by Smith as articles 373 and 240 in the main text of his book. These articles are somewhat difficult to follow and, hopefully, the explanation below will be found satisfactory, and easier to follow; it is based upon that put forward by Smith, but applies only to a single convex lens. However, the differences in the refractive indices of the humours of the eye are so slight as to allow an approximate application of the proof to the eye.



An object placed at C produces a real image at c. G is the focus of rays parallel to the principal axis from the direction Y. F is the focus of rays parallel to the principal axis from the direction X. E is the centre of the spherical refracting surface ABD. With E as centre, radius EG draw arc GH. With E as centre, radius EF draw arc FI. A is very close to the principal axis CBc. Now consider EH to be the new principal axis of the surface ABD. H, which is very near to G, is the new focal point of rays parallel to HE; but the ray from c to A passes through H after refraction. Therefore Ac is parallel to HE. Therefore HEG = AcE.

In the same way EI can be made a new principal axis, and CA can be shown to be parallel to EI, and therefore ACB = IEF.

Therefore CH:HE = EI:Ic. but H is very close to G, and I to F.

Therefore CG:GE = EF:Fc. Therefore CG = GE. EF/Fc. But GE and EF are both constants.

Therefore CG = K/Fc where K is a constant and $CG \propto 1/Fc$. ———— (a)

Therefore if CG is doubled Fc is halved.

From fig 3

If C moves to D (DG = 2CG) then c moves to d and from (a) above it follows that:

Fc:Fd = GD:GC = 2:1

This is the original equation stated by Smith.

Smith then considered the case when the accommodation was produced by a combination of both the methods outlined above. This, of course, was the case preferred by him in the main part of his book. The detailed treatment of the two separate ways in which accommodation could be obtained, outlined above, seems merely to have the purpose to draw attention to the phenomenon that apparently intrigued Smith; namely that the same order of change in the eye was required to focus as an object moved from the near point to double that distance, as was required as it then moved to infinity, and that this was true for each of the ways suggested by Smith for accommodation to be accomplished.

However, he did relate this phenomenon to the case of short-sighted persons; deducing that if they could focus clearly between two different distances, the larger being twice the smaller, as most of them could, then this involved as much change in the eye as a normal person focusing from a reasonably close distance to infinity. Smith said that this showed that their eyes were as capable of changing their figure as normal eyes, and that this was the reason that a prescribed single concave lens enabled a short-sighted person to see normally and focus over the normal range. He said[35] that short-sightedness was not "a want of power to vary the figure of the eye and the quantity of refraction, but that the whole quantity is always too great for the distance of the retina to the cornea."

Thus, Smith was able to provide a useful piece of evidence to indicate that the defect of short sight was limited to the inability to focus the image on the retina for certain distances,and that there was no defect in the eye's ability to change its conformation to enable it to focus over a range of distances.

Christian Huygens' (1625-1695) opinion that accommodation was caused solely by the movement of the crystalline nearer to the cornea for close objects, Smith dismissed. He stated that even if the lens moved so far as to touch the cornea, it would not produce the range of accommodation required by the eye. In any event, he said that this could not happen, since the uvea would intervene, and that Petit[36] had shown that even this range of movement was not possible, since the uvea in humans was close to the crystalline and plane.

De la Hire's theory that the only change which took place as the eye concentrated on distant or close objects, was the closing of the pupil for close objects, was dealt with next. Smith gave a detailed account of De la Hire's experiments, and gave a simple, but fundamental, reason why, in his opinion, their results were not reliable. Smith's account of the experiments was very clear, and since De la Hire's own accounts were couched in rather different language, it is probably worthwhile to quote Smith in full.[37]

> "Let two very small holes, a, b, be made with a pin in a card or in paper, so near to each other, that being held close to the eye, the rays that come through both, may enter the pupil. Then let a small black spot c

upon a white paper, be viewed through them; and if the experimenter be short-sighted, let the paper be placed at such a distance from his eye, as he usually sees an object at with most distinctness and most ease, as suppose at the distance of six inches; and in looking through the holes the spot will appear distinct and single. Then let the paper be removed to a greater distance, suppose of ten inches, such as that the same eye may be able to see an object without any apparent indistinctness. Then let the spot be attentively viewed by the naked eye, in order to make such a change in its conformation, as is usually supposed necessary to see an object distinctly at such an increased distance. Now, the eye being supposed to have taken the necessary conformation for seeing that spot distinctly at that distance, it may in consequence of this supposition be expected, that upon clapping the card before the eye and looking through the two holes, the spot should appear single, as it did at the former distance of six inches. But experience shews the contrary; for it appears double like two distinct spots, d, e; whose interval de is so much the greater as the distance of the paper from the eye is greater, as in fig. 5. (Plate I)

If the experimenter be long sighted, let the paper, likewise, be first placed at such a distance from the eye, as he usually sees an object at with most distinctness and most ease, as suppose at the distance of fifteen inches; and in looking through the holes the spot will appear distinct and single. Then let the paper be brought nearer, suppose to the distance of seven inches, at which distance the same eye is able to see the object without any apparent indistinctness. And let the spot be viewed attentively by the naked eye, in order to make such a change in its conformation, as is commonly supposed necessary in order to see an object distinctly upon so lessening the distance. Now, the eye being supposed to have taken the necessary conformation for seeing the spot distinctly at that distance, it may, in consequence of this supposition, be expected, that upon clapping the card before the eye and looking through the two holes the spot should appear single, as it did at the former distance of fifteen inches. But experience shews the contrary; for it appears double like two distinct spots, d, e, whose interval de is so much the greater as the distance of the paper from the eye is less, as in fig. 7.

Now the spot appears single in the first case of each of these two experiments, because the rays ca, cb, fig. 4 and 6, are united at a single point f exactly upon the retina. But when the paper is remoter, fig. 5, ca and cb diverge less than before, and therefore are reunited at f before they arrive at the retina; then crossing each other they fall upon it in two distinct points, g, h, which occasion the double appearance at d and e. For if the holes be moved upwards, the upper spot first disappears at d; because the upper ray ca, first misses the pupil. And when the paper is brought nearer, as in fig. 7,* the rays ca, cb diverge more than before, and therefore tend to reunite in the point f behind the retina, upon which they fall in two distinct points g, h, which occasion the double appearance at d and e. For if the holes be moved upwards, the under spot first disappears at d, because the upper ray ca first misses the pupil.

We come now to consider the consequence. which Mr. de la Hire draws from these experiments. His argument runs thus. It is commonly believed, that an eye which is so formed as naturally to unite the rays upon the retina, when the object is at six inches distance, can make such a change in its conformation as still to unite them exactly upon the retina, when the object is removed to a greater distance, as that of ten inches.

*see Plate I

> If this opinion were true, the eye of the observer in the second case of the first experiment, must have made such a change in its conformation. But the experiment shews that this eye was not in such a conformation as to unite the rays exactly upon the retina; for upon clapping the card before it, the appearance was of two distinct spots, not of one only, as it ought to have been, if the eye had had the supposed conformation. And just after the same manner he reasons upon the second experiment."

This excellent concise account of De la Hire's experiments, and the reasoning leading to his hypothesis, was followed by an equally lucid description of why Smith rejected the hypothesis.[37]

> "In order to make this reasoning conclusive, Mr. de la Hire ought to have proved, that whatever conformation the eye had, in viewing the spot without the holes, the same conformation must necessarily have continued, when the spot was seen through the hole.
>
> But we take the contrary to be highly probable. For when the spot was viewed at the distance of six inches, the eye was then in its natural conformation. It received the rays in the same manner as an artificial eye of the same dimensions might have done, without any the least strain 'nisus' or endeavour. But when the spot was viewed at the distance of ten inches, it must at the first instant have appeared indistinct, and in order to remedy that indistinctness, the eye may be supposed to have extended some force in order to change its conformation so as to suit itself to that distance. If so, this forced conformation will continue while the occasion remains, and no longer. While the eye is viewing the object at ten inches distance, if

> it happens in the least to relax and unbend itself, a sense of indistinctness will immediately begin to arise, which will serve as a monitor to return exactly to the necessary conformation; but the moment the eye ceases to view the object at that distance, it will probably depart from this forced conformation, and return to its natural conformation suited to the object at six inches distance. Therefore when the card is clapped before the eye, as it must necessarily then lose sight of the spot, before it comes to see the spot through the two holes, it may then probably depart from the forced conformation, and return to its natural state, or near it; the consequence of which is, that the rays will now now unite upon the retina, but will therefore exhibit the appearance of two spots.
>
> I might here observe that Mr. de la Hire himself must necessarily admit one alteration in the eye at this instant of time, namely the dilation of the pupil. Why then may not the conformation of the coats and humours as well be supposed to change at the same time."

Thus, Smith stated his opinion with elegance and clarity: that it was not possible to maintain the focus of the eye on the original spot during the time that the card with the two holes was placed in front of it; and that it would no longer be in a position to re-focus on the spot when looking through the two holes, returning, in the instant that the card was placed in front of the eye, to its natural viewing distance. Certainly, my own attempts at this experiment bear out Smith's explanation, since I have been unable to reduce the two images of the spot held close to my eye to one, when viewed through the two holes. (I see two images when the spot is close since I am long-sighted.)

This may be because of the presence of the card, with the blurred image of the two holes dominating the field of view, and making it impossible for the eye to concentrate sufficiently on the image of the spot; this I think was what Smith was implying. However, there may be an alternative explanation which can be best understood by viewing the spot through a single small hole rather than two. If one does this, owing to the stopping-down effect of the hole, the spot appears in focus for all distances, and therefore it is impossible for the eye to focus more clearly that which already appears perfectly clear.

Twenty-one years after Smith's work, the other major book on physiological optics written in the eighteenth century was published, Porterfield's "Treatise on the Eye.....". However. before considering this book, it is desirable to discuss two other works published during the intervening years. Peter van Musschenbroek's "Elements of Natural Philosophy...." and Robert Whytt's Essay on the ".......Motions of Animals".

"The Elements of Natural Philosophy chiefly intended for use of students in Universities"[37a] by Musschenbroek (1692-1761), was obviously a text book, and therefore contained few details of experimental work, but in it the author stated clearly his views on the way in which accommodation was achieved. He held that accommodation was due to the movement of the crystalline lens brought about by the ciliary processes:[37b]

> "The image of external objects is distinctly painted upon a small portion of the retina about the optical axis, but indistinctly in such places that are remote from the axis. Therefore at one view we can see but a small part of an object and all the other parts we can see but imperceptibly and confusedly. If the object be such a distance from the eye that the ray of light emitted from the several points of the object meet again by refraction in as many points on the retina, the crystalline lens of the eye continues in its own place. But if the object approaches nearer to the eye the rays are emitted from it being more diverging and as much refracted as before would not meet upon the retina but behind it. Wherefore the lens by means of the ciliar processe (sic) that contract themselves, is moved farther from the retina, that the rays unite on it. If the object is at a great distance from the eye the rays fall upon it but a little diverging, and being refracted as much as before, meet before they come at the retina. Then the ciliar processes being relaxed, the crystalline lens approaches to the retina so that the image of the object may be painted upon it. Or when the ciliar processes contract by which the crystalline lens is brought nearer to the cornea, does it at the same time become flatter because of the compression of the bag in which it inheres? Though because of its hardness it would oppose such a change."

Since the lens has to become more convex in order to see close objects, any flattening of the lens, such as that suggested above would tend to neutralise the improved close vision obtained by the movement of the lens forward. Musschenbroek, however, suggested that, in becoming flatter, the lens also became more solid by compression. It is therefore possible to infer that the refraction of the lens consequently became greater.

It is interesting to note that Musschenbroek in this section made no effort to put his conclusions into perspective by reference to the work of other scientists. There was also no attempt made to mention other possible causes of accommodation, although at this time there was little agreement among scientists on the method by which the eye focused. These omissions tend to diminish the value of Musschenbroek's book, although he did make one original suggestion, that of the compressibility of the lens.

Robert Whytt (1714-1766) in his clear and logical account of accommodation contained in his "Essay on the Vital and other involuntary Motions of Animals"[38] placed considerable emphasis on the part played by the iris in accommodation. So much so, that initially one tends to infer that he was proposing that this was the only change taking place during accommodation; only when the reader reaches the end of this section of the work does he find a recognition of the part played by the crystalline lens.

Whytt fully appreciated that, were it not for the motion of the pupil, the eye would have been dazzled in bright light and unable to see in dull; but he immediately made another point:[39]

> "Further, as the rays of light coming from the very near objects are much more divergent than those from remote ones, had the pupil been incapable of variation as to its extent, the eye would have been ill fitted for seeing distinctly at different distances; since such objects alone are seen distinctly, whose images are accurately painted on the middle and most sensitive part of the retina."

There is evidence in the last part of this passage that Whytt considered the contraction of the iris to be the only change taking place in the eye when viewing close objects, and this is borne out by the later passage below:[40]

> "The necessity of this contraction of the pupil when we look at near objects in order to render vision more distinct is easily understood; for as in near objects the divergence of the rays is much greater than in distant ones, and as those rays only serve for distinct vision, which do not diverge much from the axis of each pencil, the pupil must be contracted, in order that the useless or disturbing ones may be excluded."

However, as we shall see later, it is incorrect to infer that Whytt was of the opinion that this was the only change that took place during accommodation; he also thought that the crystalline lens moved towards the cornea for close objects.

Whytt next concerned himself with the way in which the iris functioned. He thought that the uvea or iris, was furnished with a double set of muscle fibres whose contraction or relaxation allowed the opening to be augmented or diminished. He said that one set was circular and immediately surrounded the pupil - he called this the "sphincter pupillae"; when it contracted the pupil was lessened. The other set of muscle fibres was radial and these arose from the great circumference of the uvea, where it was attached to the 'circulus albus' or union of the cornea and sclerotica. He thought that this might be called the 'dilator pupillae'. Whytt justified these hypotheses as follows:[41]

> "The circular plane of fibres is so thin and delicate, that some authors seem still to doubt of its existence; but in admitting it we are not only justified by the authority of the best anatomists, but by reason and analogy since the equable and regular contraction of the pupil cannot be conceived, without supposing some such mechanism."

The second part of this argument in favour of his theory cannot be considered very convincing. However, it is the correct one and the circular muscular fibres he mentioned do exist to contract the size of the pupil, although they are not as easily seen as the longitudinal fibres.

Since the longitudinal fibres of the iris were more conspicuous than the circular, as stated above, Whytt thought that they must be stronger, and that for this reason the natural state of the iris was one of dilation. However, in death the iris was contracted, since, he said, the longitudinal fibres lost their contractile power. The variation in size of the pupil was thought by some of Whytt's contemporaries to be due to the variation in the intensity of the light falling on the iris; he denied this, saying that it was due to the variation in light falling on the retina. He cited the case of cataract sufferers whose pupils lose a great deal of their power of contraction. This was said to be due to a disease of the iris, but Whytt denied this, preferring the explanation that it was due to the cataract's limiting the amount of light falling on the retina. In support of his arguments he stated that the nerves of the iris had no connection with the optic nerve, and also that, if only one eye was affected by a cataract, exposure of the good eye to bright light led to a contraction of the iris of the eye with the cataract, showing that the iris was not diseased.

Developing this idea of sympathetic contraction of irises further, Whytt noted the sympathetic movement between pupils; if one eye was closed, then the other iris opened; if one eye only was exposed to light, the irises of both contracted, though the iris of the eye not exposed to bright light contracted less than the other.

Whytt said that since there was no connection between irises, then the agreement then must come from a common principle in the brain.

Other simple but effective experiments were also carried out by Whytt. He noted that the pupil of an eye in bright light, which was thus already contracted, contracted even more when it viewed a close object. As a book was brought closer to the eye, then the iris was seen to grow successively smaller. He also looked at a candle at a distance of 2 - 3 feet and then at a quill at a distance of 5 - 6 inches and noticed a contraction, even though the light falling on the eye from the candle was unchanged. Taking the problem a stage further, he actually diminished the light entering the eye when viewing a close object and noted a contraction of the pupil. To do this Whytt viewed a light-coloured object at a distance of 3 - 4 feet with his back to the source of light. He then looked at a dark-coloured object at 1 foot distance; this reflected less light into the eye, but nevertheless the pupil of the eye still contracted. In this experiment it could well appear that Whytt was implying that the prime cause of contraction of the pupil was to view close objects; however, he later made it clear that his opinion was that the only cause of contraction was bright light, and that the contraction which took place when close objects were viewed (an effect which had been known for many years) was just a special case of this effect. He made this point as follows:[42]

"In viewing distant objects, the pupil is not widened by any effort of the mind, but its

> size is entirely determined by the quantity of light applied to the eye, which, as it is caeteris paribus, fainter in distant than in near objects, must occasion a small degree of dilation in the pupil."

In this section of his book, Whytt was not contradicting what he had previously stated; he was, in fact, introducing a new element into the cause of the contraction of the iris. He maintained that the contraction that took place in order to view close objects was mainly as a result of an action of will, while the contraction caused by bright light was reflex, though, of course, he did not use this term.

He also argued that in faint light the image on the retina would not be larger, owing to the enlarged pupil. This had been put forward as an explanation for the apparent enlargement of the sun near the horizon. Whytt thought that this was contrary to the laws of nature, and would also mean that all objects would appear larger in dull light, and that this was contrary to experience.

It is obvious that the motion of the iris was one which Whytt found of great interest, since he devoted the greater part of his work on the eye to it. His only mention of the part the crystalline played in accommodation occurs at the very end of the section:[43]

> "In looking at near objects, the pupil is lessened, at the same time that the crystalline humour is brought forward

> towards the cornea, by the contraction of the ciliary process; but when we contemplate distant ones, the contraction of the ciliary processes and orbicular muscles of the uvea ceasing, the crystalline returns to its situation and the pupil to that size to which it is fixed by the quantity of light applied to the eye. These motions though both voluntary, yet come to be so connected by habit that we cannot perform them separately."

Thus, it can be seen that Whytt favoured the movement of the lens towards the cornea by ciliary processes as a cause of accommodation. It will be noticed that he did not call them muscles. He made no other mention of them, and did not comment upon the obvious difficulty of considering movement of the crystalline without the involvement of a muscle. Smith, as mentioned previously, had shown that the forward movement of the lens alone was not sufficient to account for the range of accommodation possessed by the normal eye. Whytt made no reference to Smith, but it can be assumed that he placed considerable emphasis on the part played by the closing of the iris as an aid to focusing close objects. Therefore his theory could have been considered tenable by his contemporaries, since it could be considered that the considerable emphasis he placed upon the contraction of the iris for close objects together with the forward movement of the lens would provide sufficient range of accommodation for the eye. It must be remembered that the rejection by Smith of the forward movement of the lens as the sole basis for accommodation, depended upon the comparatively minor importance he gave to the contraction of the iris as an aid to focusing. This would require a correspondingly greater movement of the lens to enable a full range of accommodation to take place

It is perhaps surprising that only twenty four years after the publication of Smith's and Jurin's major contributions in the field of physiological optics, another important work was published. In 1759 William Porterfield published "A Treatise on the Eye";[44] this was a long and somewhat repetitious work, but it did attempt to cover all the aspects of current knowledge of the eye. In some cases the material it contains can be considered to be contemporary with that of Smith and Jurin, since it had already been published in journals,[45] and Jurin certainly referred to Porterfield's views.

In his study of accommodation, Porterfield chose a popular starting point, the work of De la Hire. He was, however, in no doubt that some mechanism was required in order that the eye could focus on objects at different distances:[46]

> 'From what has been said in the preceding chapter (of Porterfield's book) concerning the manner of vision and the use of the several humours of the eye in refracting the rays, so as to make pictures of objects distinct, it follows that in order to see objects at different distances distinctly, it is necessary that there should be a change in the eye lest the place in which the picture of the object is exact should fall short of or beyond the retina, and so cause the vision to be confused."

De la Hire's experiment with the holes in a card was described in detail, together with the conclusion drawn from it[47] and summarised by Porterfield as follows:

> "For suppose that I see an object distinctly at a foot distance and at the same time it appears single when viewed through the perforated card; if, to see the same object at four feet distance, it were requisite that the eye changed its conformation, then he (De la Hire) concludes it would do so, when the object is viewed at that distance through the card; which does not happen, as is evident from it being multiplied."

There had been equally good summaries of De la Hire's work before, but Porterfield was obviously impressed by the calibre of the experiment, though, as we shall see shortly, not convinced. He expressed his surprise that the theory had had so little acceptance:[48]

> "It must indeed be acknowledged that at first view the argument seems to go a great way towards a full demonstration of what he alleges; nor so far as I know, has anything been yet offered by any author, whether Physician, Anatomist or Optician, that can in the least weaken or disprove it; and yet all of them, excepting Maitre-Jean and some few others, continue to teach, that our eyes change their conformation according to the distance of objects, without so much as once taking note of De la Hire's reasoning or attempting an answer."

One might fairly gather from this that Porterfield had not read Smith's 'Opticks' or the essay by Jurin contained in it, since both of these authors deal fully with De la Hire's work, and give full details of their reasons for not accepting his hypothesis. Later in his book, however, Porterfield mentioned both these works, and it is clear that he was familiar with them; it is therefore difficult to see the justification for his above comment, that De la Hire's work had been largely ignored, without having his theories refuted.

Porterfield then gave his own view why the results in the hole-in-card experiment did not disprove the need for some additional means of accommodation, apart from the closing of the iris: [48]

> "In answer to this argument of De la Hire, I once suspected, that, when an object is viewed through a perforated card, the Eye by endeavouring to see the card, adapted itself to as near a distance as it could, and, by continuing in that state, occasioned the object to appear multiplied when at a greater or lesser distance, than to which the eye is then accommodated."

He thus shared the view with Smith that the presence of the card in front of the eye prevented the normal process of accommodation from taking place; although Smith emphasised that it was the movement of the card which interfered with the act of accommodation. It is also interesting to note Porterfield's use of the word 'accommodated'. As far as I have been able to ascertain, he was the second author to use the word in this particular context, the first having been Pemberton.

De la Hire's contention that rays of light did not necessarily have to come to an exact focus on the retina in order for the eye to see them clearly, was next considered by Porterfield. It will be remembered that this was a view shared by Jurin, who was at pains to produce a theory which reduced the stress caused within the eye by the act of accommodation. Porterfield agreed that the eye had some latitude for seeing objects clearly without changing its conformation, but he did not agree that this necessarily meant that it did not change its conformation in order to see objects which were much removed from the place where they appeared most distinctly. He cited in favour of this contention experiments with convex lenses producing images on screens, saying that in order to focus images of objects at different distances, either the lens had to be moved, or a new lens used. The fact that we can focus clearly on only one object at a time, while the other, at a different distance becomes blurred, was also mentioned.[49]

> "This in a few words is the sum of what De la Hire advances concerning our seeing objects distinctly and at different distances, without having recourse to any change in our eyes. And indeed it cannot be but the eye has some latitude of seeing objects distinctly without changing its conformation, tho' they be a little further or nearer to the eye than what is necessary for collecting the rays that come from the several points of the object in so many precise points on the retina; and that because when the object is not far removed from that place at which the rays coming from the object meet again at the retina, the image therof will be pretty distinct, and therefore will not occasion any sensible confusion of sight. But it does not from thence follow that our eyes do not change their conformation when objects are much removed from that place where they appear most distinctly."

Porterfield then put forward two axioms, which he used to determine whether the eye was focused beyond, or nearer than a given object.

Axiom I

> "When an object seen with both eyes appears double, by reason that its distance is less than that to which the eyes are directed - upon covering either of the eyes the appearance that is on the contrary side will vanish; and if it appear double, because its distance is greater than that to which the eyes are directed, upon covering either of the eyes, the appearance on the same side will vanish."

Plate 4 Vol. I from Porterfield's

"A Treatise on the Eye"

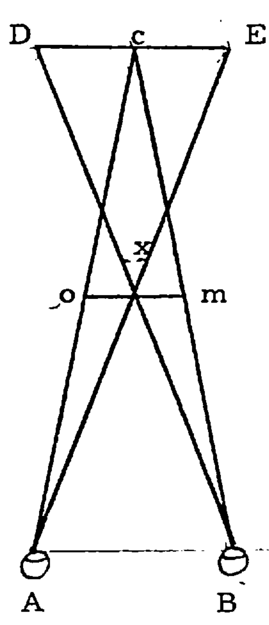


Fig. 32.

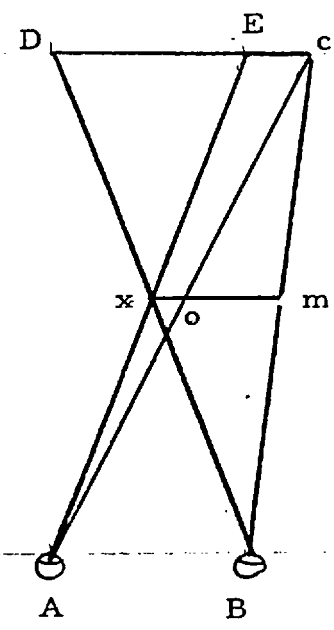


Fig. . 33.

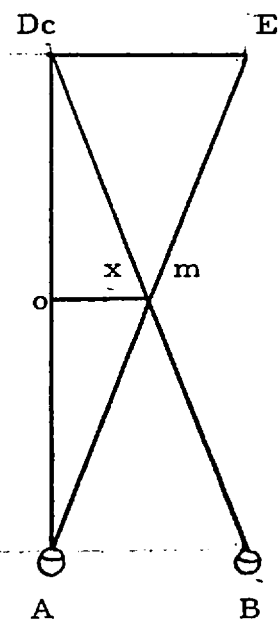


Fig. 34.

To explain Axiom I, fig. 32 was used:

The two eyes A and B were directed to c; x was the object, at a smaller distance than c. The object x would be seen by the right eye in the direction BxD; and by the left eye in the direction AxE. Therefore, two images of x would be seen. If the left eye was covered then the right hand image vanished, and if the right eye was covered, then the left image vanished. In a similar way, if both eyes were directed to x, in figs. 33 and 34, the object c would be seen by the right eye in the direction Bmc, and by the left eye in the direction Aoc. Therefore, two images of c would be seen, say at o and m. If the right eye was covered, then the right hand image would vanish, and if the left eye was covered, the left hand image would vanish.

Axiom II

> "When an object appears double from its being seen with one eye thro' two small holes made in a card or other thin opaque body, if its distance be greater than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the same side will be made to vanish. If its distance be less than that to which the eye is accommodated, upon covering either of the holes, the appearance that is on the contrary side will be made to vanish."

This second axiom is, of course, an ingenious extension from De la Hire's experiment.

Plate 5 Vol. I from Porterfield's
'A Treatise on the Eye'

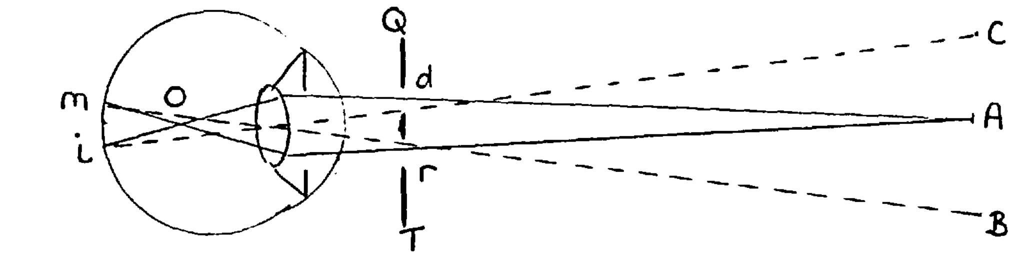


Fig. 35.

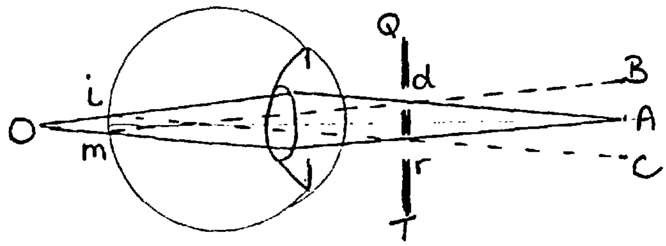


Fig. 36.

The explanation of Axiom II is as follows:

D and r are holes in the card QT. A is a small body at a greater distance than that to which the eye is accommodated in fig. 35, and at a lesser distance in fig. 36. Rays of light from A such as Ad, and Ar, will therefore not meet at a point on the retina after refraction by the eye, but will be brought to a focus at some other point O, in front of the retina in fig. 35, and to a virtual focus behind the retina in fig. 36.

These rays will cut the retina at i and m, and thus two images will be formed at these points on the retina, and the eye will see two objects apparently at B and C.

Porterfield obtained the position of B and C by drawing Ci and Bm perpendicular to the retina. If the hole d was covered, then i no longer existed and C vanished; and if r was covered, m no longer existed and B vanished. In this way his axiom was verified.

These axioms were used by Porterfield to see whether his eye was focused nearer or beyond a given object. In simple, but ingenious experiments, which involved viewing an object through slits and varying its distance from the eye, he discovered that he was able to focus closer than 9 inches, i.e. when two images were formed with the object at 9 ins., on covering one slit, the image on the same side vanished; but he was not able to focus as close as 5 inches, i.e. when a slit was covered with the object at 5 inches, the image on the other side vanished. He thought that the closest he could see an object clearly was about 7 inches.

The various means by which the act of accommodation could be carried out were considered next. Porterfield thought that the oblique exterior muscles to the eye had an incorrect disposition to change the shape of the whole eye, so that the eye could be elongated to allow close objects to be brought to a focus. He also felt that accommodation should be carried out in the same way in all animals, and pointed out that in some animals the disposition of the exterior muscles was such that they would be unable to lengthen the eye, e.g. such as both being on the same side of the eye. He thought that the four "streight" (sic) muscles acting together might pull the eye back into its socket, and so push the back of the eye forward. Presumably he considered that this would happen when the eye viewed a distant object. This method was dismissed since he said that any pressure on the eye caused confusion of the image, and therefore he could not imagine any method which pressed the eye into a different shape producing a clearer image.

Porterfield finally deduced that it was some change in the crystalline which was responsible for accommodation, by considering the case of cataract sufferers:[50]

> "as a cataract is not a Philm swimming in the aqueous humour, as has generally been believed, till of late, but an opacity of the crystalline itself, and as the couching of a cataract consists of introducing a needle into the eye and turning down the opaque humour below the pupil, it is evident that the crystalline cannot be displaced and turned down to the under part of the eye but the vitreous humour must, in giving way to it, be pushed into its place; but because its density is less than that of the crystalline, it follows that the rays of light will be less refracted, and therefore will not meet at a point on the retina, but at some distance behind it; from when the sight must be confused, unless a convex glass of a due degree of convexity be brought to assistance. . . . nor has the efflux of the aqueous humour any concern in this phenomenon, seeing it is again restored, as was known to Galen, as before observed: but this is not all that happens after the depression of the cataract; for it was also observed that the same lens was not equally useful for seeing all objects distinctly

> but that he was obliged for seeing them distinctly to use glasses of different degrees of convexity, still the more convex the nearer the object."

He thus made the valid deduction that since, when the lens was removed we could not see clearly, and further, that since we needed lenses of increasing convexity to focus as the object was closer to the eye, then the crystalline lens was responsible for the act of accommodation. He thought that if accommodation depended upon exterior muscles as had been previously postulated, then even after couching, the eye would have retained some degree of accommodating power, and that a single convex lens would be all that was necessary to restore a full range of accommodation to the eye.[51]

> "Seeing that nothing happens in the eye in couching the cataract, but that the crystalline is depressed, it follows that the change made in our eyes according to the distance of objects must be attributed to this humour."

Porterfield now turned to which changes could be made to the crystalline lens in order that it could be responsible for accommodating power of the eye. He thought that there could be two opinions:-

a. A change in convexity of the lens.

b. A change in position of the lens.

In the first case, the 'ligamentum ciliare' would have to make the lens flatter for distant objects, and on relaxing it would allow the lens to become more convex owing to its natural elasticity. Porterfield thought that this could be the reason why the outer part of the lens was easily flexible.

He was, however, of the opinion that the situation of the ligamentum ciliare, which was not in the same plane as the crystalline could therefore not make it flatter. In figure 38 overleaf taken from Porterfield's book, ao represent the ciliary ligament. He said that in order to draw out the capsule and so make it thinner it should be pulled in the directions ad, ad; since this could not be done by the ciliary ligament, he said it "can never by its contraction change the figure of the crystalline."

Returning to the varying hardness of the crystalline lens, he said that the softer exterior, with the more solid centre, was not necessarily to allow its shape to change. It could instead allow the more oblique rays striking the edge of the lens to be focused upon the retina, and not be over-deviated as might happen if the density extended to the edge. He also briefly considered the possibility that the lens was itself muscular, so that it was capable of changing its own shape, but said that no muscular fibres had been seen.

Porterfield said that Leeuwenhoek had shown that the lens was made up of scales or laminae, with up to 2, 000 layers in one crystalline. Each lamina was made up of a single fibre. Porterfield thought that this disposition was "ill qualified for changing the figure of the crystalline." He was also aware that the crystalline had no communication with the rest of the body, but it was kept in place by a capsule, and when this was opened the lens just slipped out. It had no continuity with any fibre,

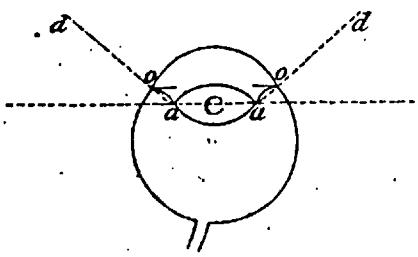


Figure 38 Plate V from William Porterfield's "A Treatise on the Eye."

blood vessel or nerve. He accurately summarised its existence as follows, saying that it had "a kind of vegetative life..... and draws its nourishment from the water in which it fluctuates."

Finally, Porterfield outlined his own theory of accommodation. He thought that it was accomplished by a movement of the lens, and that this was achieved by the ciliary ligament, which he maintained were arranged as in the previous diagram, and could well be thought to pull the lens forward when they contracted. In this way the eye could adjust to view close objects. He said:[52]

> "Now the Ligamentum Ciliare is an organ whose structure and disposition excellently qualify it for changing the situation of the crystalline, and removing it to a greater distance from the retina, when objects are too near us; when it contracts it will not only draw the crystalline forwards, but it will also compress the vitreous humour lying behind it; by which compression it must press upon the crystalline and push it forwards further from the retina....... (and it) must at the same time press the aqueous humour against the cornea; by which means this membrane which is flexible and yielding will be rendered more convex for enabling us to still the better see near objects distinctly."

This explanation is fairly straightforward, and had been put forward earlier. The concept of the lens, as it moved forwards, compressing the aqueous humour, and making the cornea more convex, thus assisting close vision, can be easily appreciated. It is less certain what Porterfield meant, when he considered the ciliary ligaments compressing the vitreous humour, and thus further assisting the forward movement of the lens. If one looks again at his diagram, however, it is possible to envisage the pull of the ligaments at the points oo, pulling in the sclerotic so that the vitreous humour behind the lens is compressed, as was said. A side effect of this would be to make the cornea more convex also, thus assisting close vision, and it is surprising that Porterfield did not mention this additional method of increasing its convexity.

However, the idea of a pull at oo from the ciliary ligaments compressing the eyeball is one which is less convincing if one considers the eyeball in three dimensions, and not merely a two-dimensional cross section. This difficulty has been dealt with earlier on Page 119 where it was shown that such a change involved the surface of the eyeball actually compressing itself like an elastic surface, and the scope for this must be very small. After this criticism, however, it is fair to say that this is only a peripheral aspect of Porterfield's hypothesis, which was soundly based upon the movement of the lens.

Porterfield went on to mention that computations had shown that the motion of the lens, which he had postulated, was insufficient to explain the range of accommodation of the eye. With charming effrontery, however, he queries the accuracy of these calculations, or even whether it was possible to measure with sufficient accuracy the various values upon which such computations were based.

It had been said, notably by De la Hire, that the ciliary ligaments were not muscles, since all muscles possessed a red colour. Porterfield pointed out that this rule did not hold universally, since the muscles of the stomach, and more relevantly, those of the iris were not red.

In Volume II of his work, Porterfield went on to discuss defects of vision. He defined short sight as follows:[52a]

> "By myopes I understand such as have the cornea and crystalline or either of them too convex, or that have the distance betwixt the retina and the crystalline too great. Thus a distinct picture of the objects at an ordinary distance will fall before the retina. In order to see distinctly they are obliged to bring the objects very nigh to their eyes, by which means the rays that are now more diverging, are made to converge and meet at the retina where a distinct picture will be made."

Thus he had correctly interpreted short sight to be due to a defect in the overall refraction of the eye, and he was aware that it was not due to inadequate powers of accommodation:[53]

> "The cause of shortsightedness is not a want of power to vary the conformation of the eye but that the whole quantity of refractions is always too great for the distance of the retina from the cornea."

Any theory which attributed accommodation to a forward movement of the lens, due to a contraction of the ciliary ligaments, had the advantage that it could hold, correctly, that the eye was at rest when it was viewing distant objects. Thus Porterfield was able to say:[53]

> "The natural state of the ligamentum ciliare, like that of all other muscles, is a state of relaxation; thus it is easy to see that the crystalline must be as near the retina as possible, whence it follows that the eye is naturally disposed to see distinctly only distant objects."

The normally-sighted person would tend to this opinion from everyday experience, finding that eyes tended to tire more easily in close work such as reading than in viewing distant objects. Any theory which held that accommodation was caused by a change of shape of the lens would normally attribute distant vision to a flattening of the lens by a pulling of the ciliary muscles on the capsule; close vision would occur when the muscles relaxed, and the lens became more convex under the action of its own elasticity. Thus the eye would be at rest for close objects, a conclusion which would be difficult to reconcile with normal experience.

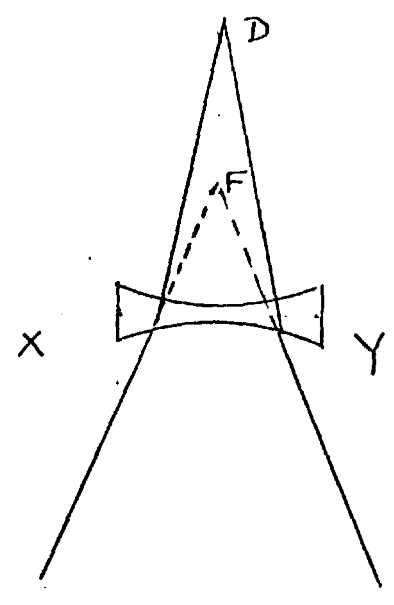
Porterfield made this point when he said that his theory fitted in with common experience, the eyes becoming tired with continued close work, owing to the constant exertion of the ciliary ligaments in giving the eyes the necessary conformation, and also to the external muscles in giving the eyes the necessary angle between the optic axes, in order to view something close.

The effect of increasing age on accommodating power was dealt with next. Porterfield attributed the loss of accommodating power to the increasing rigidity of the ciliary ligaments. He also made the almost universal mistake of believing that repeated close work contributed to short sight, and distant work to long sight:[54]

> "When this ligament (ligamentum ciliare) has become rigid and stiff the crystalline will have very little motion, whence the limits of distinct vision will be very narrow. Thus it is with those who are employed at subtle work such as engravers, jewellers, watchmakers, etc., who are very apt to become short-sighted from their constant application to small objects, and they are constantly obliged by the contraction of this ligament to bring the crystalline as near to the uvea as possible. This ligament by its constant contraction must at last shrink and have its fibres shortened which will keep the crystalline in that situation by which the eye is disqualified for seeing distant objects distinctly."

He held that the converse was true for those who were accustomed to look at distant objects: hunters, sailors, etc.

Using the diagram below, Porterfield gave a formula by which corrective lenses might be prescribed for a purblind (myopic) person.



He supposed that F was the distance at which the myopic could see clearly without glasses. D was the distance of distinct vision with glasses.

He then proposed the following formula (which is obviously a misprint) where R is the radius of the lens which would give corrected vision:

$$F = RD/RxD$$

By considering a modern optical formula, it is possible to arrive at an equation which would provide satisfactory corrective lenses, and bears a strong similarity to the above formula.

Considering the above diagram, we have the situation of a person who is myopic and can see without glasses an object at B, and who with the aid of the concave lens CD is enabled now to see clearly an object at A.

This is because the lens produces a virtual image at B of an object at A. Considering the distances of A and B from the lens to be approximately the same as from the eye, we have:-

Distance of B = F
Distance of A = D

Therefore $-1/F + 1/D = -1/f$ (virtual distances counting -ve)

but $1/f = (\mu-1)(1/r_1 + 1/r_2)$

considering the two radii to be equal, we have $-1/F+1/D = (\mu-1)(1/R+1/R)$

if $\mu = 1.5$

$= .5(\;-2/R)$ R concave therefore -ve

$$-1/F+1/D = -1/R$$

from which $R = \dfrac{DF}{D-F}$

re-arranging $F = \dfrac{RD}{D+R}$ which could be imagined to be the equation intended by Porterfield.

Considering an example of the use of this formula, we can take the case of a short-sighted person who can see clearly at a distance of 3 inches, and requires a lens to enable him to see at a distance of 18 inches. The radius of each surface of the lens is given by

$$R = \frac{3 \times 18}{18 - 3} = 3.6 \text{ inches.}$$

Porterfield commented upon the habit of some short-sighted people of screwing up their eyes so as to leave only a short gap between the eyelids. In this way, owing to the effect of a very small aperture they could see more clearly. He said that it was from this closing of the eyelids that short-sighted people were anciently called "myopese".

The word μύωψ (MYOPS) is a compound of (1) μυ which implies any kind of closing, tightening, etc., and (2) ωψ an eye.

It was used by Aristotle (and probably no one else) to mean "short-sighted". It seems a reasonable deduction that he coined this word because some short-sighted people have the habit of screwing up their eyes, almost closing them, so as to focus at a greater distance.

There are many words which apparently come from the same root μυ (MY-) in its idea of anything hidden, closed up, tightened up, such as:-

myrios	countless
mysticos	mystic
mykès	mushroom
myelos	bone marrow
myle	mill (originally the part between the mill stones)
mycteres	nostrils
muchos	a hollow
mythos	myth
myrmex	ant

Latin	mus	mouse
	mutus	dumb
	murmurare	this may be more than merely onomatopæic
English	mussel, mew	"and therefore hath he closely mew'd her up"
	mycology	study of fungi

Porterfield repeated a widely-held belief that short-sight tends to lessen with age, though mentioning that some scientists held that this was not so, mentioning that Smith was one of these.

He defined weak or "presbytical" sight as that caused by the rays of light coming to a focus behind the retina, owing to the cornea or crystalline being too flat. What he was of course describing here was normal long sight, or hypermetropia; presbyopia being the loss of accommodating power with age, which causes the near point to move away from the eye, thus making it difficult to focus on close objects.

"A Treatise on the Eye" was a somewhat repetitious book written in a style which does not perhaps impress the contemporary reader as much as the works of Smith and Jurin, written as they were in more scientific language. Nevertheless, it contained much which was of value, and in particular the axioms which it contained enabled Priestley, who thought highly of Porterfield's work, to carry out more sophisticated experiments and to prove for the first time the existence of the accommodating power of the eye.

The next major work on vision was in fact by Priestley and contained his development of Porterfield's experiments. "The History and Present State of Discoveries Relating to Vision Light and Colours" was published in 1772. In spite of the comprehensive way in which the subject was covered, it is difficult not to be slightly disappointed by the work. Perhaps it is that one expects too much from such a distinguished author. Indeed in the section devoted to accommodation[56] the only criticism that can be fairly levelled is that he made no attempt to commit himself to a theory, but merely summarised the views of others.

It is difficult to form an opinion from reading this part of the book whether his lack of commitment stemmed from modesty or uncertainty. He did, however, say at the outset that there was some change taking place in the eye to allow it to focus on objects at different distances, and thus one can assume that he dismissed mechanism using the external muscles. He also stated that scientists by this time definitely believed that the eye possessed some power by which its form could be altered so that it could focus on objects at different distances.

In his opening paragraph on the subject of accommodation Priestley said:[56]

> "That we are capable of viewing objects with nearly equal distinctness, though they are placed at considerably different distances, is evident; but the alteration which takes place in the eye for this purpose or the mechanism by which this effect is produced, is not easily ascertained."

Perhaps within this statement we might read the reason that Priestley put forward no theory of his own. One can fairly say that all the likely means of achieving accommodation had been put forward by earlier writers: from the complete denial of accommodating power, apart from that of the iris, to changing the shape of the eye, or lens, or position of the lens, or combinations of more than one effect. It might have been that Priestley was one of the first to appreciate the true depth of the difficulties involved in putting forward a hypothesis which actually fitted all the known conditions, properties, and constants of the eye. His position would be far more difficult than that of a scientist living one hundred years earlier, whose hypothesis would be difficult to check, since so little was known of the properties of the eye.

Any theory put forward by Priestley, or a contemporary, would have to be evaluated in the light of a considerable body of knowledge of the eye, and it may have been that he was not as willing as Porterfield to dismiss the results of other scientists' experiments when they disagreed with his hypothesis.

Priestley started his historical summary of accommodation, "this curious subject" as he called it, with the view of Kepler, that the ciliary processes changed the shape of the eye in order to focus at different distances. Descartes was said to favour a change in shape of the crystalline by the same processes. Not surprisingly, De la Hire's work was discussed at length, and firmly rejected. In particular, the action of the iris in rendering objects more distinct by contracting was dealt with in detail. Priestley correctly maintained that the contraction of the iris made all objects clearer, whether near or distant. However, he said that distant objects tended to be less bright so that the iris had to be dilated, and thus could not be the means by which they were clearly focused. His thinking here was perhaps rather superficial. He said:[57]

> "It is certain that the pupil is not contracted, but dilated, for the purpose of viewing objects that are very remote. Indeed, without a dilation of the pupil in those circumstances, a sufficient quantity of rays could not be admitted. When objects are near, and well illuminated, the contraction of the pupil may be sufficient for viewing them distinctly, but there must be some other provision than this for remedying the indistinctness of objects that are very remote."

Thus, a quite unnecessary complication had been introduced. De la Hire's theory maintained that the contraction of the iris helped with distinct vision of close objects only. The criticism that was commonly levelled at it was that the contraction of the iris alone was not sufficient to allow it to focus close objects sufficiently well, especially in dull light. There was no reason why the eye should not be focused naturally for distant objects, and require merely some mechanism for close-up focusing; this, in fact, is what happens. Therefore Priestley's criticism, that the natural dilation of the iris when a distant object was viewed, created the need for some other method of focusing, was not well taken.

Priestley was considerably influenced by Porterfield's work on accommodation. He called it "the most satisfactory discussion on this subject"[57] and went into considerable detail in explaining Porterfield's two axioms, and his experiments with two holes, by which he established where the eye was focused. Priestley made the pertinent point that because the slits in the experiments were closer together than the diameter of the iris, the iris could not take any part in the focusing of the eye during the experiment. Since the experiments proceeded to show that the eye could focus at different distances, then Priestley said that Porterfield had experimentally proved the power of accommodation of the eye. Thus Priestley drew more important conclusions from the experiment than did Porterfield, who merely used them to establish where the eye was focused.

The experiments which were described in detail were not those of Porterfield, but modifications by a Dr. Motte at Dantzig, whose original account I have not been able to locate, since no reference was given by Priestley. They were an ingenious adaptation of Porterfield's original experiments, a combination of both his axioms into one experiment.. The following explanation refers to the diagram overleaf, which is a copy of fig. 149 from Priestley's book. The eye B had a small piece of tin plate in front which had two slits, whose distance apart did not exceed the diameter of the pupil.

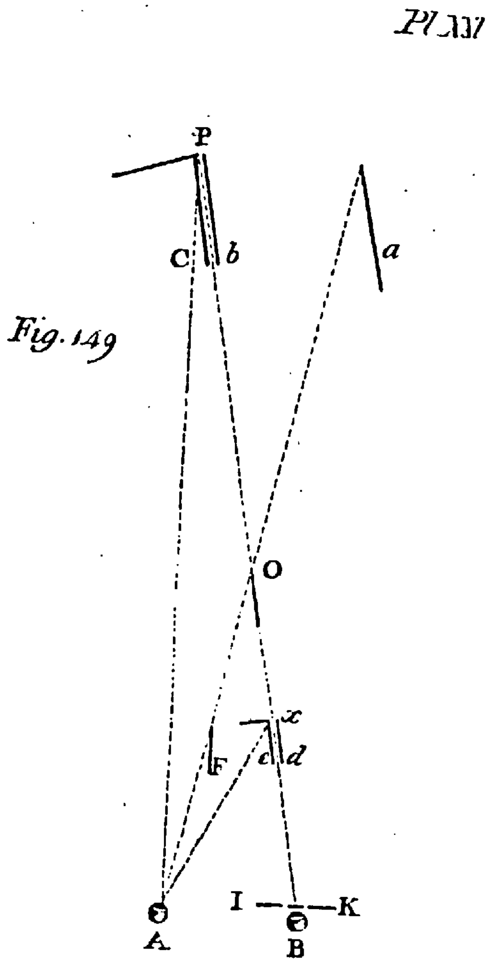


Figure 149 from Joseph Priestley's "The History and and the Present State of Discoveries Relating to Vision, Light and Colours."

Whereas Porterfield used holes in a card, Motte used slits, which gave brighter images. The object O which was also vertical, was viewed through the slits while the eye A was shut. It was at such a distance as to appear single. Now both eyes were opened and a more distant object such as P was viewed. Three images of O were seen, a, b and C. On shutting A, the image a vanished; thus from the first axiom, P was beyond O (since the image on the opposite side to the closed eye vanished.) On covering one of the slits in front of B the image b or C, whichever was on the contrary side, vanished. Thus the eye was focused beyond O. When a point such as x was now viewed, images of O at d, e and F were seen. When A was closed, or a slit covered, the image on the same side now vanished; therefore, the eye was now focused at a less distance than O. In this way it was shown that the eyes did possess the power of accommodation which, since the iris was not involved, was independent of its action. The explanation of these experiments was not as clear as that given by Porterfield, details of which have been given earlier on Pages 158-161. The conclusion drawn, however, was much more far-reaching, and one can say that the accommodating power of the eye had been experimentally established for the first time. Henceforward, discussion should centre upon how the eye focused, not upon whether it had the power to focus.

As if to accept this as a challenge, the century finished with a remarkable flourish of experimental activity by several British scientists, designed to establish the true cause of accommodating power. Young, Hunter, Ramsden, Hosack and Home were all involved in separate experiments and hypotheses within the space of eight years. It is perhaps best to discuss these events chronologically, so that one can appreciate the development of hypotheses and experiment and the swings of opinion from one theory to another, during this time.

The first significant paper in this eight-year debate was written by Thomas Young and read to the Royal Society on 30th May 1793.[58] Young briefly dealt with the various earlier hypotheses, starting with the work of Kepler, and giving his reasons for rejecting them. He said that he was led to the conclusion that accommodation was achieved by means of the crystalline lens by reading accounts in Porterfield and others that the removal of the lens-"couching" for cataract-also removed the power of accommodation. He thought that in order to change the focus, the crystalline must change its shape, becoming more convex for close objects. This change in shape was, in his opinion, achieved by a muscularity possessed by the capsule containing the lens.[59]

> "I had concluded that the rays of light, emitted by objects at a small distance, could only be brought to foci on the retina by a nearer approach of the crystalline to a spherical form; and I could imagine no other power capable of producing this change than a muscularity of a part or the whole, of its capsule."

Young, however, went quickly on to recount that he had found evidence in an ox's eye that the muscularity lay within the crystalline lens itself, and not within the capsule. On turning the lens of an ox out of its capsule, he discovered, with the aid of a magnifying glass, a series of fibres, which he called tendons. The disposition of these fibres throughout the lens led him to conclude that they were muscular in nature.[59] "Such an arrangement of fibres can be accounted for on no other supposition than that of muscularity." Young had here ventured into a field which had been thoroughly investigated by many experimenters and had come to a startling conclusion, apparently upon the evidence of one experiment. His next conclusion is perhaps even more rash, since he mentioned that the lens was attached to the capsule by 'vessels', the nature of which he did not specify, and by 'nerves':[59a]

> "This mass is enclosed in a strong membrane capsule, to which it is loosely connected by minute vessels and nerves; and the connection is more observable near its greatest circumference."

It is therefore perhaps not surprising that some years later, after many ingenious and exacting experiments, Young seemed far less certain about the cause of the change in shape of the lens during accommodation. In fact it was his inability to verify the existence of the tendons in the lens by later and more extensive experiments, which led him to abandon his theory of lens muscularity: [59a]

However, for the moment, Young continued to develop his early theory of muscularity, with an explanation of the way in which the change in shape was achieved.

> "I conceive, therefore, that when the will is exerted to view an object at a small distance, the influence of the mind is conveyed through the lenticular ganglion, formed from the branches of the third and fifth pair of nerves, by the filaments perforating the sclerotica, to the orbiculus ciliaris, which may be considered as an annular plexus of nerves and vessels; and thence by the ciliary processes to the muscle of the crystalline, which by the contraction of its fibres, becomes more convex, and collects the diverging rays to a focus on the retina."

He maintained that a contraction of the fibres in the lens would produce a more spherical shape, since the minimum surface area for a given volume is, in fact, a sphere.

In support of his hypothesis of crystalline muscularity Young quoted the observations of Leeuwenhoek, who had come to a similar conclusion, though with the muscle having a different conformation within the crystalline lens. Young also pointed out that he himself had observed only the crystallines of oxen and sheep, which, however, agreed very closely with the descriptions given by Leeuwenhoek of the crystallines of a number of other animals. He thought that land animals probably had a common method of accommodation. Fish, with their almost spherical crystalline lenses, must, he thought, have another method of accommodation.

Young's hypothesis was further explored by John Hunter (1728-1793) and Everard Home(1756-1832). In fact Hunter claimed that the discovery of the muscularity of the crystalline was his.[60] However, he was unable to develop his work since he died before he was able to deliver the Croonian Lecture to the Royal Society on this subject. The preparations he had made for this lecture were used in the lecture given to the Society by Everard Home in November 1793,[61] who said that for several years Hunter had had the idea that the crystalline humour adapted the eye to see at different distances by its own internal actions. He had observed the 'taenia hydatigena' in a living animal and was surprised to see the quantity of contraction that took place in a membrane which was devoid of muscular fibre, and he had made use of this fact in developing his ideas based upon his observations of the structure of the crystalline humour. From his dissection of the eye of a cuttle fish, he gathered that the exterior parts of the crystalline humour were fibrous and composed of laminae, whereas the central parts were transparent, without any visible laminae. Although the crystallines of other animals did not show this fibrous appearance so clearly, Hunter assumed that they were similar to the cuttle fish, and therefore that all crystallines were made up of fibrous laminae on the outside, while the interiors were clear. This topic was also considered in May 1794 by David Hosack[62] who reviewed the action of the pupil, but in the main produced a critical study of Young's work, which has just been discussed. He thought that the eye must have additional means of accommodation other than the contraction of the iris, since it was clear that the size of the pupil was mainly governed by the intensity of light falling upon the eye. Considering Young's hypothesis that the lens was muscular, Hosack combined Young's figures which required 6 muscles in each lamina of the lens, with those of Leeuwenhoek, that there were nearly 2, 000 laminae in the lens and found the resulting figure, that there were nearly 12, 000 muscles in the lens of the eye, incomprehensible. He also failed to find any evidence of muscles in the lens, either when freshly dissected, or dried. Disagreeing with the view of Young and Porterfield, he said that it was the commonly held view that, after an operation for cataract, the eye still possessed accommodating power. However, one comes to question the rigour of Hosack's reasoning on this matter, when he said:[63]

> "Besides if the other powers of the eye are insufficient to compensate for the loss of this dense medium, the lens, a glass of the same shape answers this purpose, and which certainly does not act by changing its figure."

He admitted that the vision in this case was not as perfect as before, but concluded that the crystalline lens was not as necessary for vision as had been represented, especially in view of the fact that when it was removed, its place was occupied by vitreous humour whose refractive index was nearly the same as that of the lens. This reasoning is difficult to follow if one considers the extremely thick lenses which are required to restore the sight of a person who has had an operation for a cataract. The following statement which he made in support of the crystalline hardly does justice to such a vital and unique part of the eye:[63]

> "At the same time we cannot suppose that the lens is an unnecessary organ in the eye, for nature produces nothing in vain , but that it is not of that indispensable importance writers in optics have taught us to believe."

Considering the ciliary processes, perhaps surprisingly he thought them to be muscular, but assumed that they could not be involved in accommodation since in "couching" they were destroyed, and in his incorrect view, the power of accommodation was still possessed after the removal of the lens. Finally, he stated his own view; that accommodation was caused by the external muscles, which act upon the eyeball to make it longer when we look at something close up. His dissection of eyes had led him to believe that the disposition of the muscles was such that they could perform this function. He had also carried out a rather drastic experiment where he had changed the shape of his eye, using an instrument called a 'speculum oculi' so that he could focus at a distance of two inches and read the print of a book held there. Regretfully, one must conclude that the intellectual and experimental rigour behind this article is not of the same order as in the articles by other authors at this particular time.

Later in the same year Home had the opportunity of developing Hunter's work further and making contributions of his own, which he presented to the Royal Society in the Croonian Lecture on Muscular Motion, which he read in November 1794.[64] It is difficult, however, to estimate the amount of influence that Hunter's work had on Home. Home owned Hunter's notes and before his own death he burnt them.

Home's work on muscular motion was done with Jesse Ramsden (1735-1800), and their aim was to complete the work started by Hunter. Home stated that Ramsden was already familiar with the subject, and had brought to their experiments certain theories of his own.

Ramsden held that it was known that the crystalline consisted of substances of different densities, the centre being the most dense, and the density diminishing gradually towards its edges, so that its refractive power becomes nearly the same at its edge, as those of the substances with which it is in contact, namely the aqueous and vitreous humours. He felt that the density of its central parts, and the refractive index at its edge, which was very similar to that of the surrounding humours, made it unlikely that the lens was the means by which the eye accommodated. Here he was presumably thinking of accommodation being carried out by the lens changing shape. In his view the function of the crystalline was to correct the aberration arising at the cornea where the main refraction takes place. The eye seemed to him to be perfectly corrected for chromatic aberration, achieving this by the gradual change in its refracting power towards the centre of the crystalline, thus avoiding the multiple reflections created by the complicated lens systems of achromatic telescopes, which reduce the intensity and clarity of the image. In fact, in the eye there appeared to be only one extraneous image caused by reflection, that formed at the anterior surface of the cornea.

Since, as we have seen already, it was widely held that the removal of the crystalline in a cataract operation resulted in the loss of accommodation (as clearly stated by Porterfield, who deduced that it was thus the crystalline that allowed the eye to accommodate), Ramsden was rejecting a fairly longstanding belief, and to justify his theory, he turned to a cataract sufferer. He needed someone young, who had not yet lost any of his accommodating power owing to age and who had a cataract in one eye, the other being perfect. Benjamin Clerk, a sailor aged 21, provided him with an opportunity, having had a cataract removed in November 1793, and he was willing to allow Ramsden to carry out experiments upon him. The method of the experiments was to place a suitable lens in front of the 'couched' eye and to note where objects appeared most distinct, and also the maximum and minimum distances of distinct vision were again noted. Experiments carried out soon after the operation seemed to satisfy Ramsden that the eye which had no crystalline lens could still accommodate; the details published, however, seem far from conclusive. A year later a further experiment was carried out with the same man:[65]

> "The perfect eye with a glass of $6\frac{1}{2}$ inches focus, had distinct vision at 3 inches; the near limit was $1\frac{7}{8}$ inches, the distant one less than 7 inches.
>
> The imperfect eye, with a glass 2-2/10 inches focus, with an aperture 3/40 of an inch, had distinct vision at $2\frac{7}{8}$ inches, the near limit $1\frac{7}{8}$ inch, the distant one 7 inches.
>
> From the result of this experiment we find that the range of adjustment of the imperfect eye, when the two eyes were made to see at nearly the same focal distance, exceeded that of the perfect eye."

This is the only account giving the details of the results of Ramsden's experiments, although Home stated that he had carried out others which confirmed his results. The experiments were by their nature entirely subjective, since they required the subject to estimate his limits of distinct vision; it also appears that the imperfect eye had the benefit of a small stop in front of it, while the good eye had to rely upon the contraction of the iris which took place with the viewing of a close object. Nevertheless, Home and Ramsden were satisfied with their results and thought that the eye could accommodate without a lens:[65]

> "The results of these experiments convinced us that the internal power of the eye, by which it is adjusted to see at different distances, does not reside in the crystalline lens; we were also satisfied by the facts and arguments adduced in Mr. Hunter's letter on this subject, published in the first part of the last volume of the Philosophical Transactions, that it does not arise from a change in the general form of the globe of the eye; we therefore abandoned both of these theories."

Home and Ramsden now turned their attention to the cornea, to see whether a change in its curvature could be responsible for accommodation. It must be said that their experiments in this field, which were far superior to the one just described, led them away from the correct cause of accommodation. Their first experiment was to ascertain whether or not the cornea was elastic. They took a sample from a recently dead person, and showed that it could easily be stretched by 1/11th of its original length. Home and Ramsden had therefore shown that a change in curvature was possible. They now had to show how it happened, and that it did indeed take place.

Home first investigated whether the four external straight muscles of the eye could be responsible for the change in curvature of the cornea. On dissecting an eye, he discovered that the muscles approached to within 1/8in. of the cornea before their tendons became attached to the sclerotic coat of the eye. On gentle pulling he discovered that the tendons actually pulled away a layer of the cornea with them. Home was satisfied that he had clearly shown that the straight muscles had a connection with the cornea. In the meantime, Ramsden was devising a piece of apparatus which could measure any change in the external shape of the eye. The apparatus consisted of a board with a hole in the centre. The subject put his face through the hole, which held it in a fixed position. There was a microscope on the outside of the board, focused so that its field of view took in the front of the cornea which projected beyond the eyelids. The microscope could be moved forwards and vertically and horizontally. Difficulty was at first found in recognising the image of the cornea, but eventually four curved lines were seen clearly which were taken to be the outline of the cornea. The subject was made to look at a chimney 235 yards away, and then at an object 6 inches away. When this change of focus took place, the curved lines were seen to separate from each other, and the microscope had to be withdrawn from the cornea, whenever the person's eye was adjusted to the near distance; the reverse took place when it was fixed upon a distant object. Care had to be taken that both the distant object and the close object were exactly in the same straight line, so that the eye did not have to change the direction of its axis of vision. After some time it was necessary to shade the room to reduce eye strain; it was then discovered that the curved lines seen in the microscope were not the image of the cornea, but the image of the reflection of the window frame in the curved surface of the cornea. This accidental discovery, however, enabled the researchers to take a positive step forward, since, in the shaded room, the image of the cornea was clearly seen in the microscope.

Further experiments, now with a clear image of the cornea, showed that, when the eye was focused on a distant object, the surface remained in line with the micrometer wires of the microscope, and when it was adjusted to the close object, the image of the surface projected considerably beyond the wire. Since the room was now shaded, the original distant object was no longer visible, and the new distant object was now only 90 feet away. In changing the focus from the close to the distant object, the movement of the cornea was estimated to 1/800th part of an inch. Similar results were found when experiments were carried out on the eyes of three other subjects. Various attempts were made to obtain similar results by performing other functions with the eye, such as deliberately moving its axis while not changing its focus, but Home said that these motions did not give at all similar appearances in the microscope to those seen in the adjusting of the eye to different distances.

From these experiments Home drew the following conclusions:[66]

> "1st. That the eye has a power of adjusting itself to different distances when deprived of the crystalline lens; and therefore the fibrous laminated structure of that lens is not intended to alter its form, but to prevent reflections in the passage of rays through the surfaces of media of different densities and to correct spherical aberration.
>
> 2d. That the cornea is made up of laminae; that it is elastic, and when stretched, is capable of being elongated 1/11 part of its diameter, contracting to its former length

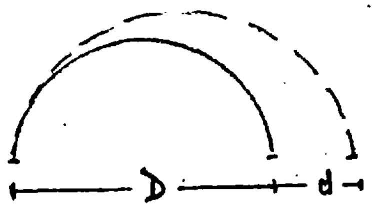
> immediately upon being left to itself.
>
> 3d. That the tendons of the four straight muscles of the eye are continued on to the edge of the cornea and terminate, or are inserted, in its external lamina; their action will therefore extend to the edge of the cornea.
>
> 4th. That in changing the focus of the eye from seeing with parallel rays to a near distance, there is a visible alteration produced in the figure of the cornea, rendering it more convex; and when the eye is again adapted to parallel rays, the alteration by which the cornea is brought back to its former state is equally visible."

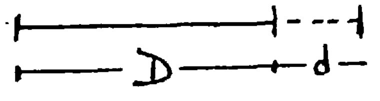
Having established to his own satisfaction that accommodation was produced by a change in convexity of the cornea - and it must be admitted that his experiments appear to be extremely elegant and precise - Home went on to explain how this change was produced. Home said that the four straight muscles of the eye are attached to the bottom of the bony orbit, near the 'foramen opticum'. They become broader as they pass forward and change into tendons as they arrive at the front part of the eyeball. These tendons adhere to the sclerotic coat and terminate in the external lamina of the cornea, which Home thought appeared as if it were a continuation of them. He said that these muscles could produce three very different effects on the eye: acting separately they move the eye in different directions; acting together with a small amount of contraction, they steady the eyeball: and when this contraction is increased, they compress the lateral and posterior parts of the eye. Home stated his opinion that this compression would force the aqueous humour forward against the centre of the cornea, while its circumference would be steadied by the action of the muscles. Thus the radius of curvature of the cornea would be rendered less, and its distance from the retina increased. He said that his experiments had shown that the eyeball did not recede into its orbit by these actions.

In support of his hypothesis, Home made a number of points. He felt that the straight muscles were larger, and extended further forward on the eyeball, than was necessary "for the purposes generally assigned to them; but when applied to so important an office as that we have just stated, their size and anterior insertion are easily explained." Anticipating criticism that, in general, muscles did not have multiple functions, he gave examples of other muscles in man and animals which had more than one function, dependent upon their degree of contraction.

Home emphasised that his experimental evidence led to the belief that the eye was at rest when viewing distant objects. While carrying out his experiments the subjects had commented upon the effort required to focus upon close objects. Thus the eye adjusted to distant objects owing to its elasticity, and used muscular action for the less frequently required near distances. The loss of elasticity with advancing years was, he said, well known in man, and it was the loss of elasticity of the cornea, rather than a weakening of the straight muscles, which produced the increasing inability of the eye to accommodate as one got older.

Home did not make clear what he meant by the extension of the cornea. Two interpretations were possible from the description given. The first corresponds to diagram II overleaf. Astrip of cornea was taken from a recently dead person. The arc of the





Diagrams II and III demonstrating two ways in which the cornea might be stretched cornea at rest was subtended by a distance D, and Home and Ramsden showed that it could be stretched by d, where d/D - 1/11. Or it may have been that the strip of cornea (diag. III) was first straightened and measured, equalling D; it was then found possible to stretch it linearly by a distance d; once again d/D = 1/11. As has already been pointed out when dealing with Jurin's work (p. 119), if the cornea is to change its convexity, a measure of surface elasticity is necessary and therefore to satisfy this requirement Home and Ramsden should have carried out the experiment corresponding to diagram III.

In spite of the confident tone of the paper which has just been discussed, Home was aware that his hypothesis was new, and he therefore returned to the subject a year later in the Croonian Lecture, read to the Royal Society on 12th November 1795.[67] He said that, since his explanation of accommodation was new, he had taken great care with the experimental verification of the basis of his hypothesis. In order to establish with greater certainty that the cornea did change its curvature during the act of accommodation, he now used an optical method. Its basis was to view an image reflected in the cornea, using a microscope with a divided eye-glass, and detect a change in the image as the eye focused upon images at different distances; the change in the image was to be shown to be due to the change incurvature of the cornea. The experiments were not conclusive, although Home did endeavour, not very convincingly, to show that they satisfied his hypothesis.

The experiment was set up initially using convex mirrors of .4in. and .5in. foci. In this way Home avoided any unsteadiness of the eye. An image was produced in the first mirror by a 3ft. by 6in. board, and the microscope focused upon the image of this board. Two images were produced by the divided eye-glass micrometer attached to the microscope and these were adjusted by means of the micrometer, until their surface of contact, which appeared as a black line, was rendered as small as possible. The other mirror was now put into place, and the line, which represented the contact of the two images, now had considerable breadth. It is not clear exactly what Home was measuring in this way. He said that the line had considerable breadth[68] "corresponding exactly to the difference between the convexities of the mirrors." One can therefore imagine that the change in convexity of a surface was detected by a varying width of the line in the divided eye-glass. In his subsequent experiments, focusing the microscope upon an image of the board produced in a cornea, Home therefore produced a thin line initially, and then attempted to see whether this line broadened as the subject focused his eye upon an object at a different distance. Broadening of the line would show that the convexity of the cornea had changed, and thus that the eye accommodated by means of a change in convexity of the cornea.

The same apparatus was used to hold the eye steady as had been used in the previous set of experiments. Home was the first subject, and Ramsden performed the experiments. Initially, when the eye was fresh, there was a perceptible change in the micrometer. However when the eye became fatigued, this change was not seen any more. Ramsden found that every time the eye adapted itself to a change of focus, the object glass of the microscope had to be moved towards, or away from the cornea. Experiments carried out on subsequent days could not detect this change, and they concluded that the observed change may have been due to the head having been moved forward. A test experiment, in which one of the convex mirrors was moved forward, confirmed that a change in the thickness of the line was seen. In spite of this, Home concluded, but not very satisfactorily, that there was a change in curvature in the cornea when the eye focused on objects at different distances, but that it was too small to admit of any conclusions being drawn from it. Further experiments were carried out with other young people, but no results were given, and Home commented that their eyes quickly became fatigued by the experiment.

In spite of the lack of results, Home was determined that his hypothesis was the correct one, although, as we shall see later, he did modify it so that the change in curvature of the cornea became only one of the changes taking place during accommodation. He now set out to see what degree of change in curvature could be detected, under ideal conditions, by the microscope method. He was of the opinion that it was impossible to keep the cornea absolutely still during the experiment, and under these imperfect conditions a change in curvature might take place which was not sufficient to be detected. He found that he could just detect the difference between the curvatures of two convex mirrors of radii .4in. and .408in. He deduced from these results that the change in (the radius of ?) the cornea could not be more than 1/125in.

Home now set out to find other changes in the eye which could also have an effect upon accommodation. He first devised an experiment to see whether the axis of vision could be extended by a uniform pressure applied to the eye. This was carried out by taking the eye of a dead subject and measuring its diameters by means of calipers. A hole was made in the centre of the optic nerve and a pipe fixed into it, through which air could be blown into the cavity so as to distend the eye. While the eye was distended, the same diameters were measured and compared with those previously taken. It was found that when the eye was distended, the transverse axis was diminished, and the axis of vision was lengthened. This effect was found only in the eyes of young subjects, and it was not possible to detect any change in the eye of a man of fifty. Home deduced from this experiment that when the pressure is increased laterally, and from outside the eye, the elongation must be greater still. It must be said that this conclusion does not appear to follow from the experiment, since distending the eye as in the experiment by increasing the internal pressure has surely an opposite effect to increasing the lateral pressure outside the eye. It is therefore difficult to imagine that the first experiment proved that increased lateral pressure would also increase the length of the optic axis.

Home now came to combine the changes in the eye, and to explain how the combination occurred and produced the accommodating power of the eye. He said that the lateral pressure on the eye, in which the contraction of the four straight muscles played a considerable part, would elongate the eye, increase the convexity of the cornea, and push the crystalline lens and ciliary processes forward in the same proportion as the cornea was stretched. He said that the ciliary processes form a septum between the vitreous and aqueous humours and were moved forward with the lens when the cornea was rendered more convex. In order that this might happen he thought that the ciliary processes were probably possessed of a muscular power. This does not seem necessary from the description he had just given, from which it would appear that the ciliary processes played only a passive role and therefore had no need of muscularity; however Home also made the interesting point that it was a commonly held opinion that they had a muscular power. As we shall see later, Young strongly denied that the ciliary processes possessed muscularity, but it is perhaps strange that, in view of the common belief in their muscularity, the change in shape of the crystalline due to the action of the ciliary processes did not figure more prominently in the flurry of activity in this subject at the end of the eighteenth century.

Concluding his paper, Home thought that the adjustment of the eye to objects at different distances was produced by three different changes: an increasing curvature of the cornea; elongation of the axis of vision; and a motion of the crystalline lens. He thought that these changes depended a great deal upon the contraction of the four straight muscles of the eye. Ramsden produced some figures which led him to believe that the change in curvature of the cornea produced one third of the accommodating power, and the movement of the lens and the elongation of the axis, the other two thirds.

It is appropriate to end this discussion of the theories of accommodation produced at the end of the eighteenth century with an account of a paper by Thomas Young.[69] In this, the experiments evolve until they reach an elegance not seen before in research in this subject, and Young showed a truly scientific readiness to discard hypotheses which he had held only a few years before. It is a great pity that after such logical and methodical investigations of all the properties of the eye which could have a bearing on accommodation, Young was not able to make the inspired leap to the correct explanation which is also fundamental in true scientific exploration.

Young started his experimental work by referring to the Optometer first described by De la Hire, and developed by Porterfield and later by Dr. Motte. This was the instrument which used two holes or slits in a card, sufficiently close to lie within the area of the pupil, and enabling the point of focus of the eye to be determined.

He described his own improved version of the instrument, and went on to describe experiments he had carried out on his own eyes to determine their point of natural focus and other optical dimensions. In so doing he ranged over many other points: the smallest angle subtended by a visible object, the number of sentient points on the retina, the three-colour theory of vision, and the surprise discovery of his own astigmatism, although of course the name had not yet been coined, since the defect had not been widely noticed. This part of the paper was merely a prelude to the discussion of accommodation, yet covered so many important aspects of vision, that I submit a reprint in Appendix II.

The principle and design of the optometer is shown in figs. 109, 110 and 111 after Appendix II. From the fact that any object not at the eye's natural focus would appear double, when viewed through the optometer, Young devised a simple method of quickly finding this focus. Instead of using an object and moving it towards and away from the eye, he arranged for the object to be a straight line pointing towards the eye at an oblique angle. When viewed through the optometer, this line appeared double, except at the eye's natural distance of vision, so that the subject saw two intersecting lines, the point of intersection corresponding to the natural point of vision. An index sliding on the scale of the optometer quickly measured this distance. Young's Optometer used slits rather than holes, and he had a series of interchangeable cards, with slits varying in distance apart from 1/10 to 1/40 in. to suit the size of pupil of the subject.

For convenience, Young chose to make as many experiments as possible using his own eyes. He first used a number of ingenious devices to measure their constants. To measure the vertical and horizontal chords of the cornea, he used dividers with small keys fastened to their points. With the rings of the keys, he was able to touch the surface of the eye. He measured the protrusion of the cornea from the sclerotica, by looking at the reflection of the left cornea with his right eye, using a small mirror held by his nose, and with a scale held beside his left temple on which he read the protrusion. From the vertical and horizontal diameters and the protrusion, he was able to calculate the radius of the cornea. He found it to be 31/100in. The descriptions which Young gave of these measurements were not very detailed, and in one case in particular it is difficult to establish how he made the calculation from the description of the measurements he took. This was his description of the way he measured the length of the optic axis of his own eye:[70]

> "To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowances of 3 hundredths for the coats is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded."

From my own eyes I find it impossible to make the image caused by pressure on the sclerotic coincide with the optic axis, but Young said that this could probably be done only with protruding eyes. However, this difficulty apart, I am still unable to account for his method of calculating the optical axis from the measurement he described.

Young discovered that the aperture of his pupil varied from 27 to 13 hundredths of an inch, although he realised that this was subject to the magnification of the cornea; therefore he estimated that its true range was perhaps from 25 to 12 hundredths. In a state of relaxation his eye focused rays from a vertical object at a distance of ten inches, to a point on the retina, and from a horizontal object, at seven inches. In this way he had discovered his own astigmatism. This discovery was made using the optometer, and Young said:[71]

> "For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches; if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments."

This defect had been noticed, but apparently not investigated, and Young mentioned that he had now discovered that others who suffered from it helped to overcome it by holding a lens obliquely to their eyes. Young stated that the effect was not produced by the cornea, since he had subsequently found that it still existed in cases where the cornea had been removed. He said:[71]

> "The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimenions already given, to be about ten degrees."

In fact the defect can well be caused by uneven curvature of the cornea.

Young went on to estimate the refractive index of the crystalline lens and its focal length. He did this, partly by using measurements of his own eye, which he had already calculated, and partly by carrying out experiments on eyes from dead persons and animals. He found difficulty in both these methods: in the first, his measurements contained a significant proportion of estimates; in the second, changes, such as the absorption of water into the crystalline, took place after death. He also appreciated that the crystalline had a varying refractive index, which became larger towards its centre. Before moving to the discussion of accommodation, Young also considered the position of the optic nerve in the retina, and its size, and also the degree of achromatism possessed by the eye.

Turning now to the faculty of accommodation, Young first gave some instances of the power of accommodation possessed by some men and women of his acquaintance:[72]

Young-closest distance of perfect vision - horizontal rays 2.6in.

Young - closest distance of perfect vision - vertical rays 2.9 in.

Wollaston-closest distance of perfect vision 7 in.

Abernathy could see clearly between 3 and 30 in.

Young lady could see clearly between 2 and 4 in.

Middle-aged lady could see clearly between 3 and 4 in.

Unfortunately he clearly thought that the sort of vision possessed by these acquaintances was reasonably normal, whereas the two ladies suffered from acute short-sight. However, Young used these results to satisfy himself merely that the range of his own eyes was about "medium". This can be considered to be a most unexpected comment, since at the time Young was just under thirty years of age and would still have good powers of accommodation; but his near point of 2.6 inches indicates that he suffered from short sight, and it is surely very surprising that he was not aware of his abnormality. He also commented that there seemed to be some reason to think that the faculty of accommodation diminished in some measure as persons advanced in life. This was widely accepted at the time, and it is perhaps surprising that Young was not more clearly aware of it.

He then went on to consider the degree of change which would be necessary in the eye in order to bring about the range of accommodation which he had outlined above, and which he thought was reasonably normal. The changes in the eye that he considered were: a change in the radius of the cornea; a change in the distance of the lens from the retina, or some combination of these two; or a change in the figure of the lens. One can presume that he considered that this last change would occur on its own, and not in combination with other changes. In order to consider these changes independently of the effects of the other refracting surfaces in the eye, Young used an ingenious device. He drew a series of curves which were the loci of the points of focus of rays entering the eye from an object 10 inches from the eye, as they would be formed by successive refractions at the different surfaces of the eye. He chose the distance 10 inches, since this was the distance of natural relaxed vision for his own eyes, which he intended to use for his experiments. These curves are shown in Plate 10, fig. 80. Details of how these curves were calculated were not given, but the image formed by the cornea alone - with the crystalline lens removed - would fall between curves 1 and 2, the distance between these two lines showing the degree of confusion of the image. The advantage of this method was that, when Young wished to consider the change in the radius of the cornea, necessary to account for the degree of accommodation possessed by his eye, he merely had to calculate the change in curvature necessary to ensure that the image of objects at different distances always fell within the limits of curves 1 and 2. When his eye was at rest, the cornea focused rays from an object at 10 inches within curves 1 and 2. The closest point from which he could focus rays was 2.9 in.; therefore he had to calculate what change in the curvature of the cornea would bring rays emanating from a point at this distance to a focus within curves 1 and 2. In this way Young had eliminated the necessity of considering the effect of the other refracting media on the rays, and it was also no longer necessary to relate all the images to the curve of the retina.

He found that the radius of the cornea would have to be diminished from .31 in. to .25 in. in order that he could see objects at 2.9 in., or very nearly in the ratio of five to four.

Considering that the retina might move in order to allow a change of focus, Young said:[73]

> "Supposing the change from perfect vision at ten inches to perfect vision at 29 tenths to be effected by a removal of the retina to a greater distance from the lens, this will require an elongation of 135 thousandths, or more than one seventh of the diameter of the eye. In Mr. Abernathy's eye, an elongation of 17 hundredths, or more than one sixth, is requisite."

Fig. 80.

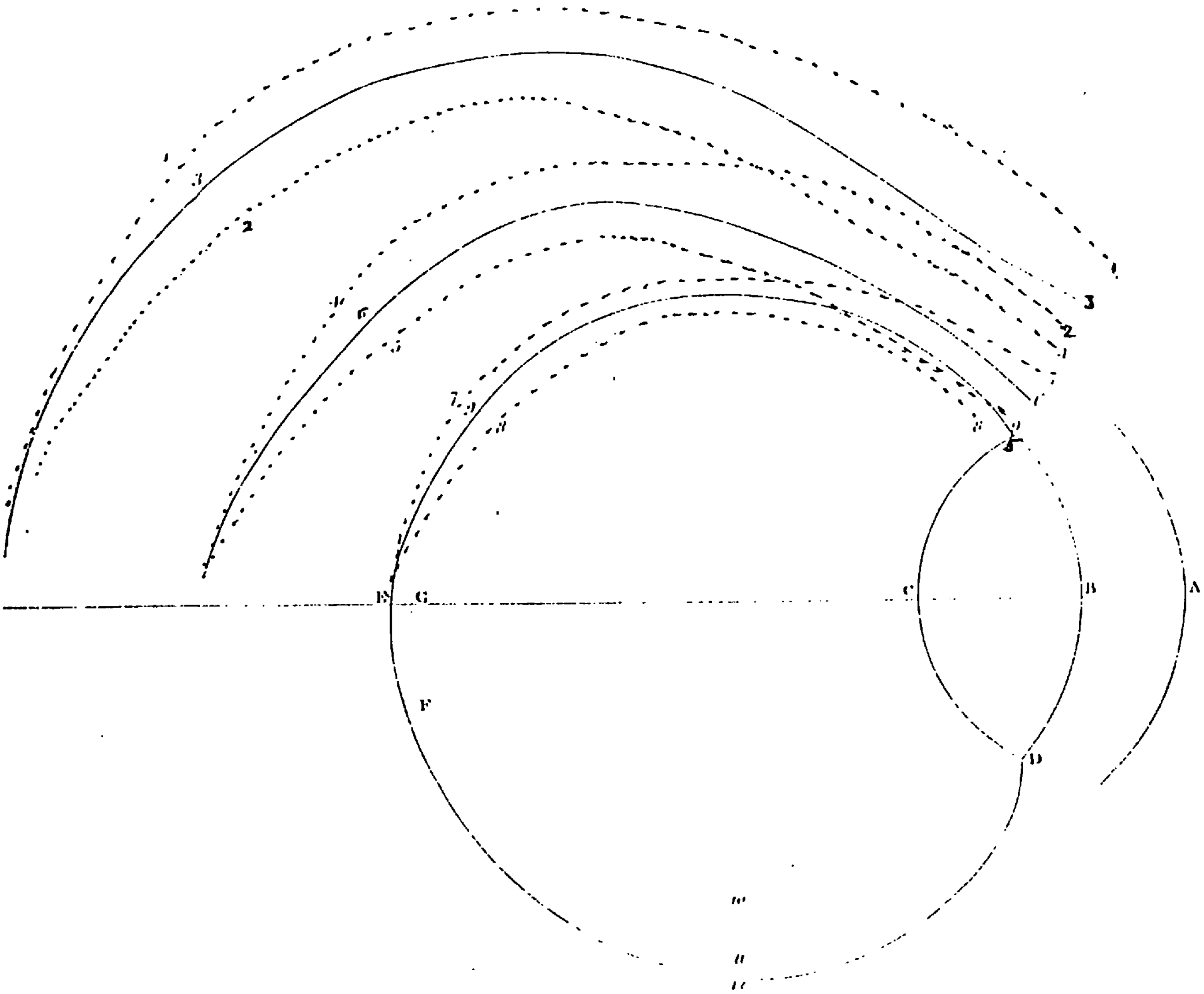


Plate 10 from Thomas Young's "Acourse of Lectures on Natural Philosophy.

Pub. by J. Johnson London 1 July 1807

Considering that a combination of these effects might occur, Young stated that if the radius of the cornea be diminished by one sixteenth, to .29, the eye would have to be elongated by .097 or about one ninth of its diameter.

Calculating the change in the curvature of the lens which would produce the same effect presented greater problems:[73]

> "Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. This is more than double the actual change. But it is impossible to determine precisely how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed."

Young calculated that, disregarding the elongation of the axis, the anterior surface would become 21, and the posterior 15, hundredths.

Young felt that, in spite of the very high standard of experimental work which had been carried out by Home and Ramsden and also by Dr. Olbers(1758-1840) of Bremen, all of whom failed to detect any change in the curvature of the cornea during accommodation, there was still room left for a repetition of the experiments. The first experiment involved viewing through a microscope the images of two candles formed in the cornea of the subject. At the same time the other eye of the observer viewed a graduated card marked into 40ths. of an inch. The subject was asked to focus on objects at different distances, without changing the direction of his eyes. The sizes of the images of the candles were compared throughout against the graduated scale and no change could be detected. As any experienced physicist will know, the comparison of two different images simultaneously through different eyes sounds far more difficult in the description, that it is in practice. Nevertheless, Young experienced difficulty in measuring distances with his naked eye, without an error of one 500th. of an inch.

This led him to repeat the experiment without using the magnifying power of the microscope. The initial reaction of the reader to this decision must be to question how this can lead to greater accuracy, but Young, by this means, was able to adapt the experiment to use his own eyes, and be at once his own subject and observer - a situation that he always seemed to prefer. For the experiment Young used a divided eye-glass, the two portions of which were separated to allow the images to pass between them. The images were formed in his own cornea and he viewed them by looking in a mirror. He noticed no change in the images when he changed the accommodation of his eye. The description of this experiment is brief and I have not been able to understand how it would work. It would involve viewing the images formed in the cornea, by using a mirror. The images thus formed in the mirror would be at a given distance from the eye, which would be accommodated to view them at this distance. For the second part of the experiment, the eye changes its accommodation, but the image would be at the original distance and would be difficult to view clearly, since the eye would now be focused at a different distance. Young said:[74]

> "I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance."

This statement is not quite as remarkable as it seems, and one can quite easily verify for oneself that a close object can be kept under careful scrutiny, while the eye is focused for a distance. However, while the change in accommodation is actually taking place, concentration is somewhat lost, and it would be at this instant that any change in the size of the image in the cornea would be most easily detected.

Young carried out three other experiments of a similar nature, but differing slightly in detail, and was unable to discover any positive evidence of change in radius of the cornea. In a last experiment he no longer used a method based upon Home's and Ramsden's original experiment; he devised a method that shows clearly his talent for experimental ingenuity. Young took a lens from a microscope, together with its fixing cylinder. He filled the cylinder three-quarters full with water and applied it, as one does an eye-bath, to his eye. The cornea was not in contact with water and acted in conjunction with a (roughly) plano-concave water lens. Young's own account and his diagram of this experiment (Plate 9, fig. 77) is reproduced in Appendix II, but I have taken the liberty of drawing what I hope is a clearer diagram overleaf, Diag. IV. He found that his eye became long-sighted, owing to the water, and that the microscope lens was not sufficient to restore its normal range of vision, although the addition of a further convex lens did this. Testing his eye with an optometer, he found that his astigmatism remained. He also found, more importantly, that his power of accommodation was the same as before. In testing his accommodating power he had to make some allowance for the fact that he could no longer measure accurately from the surface of his cornea. Indeed, his first results showed a slight diminution in the accommodating power of his eye, but

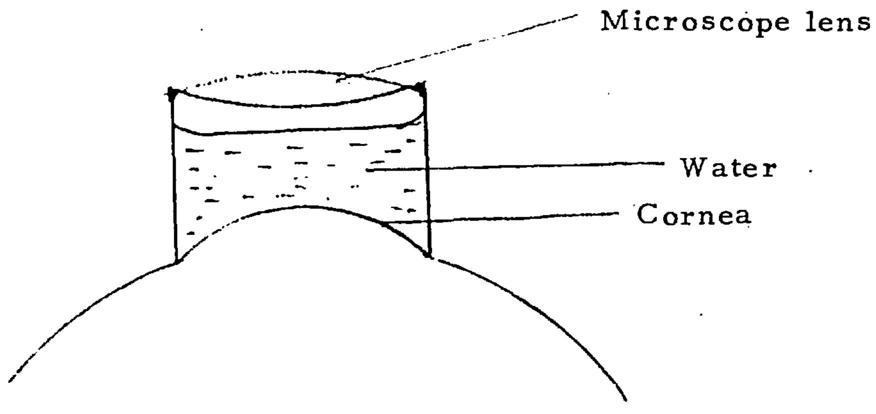


Diagram IV showing how Thomas Young investigated whether or not the cornea changed its curvature during accommodation he calculated that this was because the 'artificial' cornea, as he called it, was about 1/10th inch from the natural cornea.

Therefore, by a number of carefully executed experiments, Young had shown that the cornea did not change its radius of curvature during the act of accommodation. One can draw an interesting inference about the embryonic nature of scientific method at this time from the fact that Young apologised for giving details of his experiments:[75]

> "After this, it is almost necessary to apologise for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence."

The next possible cause of accommodation to be considered was the lengthening of the eye in order to view close objects. It had been estimated earlier in this paper that in order to allow for the observed range of accommodation by this method, the eye would have to increase its length by one seventh. The first method used by Young was to turn his eye inwards as much as possible, and view the images of two candles formed in the front surface of the eye, using as before a mirror placed in front of the same eye. One of the images was arranged so that it was at the extreme edge of the sclerotica and defined it as a bright line. The other image was formed in the middle of the cornea. As the eye accommodated, he noticed no change in the images.

The next experiment, which was described as "much more delicate", involved wedging the eye so that it could not lengthen. A key was used and was wedged between the eye and the surrounding bone; the whole procedure seems extremely painful and even more so when one considers Young's actual description:[76] "The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by moderate pressure between the eye and the bone." One wonders at his phlegm as one reads the dispassionate account of the experiment, and also how he could take the necessary observations while in such discomfort. The actual experiment was comparatively simple. The insertion of the key gave rise to what Young called a 'phantom' image on the retina. If the eye attempted to lengthen, it would be prevented by the wedged ring, and the size of the 'phantom' would increase. No such effect was noticed, although the power of accommodation remained as great as usual.

If the eye did indeed lengthen during accommodation, Young realised that there would be a slight change in the size of the image during the change in focus. This would be due to the simple fact that the rays would have further to travel before focusing upon the retina, when a close object was studied, and therefore the image would be slightly larger. Young was of course assuming that the lens did not change shape during accommodation, and in any case he was of the opinion that the lens played little part in refracting the rays. He used the effect of an enlarged image being formed if the eyeball lengthened in his next experiment, which was ingeniously simple. He placed two candles so that their images upon the retina fell upon the opposite edges of the blind spot. Without in any way changing the direction of vision, he now made as much change as possible in the focus of his eye. If there were any elongation of the axis, he would have expected a movement of one of the candles. Young's description, which is given below, is not very clear on the exact details of the experiment, but the basis of the method is beautifully simple. Either both candles were initially invisible, and if the eye lengthened its axis, one or both would be expected to appear; or they were both just visible, and on lengthening the axis, one or both would disappear, or there would be some movement of the images. Young's own description was as follows:[76]

> "I placed two candles so as to exactly answer to the extent of the termination of the optic nerve and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Plate 9. fig. 79.) But this did not happen: the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole."

Young briefly considered the possibility that compensating changes might simultaneously take place in the curvature of the cornea and/or lens, which would keep the size of the image constant, but felt that in fact it was highly improbable that any material change took place in the length of the axis. In any case he found it difficult to imagine how such a change could be brought about.

Considering the action of the straight muscles, Young acknowledged that, acting independently of the eye socket, they could be imagined to flatten the eye and thus lengthen its axis. Even considered in the normal situation in the orbit, it could be conceived that eye could be lengthened by being forced further into the orbit. However, when the eye were turned to either side, the orbit would then tend to shorten the eye. Young said that this change should be detectable with the eye held in different positions, but he had tested it with the optometer and had found no such change in the focus.

Having so exhaustively shown that no change in the curvature of the cornea or lengthening of the axis of the eye could be detected, Young dismissed in a paragraph the claim that some combination of these effects produced accommodation, though he acknowledged that the theory had gained great respectability from the work of Olbers, and of Home and Ramsden.

Turning lastly to the crystalline lens as the possible cause of accommodation, Young, not surprisingly, attempted to satisfy himself whether or not patients who had had a cataract operation had lost the power of accommodation. It must be remembered that evidence was conflicting on this matter, and that Home's and Ramsden's experiments reported in the Croonian Lecture in 1795, had given great weight to the opinion that accommodation remained after the removal of the lens. It must also be remembered that prior to these experiments Young himself was of the opinion that the crystalline changed shape, and was responsible for accommodation. One can be reasonably certain that Home's and Ramsden's work played an important part in causing him to reinvestigate the whole problem. If he discovered that without the crystalline lens accommodating power was lost, as Porterfield had maintained much earlier, then it was possible, even likely, that the lens was responsible. If accommodation was still present to the same extent as before, then the crystalline could not possibly be responsible.

Young carried out measurements on a number of subjects who had had cataract operations. He found that in general they did possess a range within which they could see reasonably clearly, but when he used the optometer, he could find no measurable accommodating power. He felt reasonably satisfied with his results, but acknowledged that some of the subjects were not ideal, owing perhaps to age or to an imperfect operation which, in one case, had left a portion of the lens capsule across the pupil. Therefore, in order to establish the position as clearly as possible, he resolved to look again at the evidence put forward by Home and Ramsden in their 1795 Croonian Lecture. Young felt that the observations made upon the eye of Benjamin Clerk, which were published in that lecture, could be explained by the distinction made much earlier by Jurin between distinct vision and perfect vision. Young also queried, as I did on P. 186 the effect in the experiments on the 'couched' eye of the use of a small stop in front of the eye. He pointed out that with the same aperture of 3/40 in. he had a range for reading from $1\frac{1}{2}$ to 30 in., whereas Clerk had a range, using the same lens, of $1\frac{7}{8}$ to 7 in.

Having progressed so far in his experiments, Young now permitted himself for the first time to state the aim of his paper, although the outcome was becoming progressively clearer to the reader:[77]

> "Hitherto I have endeavoured to show the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which in the first place come very near to a mathematical demonstration of the existence of such a change, and, in the second explain in great measure its origin, and the manner in which it is effected."

In the light of the care with which he carried out his preceding experiments, one can only admire Young's meticulous application of scientific method, when many of his contemporaries were rushing to far-reaching conclusions based upon only the flimsiest experimental evidence. He had carried out the most meticulous investigation of all the alternative theories, before allowing himself to experiment in the area which he felt was the correct one: the change in shape of the crystalline.

The experiments to which Young referred, as providing a mathematical proof of the change in shape of the lens, had to do with the change in shape of the images of point sources of light, when the eye changed its focus, and, also, with the change of shape of the slits of an optometer as the eye accommodated. In the first experiment he noted that if the point was beyond the furthest focal distance of the eye and the eye relaxed, then the eye distorted the image, so that a star with a bright centre was seen. If the focus of the eye was made shorter, then the central part of the image became less bright, and the edge much brighter. The appearance of the first object is given in Appendix II, Plate 12, Fig. 92, n. 36-9, and the second case n. 41. The second experiment concerned the appearance of the slits of the optometer. While the eye was relaxed, these appeared perfectly straight, but when the eye accommodated, they appeared curved: Fig. 92, n. 42, 43, 44, 45.

Once again the explanation which Young gave of the way in which he reached the conclusion from his results was brief. He drew attention to his observations that if the point of light was brought well within the focal distance of the eye, the change in accommodation of the eye increased the illumination of the centre at the expense of the edge. He said that this effect could still be produced even when the effect of the cornea was removed by immersion in water. He thought that the only way to explain this observation was to assume that the lens acquired a greater degree of curvature at its centre than at its edge. Further, he emphasised that the apparent change in shape of the optometer slits as the eye accommodated could not be explained by any imaginable movement of the retina, while the refraction of the lens stayed the same.

The reasoning here, I feel, is far from clear, but is probably roughly as follows. Young noticed the curvature of the slits as the eye accommodated, and attributed them to spherical aberration; he thought that this could be caused only by the increased curvature of a refracting surface in the eye, and certainly not by movement of the retina. Thus accommodation must be due to the increased curvature of either the lens or the cornea. However, in another experiment, he noticed that during accommodation the image of a point source changed its nature, and that this still happened, even when the effect of the cornea was removed by immersing it in water. Thus accommodation could not be caused by the change in curvature of the cornea; it could be caused only by the change in focal length of the lens. As if to answer the criticism that the eye did not suffer from spherical aberration, he noted that the point source effect could be noticed only by people with large pupils and a great degree of accommodating power. Thus Young had proved to his own satisfaction by means of a positive experiment that the changes associated in some people with accommodation could be explained only by means of an increase in curvature of the lens. He had therefore satisfied his own exacting standards, and provided, as he had wished, a positive experiment in favour of a change in the lens, whose results he said outweighed all those in which he had merely shown that no change was possible other than a change in the lens.

Young attributed the spherical aberration to the fact that eye which had this ability to see the curvature of the lines had large pupils and therefore some rays passed through the edge of the lens, which was always more susceptible to aberration. He also made the interesting point which had only been hinted at by earlier writers, that, when viewing close objects, the pupil narrows in order to prevent the distortion which would otherwise arise owing to rays passing through the edge of the lens. It is interesting that a phenomenon which had been known for well over one hundred years had not been explained before, given the simplicity of the explanation. It had also been known since as early as 1650 that "stopping down" telescopes reduced spherical aberration. Young felt that the increase in lateral thickness of the lens would cause no problem within the eye. He also felt that the aberration of the lens was inevitable, since, if the curvature of the lens on accommodation increased as it should, in order to eliminate aberration, right to the edge of the lens, then the lens would become so fat that its diameter would decrease, tearing the attachments to the ciliary processes.

Having satisfied himself that the lens did in fact change shape, Young now turned his attention to the way in which this occurred. He felt that the change could not be caused by an external action. While being far less certain that the lens was a muscle than he had been in his paper originally published in 1793, Young nevertheless felt that the behaviour of the lens was analagous to that of a muscle. He noted that a muscle never contracted without at the same time swelling. However, he decided that what he had earlier described as tendons were not. He also considered whether the swelling of the lens might also be accompanied by an increase in refracting power. This was obviously an attractive idea, and one which I have not met elsewhere. He felt, however, that there was no evidence to support this theory. Again he was first, as far as I can ascertain, in attempting to see whether the lens could be made to change shape by means of an electric current. His experiment was not able to produce any change in the lens.

Young allowed himself to express one strongly-held conviction, which he readily acknowledged was not supported by experimental evidence. He said that he had tried very hard to find evidence in favour of nerves entering the lens, but to no avail; nevertheless, he said he was convinced that such nerves existed. However, his efforts had given him a great insight into the nature and situation of the ciliary substances. The nature of these substances had been somewhat in doubt, even to the extent that what actually comprised the ciliary substances was not clearly agreed. Young described his method of dissection, from the back of the eye, and clearly defined their extent. Unfortunately, his observations led him to a wholly erroneous conclusion:[78]

> "The appearance of the processes is wholly irreconcileable with muscularity; and their being considered as muscles attached to the capsule is therefore doubly inadmissible. Their lateral union with the capsule commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be cannot easily be determined: if it were necessary to have any peculiar organs for secretion we might call them glands for the percolation of aqueous humour; but there is no reason to think them requisite for this purpose."

Some details were given of Young's investigations. He admitted to being initially deceived by the red colour of the choroids of fish; it will be remembered that a red colour was widely considered to be essential in muscles; however, he felt that this criterion of colour no longer carried any weight. He felt that certain properties of the terminations of the processes, and some apparent transverse divisions, did resemble those of muscles. Under a microscope, however, Young found the processes to be of a uniform texture, without the least fibrous appearance.

It is indeed a great pity that Young so positively misled himself in the actual agent causing the lens to change shape. He came early to his incorrect conclusion and one can see from the foregoing quotation how adamant was his view. The whole consideration of the ciliary processes occupies little more than a page of his work, and his conclusion made it impossible to discover their correct function in the correct solution to the problem of accommodation. He said of the ciliary process:[79]

> "Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity."

In spite of the undoubted rigour and stature of Young's work, which should surely have established beyond reasonable doubt that the eye had a definite internal power to accommodate, .Everard Home in the Croonian Lecture to the Royal Society on 5th November 1801, continued to argue against such a power of accommodation.[80] In this lecture, Home gave an account of his continuing work to establish that the ability of the eye to accommodate did not depend upon any internal changes in the crystalline lens. As in his previous work, he used as subjects men who had had the lenses of their eyes removed in cataract operations. It will be recalled that Home and Ramsden in their original work used experiments which were extremely simple, merely requiring the subject to say at which distances he could see distinctly. It is clear from Home's early remarks in this later paper that he felt that such experiments were far less liable to error than more sophisticated ones. He described his first experiment as follows:[81]

> "A piece of pasteboard, with a letter of a moderate size, as an object upon it, was put into his hands; as he could not read, the page of a book might have confused him: he was directed to vary the distance of the pasteboard from his eye, till he had ascertained the nearest and most distant situations, in which the object appeared distinct; these distances, by measurement, were 7 inches, and 18 inches. In repeating this experiment several different times, he brought the object very correctly to the same situations.
>
> This result convinced Mr. Ramsden, that the eye possessed the power of varying its adjustment; and he did not think any more complex experiments would be nearly so satisfactory; consequently, no others were made, and the man was allowed to go into the country.
>
> It was intended to make him a present of a pair of spectacles, allowing him to choose those best adapted to his eye; but his sight was so very good, that we entirely forgot it, till some time after he was gone.
>
> These experiments confirmed the former ones so very strongly, and from their simplicity were so much less liable to error, that Mr. Ramsden and myself considered the object of our inquiry completely attained; the reason for not, at the time, laying them before this learned Society was, that they established no new fact, and the former ones did not appear to require their support."

However, in the light of Young's experiments, Home did decide to carry out experiments using an optometer similar to that devised by Young, to see whether accommodating power could be detected after cataract operations. His initial trials with the instrument caused him some surprise, since neither he nor any of his friends over the age of forty could gain any positive results with the apparatus. When the subject looked through the instruments, two intersecting lines were seen. If the eye was then accommodated to a different distance, the point of intersection moved. Home found that only young persons under the age of thirty could change the point of intersection. This caused Home some considerable concern, since if he himself could not obtain positive results with the apparatus, he could not expect the subject of a cataract operation to obtain any:[82]

> "As I could not doubt of my own eye having the power of varying its adjustment, I was led to believe that the instrument required some address in the management, which I had not acquired; and therefore despaired of making Henry Miles sufficiently master of it, to do justice to my views."

Home was clearly surprised that only young people could operate the optometer satisfactorily. Therefore it is probable that the loss of accommodating power that occurs with advancing middle-age was not well understood at this time. As the power of accommodation is steadily lost, the eye usually becomes more long-sighted and spectacles are required for close work. This defect is called presbyopia (a word used by Home.) Persons who are naturally long-sighted suffer from hypermetropia. The two defects produce similar symptoms, but presbyopia is caused by loss of accommodating power, while in an hypermetropic eye a full range of accommodating power is maintained. The loss of accommodating power can be used as an accurate measure of the ageing process, since it occurs to a very similar degree in all people of a given age. Therefore, regardless of other signs of apparently maintained youth or premature ageing, we all lose our accommodating power at a very similar rate. At the end of the eighteenth century, however, it would appear probable that presbyopia was considered to be an increasing tendency to long-sight, rather than a loss of accommodating power.

Home overcame the deficiency of the optometer by removing the lens and by using a line four feet long. The increase of the distance at which the eye had to focus now made it suitable for presbyotic eyes. Home quoted the results of the use of the optometer by three subjects, who all made the lines intersect at about 12 inches, as the near distance, and at about 30 inches, as the furthest distance. Henry Miles, who had had the cataract operation, then used the instrument and achieved a near-point intersection of 8.3 inches, and a far-point intersection of 13.3 inches.

Another experiment was also devised, in which Miles had to focus on a spot on a sheet of card placed close to the eye, and then change focus to another spot at a greater distance. He found difficulty in seeing the second spot clearly immediately after he was directed to look at it. The result was the same whether he looked first at the distant spot, or first at the close spot.

From the results of these two sets of experiments, Home and Sir Henry Englefield, who worked with him, were satisfied that Miles' eye was not deprived of its power of adjustment.

Thus, at the beginning of the nineteenth century Science found itself considerably advanced in this particular branch of Physiological Optics; and yet a considerable part of the mystery of accommodation still remained. Young's work was undoubtedly of greater stature than anything which had preceded it. One's view of his ingenuity of experiment and the carefully-reasoned conclusions can only be enhanced when one compares his work with the efforts of his contemporaries. Many others had made isolated steps forward, which, for their time, were as impressive as any of the individual conclusions of Young and one could number among these Mariotte, De la Hire, Porterfield, Jurin and Petit. Young's unique contribution was the comprehensive way in which he rigorously explored every aspect of a problem, not allowing prejudice to colour his conclusions. And yet, the overall feeling must surely be one of regret that the true cause of the change of shape of the crystalline had still not been found.

Young's paper is reproduced in Appendix II to this section.

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127. Meeting with no ſatisfaction in any of the *hypotheſes* above related, I have applied myſelf to a diligent conſideration of the parts of the eye, in order to find out, if poſſible, ſome power or powers ſeated within it, by which its conformation may be ſo altered, as adequately to anſwer the effects obſerved. And in order to enable the reader to judge how far I may have ſucceeded in this reſearch, I ſhall, before I lay down my own opinion, examine a little into thoſe parts of the eye, which I think ſubſervient to the effect in queſtion.

128. The *cornea* is a compreſſible and ſpringy membrane, eaſily giving way to any force external or internal, and eaſily reſtoring itſelf to its former figure by its own ſpring aſſiſted by the preſſure of the aqueous humour within it.

129. The *uvea* is a muſcular membrane, and as ſuch is capable of contracting itſelf into leſs dimenſions. It ariſes from a circular ridge or protuberance running all along the inſide of the *cornea* at its juncture with the *ſclerotica*, which ridge I do not remember to have ſeen hitherto taken notice of by any Anatomiſt.

That the *uvea* is furniſhed with a narrow ring of circular muſcular fibres on the edge next the pupil, is now generally agreed by Anatomiſts, though, I think, not ſo much from their being able to demonſtrate thoſe fibres, as from reaſon; for as much as the contraction of the pupil upon a ſtrong light, or upon attentively viewing a very near and ſmall object, is plainly viſible, and that contraction is juſtly preſumed to be owing to ſuch a muſcular ring. Mr. *Ruyſch* indeed has repreſented this ring of muſcular fibres in one or two of his figures, but he tells us at the ſame time, *Sculptor hic juſto diſtinctius repræſentavit, nam in objecto ipſo non ita luculenter viſuntur.* * And in another place he ingenuouſly declares, † *Fateor huſce fibras circulares non tam luculenter conſpici poſſe, quin oculi mentis in ſubſidium ſint vocandi.*

It is likewiſe an agreed point, that the *uvea* is furniſhed with ſtreight fibres inſerted into this ring, and having their origin from that part of the *uvea* which is connected to the inner edge of the *cornea*, and that theſe ſtreight fibres, which are put upon the ſtretch and drawn out into a greater length when the ring contracts, do again by their ſpring, or by their muſcular force, reſtore themſelves to their former dimenſions, and thereby ſerve to dilate the pupil, when the abovementioned muſcular ring ceaſes to contract, and is in a ſtate of inaction.

But it is here to be conſidered, that, when theſe ſtreight fibres are thus put upon the ſtretch by the contraction of the muſcular ring, they muſt neceſſarily draw the edge of the *uvea*, which is connected to the *cornea*, and likewiſe the edge of the *cornea* itſelf, a little inwards at the ſame time. And this edge of the *uvea* cannot be drawn inwards, without contracting into a leſs circumference than it had before. Muſt not therefore this edge of the *uvea*, which is next the *cornea*, be furniſhed with a ring of circular fibres, whereby it may contract itſelf into a leſs circumference, as well as that edge of the *uvea* which is next the pupil? To me this part of the *uvea* appears of ſuch a ſtrength, and to adhere ſo ſtrongly to the *cornea*, by the reſiſtance it makes in tearing them aſunder, that I make no doubt of its being muſcular, there ſeeming in this place to be no occaſion for a membrane of that ſtrength, unleſs it were to exert a muſcular force, and ſuch a one as might overcome a conſiderable reſiſtance. I ſhall therefore make no ſcruple of qualifying this limb of the *uvea* next the *cornea*, by the name of the greater muſcular ring of the *uvea*, to diſtinguiſh it from the other ring next the pupil, which I ſhall hereafter call the leſſer muſcular ring.

It will perhaps be objected to me, that the exiſtence of this ſuppoſed greater muſcular ring has not yet been proved by ocular demonſtration. I anſwer, neither has the exiſtence of the leſſer muſcular ring been yet proved in the ſame manner.

But it may be ſaid, although the exiſtence of the leſſer muſcular ring has not been demonſtrated by ocular inſpection, yet it is juſtly inferred from its effect, the contraction of the pupil, which is viſible, and is no other way to be accounted for, but by ſuppoſing the exiſtence of ſuch a muſcular ring.

I

* Theſ. Anatom. II. p. 87. † Ibid. p. 14.

I anſwer, the change of conformation, in adapting the eye to very near objects, is no leſs certain than the contraction of the pupil: And this change of conformation has not yet been adequately accounted for, but may be fairly made out by ſuppoſing the exiſtence of the greater muſcular ring, as I propoſe to ſhew by-and-by.

130. The cryſtalline humour is contained in a very fine membranous *capſula*, with a water between them, after the manner of the heart in the *pericardium*.

This I take from the obſervations of the late Anatomiſts, particularly the famous Monſ. * PETIT, from whom I muſt likewiſe obſerve, that the † back part of this *capſula*, or that part which inveſts the hinder ſurface of the cryſtalline humour, adheres to the membrane encloſing the vitreous humour, yet ſo as to be ſeparated from it without cutting: but that all along the limb or edge of the cryſtalline theſe two membranes adhere ſo firmly together, as not to be parted without the knife.

I muſt alſo take notice that, from the meaſures taken by this diligent and accurate Anatomiſt, as well as from autopſy, it appears that the figure of this compound body conſiſting of the cryſtalline humour, the water ſurrounding it, and the *capſula* containing them both, is ſuch as would ariſe from two ſegments of equal breadth, but of unequal ſpheres, clapt together on their plane ſides, and having the ſharp edge rounded off, ſo as to leave an obtuſe limb or edge of ſome conſiderable thickneſs, by which means the attachment of the edge of the *capſula* to the membrane of the vitreous humour all along that edge is rendred much ſtronger than it could otherwiſe be.

And to render this attachment ſtill ſtronger, I have obſerved the limb of the *capſula* to be indented all round with ſhallow tranverſe *ſulci*, or furrows, ſeemingly perpendicular to the limb, into which furrows I ſuppoſe the membrane of the vitreous humour is all along inſerted.

Thus much from obſervation, and till it ſhall be otherwiſe determined by experiment, I take leave for facility of calculation to ſuppoſe, that the *capſula*, the water within it, and the cryſtalline humour itſelf have all of them one and the ſame refractive power.

131. The *ligamentum ciliare* is a muſcle compoſed of longitudinal fibres, and is much weaker than the *uvea*. It ariſes cloſe behind the *uvea*, from the abovementioned circular ridge at the juncture of the *cornea* and *ſclerotica*, and running over the outer edge of the vitreous humour is inſerted all round the anterior ſurface of the *capſula*, upon which, ſays Monſ. ‡ PETIT, this ligament prolongs its fibres and the veſſels which it furniſhes to the *capſula*.

Now as that part of the *capſula*, into which theſe muſcular fibres and veſſels are inſerted, muſt thereby be rendred ſomething leſs diaphanous than the reſt, it is probable that this inſertion does not extend far enough towards the middle of the *capſula*, to be in the way of the rays that paſs the pupil in its greateſt dilatation.

132. If what is contained in the four preceding articles be allowed me, I think the change of conformation in the eye, to ſee objects diſtinctly at different diſtances, may be explained in the following manner.

When the eye is perfectly at reſt, no force, ſtrain, or effort of any kind being uſed by any of its parts, it is then ſuited to ſee with *Perfect Viſion* at ſome one determinate moderate diſtance.

This diſtance, I ſuppoſe, is for moſt eyes about 15 or 16 inches, the uſual diſtance for reading a print of a middle ſize. For it is likely, we uſually read at that diſtance, where viſion is perfect without any ſtraining of the eye, and at which conſequently we read with moſt eaſe, and can continue it longeſt.

133. When we view objects nearer than the diſtance of 15 or 16 inches, I ſuppoſe the greater muſcular ring of the *uvea* contracts, and thereby reduces the *cornea* to a greater convexity. And when we ceaſe to view theſe near objects, this muſcular ring ceaſes to act, and the *cornea* by its ſpring returns to its uſual convexity ſuited to 15 or 16 inches. In which condition the elaſticity of the *cornea* on the one ſide, and the tone of the muſcular ring on the other, may be conſidered as two antagoniſts in a perfect *equilibrium*. Eye alte[illegible] nea[illegible] ject[illegible]

134. When the eye is to be ſuited to greater How tered remo[illegible]

* Memoires de l'Acad. Royale 1730. † Ibid. p. 436. ‡ Ibid. p. 438.

distances than 15 or 16 inches, I suppose the *ligamentum ciliare* to contract its longitudinal fibres, and by that means to draw the part of the anterior surface of the *capsula*, into which these fibres are inserted, a little forwards and outwards. And at the time this is done, the water within the *capsula* must necessarily flow from under the middle towards the elevated part of the *capsula*, and the aqueous humour must flow from above the elevated part of the *capsula* to the middle. Consequently, the middle part of the anterior surface of the *capsula* must a little sink, while the other is elevated, or the whole anterior surface within the insertion of the *ligamentum ciliare* must be reduced to a less convexity. And when the contraction of the *ligamentum ciliare* ceases, the anterior part of the *capsula*, which has been put a little upon the strain by that contraction, will by its elasticity recover itself and return to its former figure. In which condition the elasticity of the *capsula* and the tone of the ligament may also be looked upon as two antagonists perfectly *in æquilibrio* with one another.

This *capsula* as it is a very tender membrane, and contains a water between its inner surface and the crystalline, can readily obey the effort of so weak a muscle as the *ligamentum ciliare*, which would not be sufficient to flatten the crystalline itself, considering the firmness of its contexture. And hence appears the true use of the *capsula* and the water within it.

Here possibly it may be thought, that the *ligamentum ciliare* might as well have been made stronger, and have been inserted into the crystalline itself, in which case it had been sufficient to have drawn outwards and flattened the crystalline without all this *apparatus* of a *capsula* and water within it.

But this would not so well have answered the end proposed. For the *ligamentum ciliare* arises from the edge of the *cornea* at its union with the *sclerotis*, close to the *uvea*; and consequently when the longitudinal fibres of such a stronger *ligamentum ciliare* had shortened themselves, they must not only have drawn the crystalline outwards, but must have drawn the *cornea* inwards, that is, they must not only have lessened the convexity of the crystalline, but must have increased the convexity of the *cornea*: And these two effects would have been contrary to one another, the flattening of the crystalline tending to suit the eye to remoter objects, and the increasing the convexity of the *cornea* tending to suit the eye to nearer objects. Whereas the *ligamentum ciliare*, being made so weak, cannot sensibly affect the *cornea*, and yet by means of this admirable contrivance of a *capsula* and water within it, is sufficient for the intended effect.

I need not take notice, that such a stronger *ligamentum ciliare* might by its contraction have endangered the disuniting the crystalline humour from the vitreous.

I had once thought, that both surfaces of the *capsula* were rendred less convex by its edge being drawn a little outwards, and had formed my computations from that notion. But upon considering the close attachment of the hinder surface of the *capsula* to the membrane of the vitreous humour, particularly the firm adhesion of these two membranes at the edge of the *capsula*, as likewise the situation of the anterior and outer part of of the vitreous humour, such as that it must necessarily obstruct the drawing the edge of the *capsula* outwards, and especially upon calling to mind the situation and insertion of the *ligamentum ciliare*, I found the suiting the eye to distant objects could not be performed but by the flattening of the anterior surface only.

135. This may suffice to give a general notion of the manner, by which the eye alters its conformation according to the different distances of objects;

Appendix II

V. ON THE

MECHANISM OF THE EYE.

BY

THOMAS YOUNG, M.D. F.R.S.

FROM THE PHILOSOPHICAL TRANSACTIONS.

Read before the ROYAL SOCIETY, *November* 27, 1800.

I. IN the year 1793, I had the honour of laying before the Royal Society some observations, on the faculty, by which the eye accommodates itself to the perception of objects at different distances*. The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it †; an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens ‡. I therefore thought it incumbent on me, to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a Dissertation published at Gottingen in 1796 §, and also in an Essay presented last year to this Society ‖. About three months ago, I was induced to resume the subject, by perusing Dr. Porterfield's paper on the internal motions of the eye ¶; and I have very unexpectedly made some observations, which, I think I may venture to say, appear to be

* Phil. Trans. 1793. 169.
† Phil. Trans. 1794. 21.
‡ Phil. Trans. 1795. 1.
§ De Corporis humani Viribus conservatricibus, p. 68.
‖ Phil. Trans. 1800. 146.
¶ Edinb. Med. Essays. IV. 124.

finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person, who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

II. Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then describe an instrument for readily ascertaining the focal distance of the eye; and with the assistance of this instrument, I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture, which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally deserving of comparison. I shall proceed to relate a variety of experiments, which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments, as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III. Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so diversified are its powers, that many of them have been hitherto uninvestigated: and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come, almost unprepared, into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and of its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound, to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive hard bodies, obeying the motions of the air nearly in their whole extent at the same instant. In the eye, we judge very precisely of the direction of light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct; and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Supposing the eye capable of conveying a distinct idea of two points subtending an angle of a minute, which is, perhaps, nearly the smallest interval at which two objects can be distinguished, although a line, subtending only one tenth of a minute in breadth, may sometimes be perceived as a single object; there must, on this supposition, be about 360 thousand sentient points, for a field of view of 10 degrees in diameter, and above 60 millions for a field of 140 degrees. But, on account of the various sensibility of the retina, to be explained hereafter, it is not necessary to suppose, that there are more than 10 million sentient points, nor can there easily be less than one million: the optic nerve may, therefore, be judged to consist of several millions of distinct fibres. By a rough experiment, I find, that I can distinguish two similar sounds proceeding from points which subtend an angle of about five degrees. But the eye can discriminate, in a space subtending every way five degrees, about 90 thousand different points. Of such spaces, there are more than a thousand in a hemisphere: so that the ear can convey an impression of about a thousand different directions. The ear has not, however, in all cases, quite so nice a discrimination of the directions of sounds: the reason of this difference between the eye and ear is obvious; each point of the retina has only three principal colours to perceive, since the rest are probably composed of various proportions of these; but there being many thousands or millions of varieties of sound audible in each direction, it was impossible that the number of distinguishable directions should be very large. It is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the very great diversity of tones that we can distinguish, in the same manner as each sensitive point of the retina receives a distinct impression of the colour, as well as of the strength, of the light which falls on it; although it is extremely probable, that all the different parts of the surface, exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession, according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects, in the same instant, cannot easily be determined; but it is sufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary, perception of two or more parts of the same object. The extent of the field of perfect vision, for each position of the eye, is certainly not very great; although it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

IV. Dr. Porterfield has applied an experiment, first made by Scheiner *, to the determination of the focal distance of the eye; and has described, under the name of an optometer, a very excellent instrument, founded on the principle of the phenomenon †. But the apparatus is capable of considerable improvement; and I shall beg leave to describe an optometer, simple in its construction, and equally convenient and accurate in its application.

* Priestley's opt. 113.

† Edinb. Med. Ess. IV. 185.

Let an obstacle be interposed between a radiant point (R, Plate 15. Fig. 109,) and any refracting surface, or lens (CD), and let this obstacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, so as to form an image on it. Then it is evident, that when this plane (EF) passes through the focus of refracted rays, the image formed on it will be a single point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the small pencils, passing through the perforations, will no longer meet in a single point, but will fall on two distinct spots of the plane (G, H; I, K:) and, in either case, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 110,) the one nearer to the lens than the first point, the other more remote; and, when the plane, which receives the images, passes through the focus of rays coming from the first point, the images of the second and third points must both be double (*s s*, *t t*;) since the plane (EF) is without the focal distance of rays coming from the furthest point, and within that of rays coming from the nearest. Upon this principle, Dr. Porterfield's optometer was founded.

But, if the three points be supposed to be joined by a line, and this line to be somewhat inclined to the axis of the lens, each point of the line, except the first point (R, Fig. 111,) will have a double image; and each pair of images, being contiguous to those of the neighbouring radiant points, will form with them two continued lines; and the images being more widely separated as the point which they represent is further from the first radiant point, the lines (*s t*; *s t*,) will converge on each side towards (*r*) the image of this point, and there will intersect each other.

The same happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect vision, the image on the retina will be single; but, in every other case, the image being double, we shall appear to see a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, crossing each other in the point of perfect vision. For this purpose, the holes may be converted into slits, which render the images nearly as distinct, at the same time that they admit more light. The number may be increased from two to four, or more, whenever particular investigations render it necessary.

This instrument has the advantage of showing the focal distance correctly, by inspection only, without sliding the object backwards and forwards, which is an operation liable to considerable uncertainty, especially as the focus of the eye may in the mean time be changed.

The optometer may be made of a slip of card paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which must not be too strong. The end of the card must be cut as is shown in Plate 9. Fig. 71, in order that it may be turned up, and fixed in an inclined position by means of the shoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved (Fig. 72.). A hole about half an inch square must be made in this part; and the sides so cut as to receive a slider of thick paper, with slits of different sizes, from a fortieth to a tenth of an inch in breadth, divided by spaces somewhat broader; so that each observer may choose that which best suits the aperture of his pupil. In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens of four inches focal length; and a scale must be made near the line on each side of it, divided from one end into inches, and from the other according to the table here calculated, by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus. If ivory be employed, its surface must be left without any polish, otherwise the regular reflection of light will create confusion; and in this respect, paper is much preferable.

The instrument is easily applicable to the purpose of ascertaining the focal length of spectacles required for myopic or presbyopic eyes. Mr. Cary has been so good as to furnish me with the numbers and focal lengths of the glasses commonly made; and I have calculated the distances at which those numbers must be placed on the scale of the optometer, so that a presbyopic eye may be enabled to see at eight inches distance, by using the glasses of the focal length placed opposite to the nearest crossing of the lines; and a myopic eye, with parallel rays, by using the glasses indicated by the number that stands opposite their furthest crossing. It cannot be expected, that every person, on the first trial, will fix precisely upon that power which best suits the defect of his sight. Few can bring their eyes at pleasure to the state of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus ascertained, will be found sufficient for ordinary purposes. I have also added to the second table, such numbers as will point out the spectacles necessary for a presbyopic eye, to see at twelve and at eighteen inches respectively: the middle series will perhaps be the most proper for placing the numbers on the scale. The optometer should be applied to each eye; and, at the time of observing, the opposite eye should not be shut, but the instrument should be screened from its view. The place of intersection may be accurately ascertained, by means of an index sliding along the scale.

The optometer is represented in Plate 9. Fig. 72 and 73; and the manner in which the lines appear, in Fig. 74.

Table I. *For extending the scale by a lens of 4 inches focus.*

4	2.00	13	3.06	70	3.76	—40	4.44	—11	6.29
5	2.22	14	3.11	80	3.81	—35	4.51	—10	6.67
6	2.40	15	3.16	100	3.85	—30	4.62	— 9.5	6.90
7	2.55	20	3.33	200	3.92	—25	4.76	— 9.0	7.20
8	2.67	25	3.45	∞	4.00	—20	5.00	— 8.5	7.56
9	2.77	30	3.52	—200	4.08	—15	5.45	— 8.0	8.00
10	2.86	40	3.64	—100	4.17	—14	5.60		
11	2.93	50	3.70	—50	4.35	—13	5.78		
12	3.00	60	3.75	—45	4.39	—12	6.00		

Table II. *For placing the numbers indicating the focal length of convex glasses.*

Foc.	VIII.	XII.	XVIII.
00	8.00	12.00	18.00
40	10.00	17.14	32.73
36	10.28	18.00	36.00
30	10.91	20.00	45.00
28	11.20	21.00	50.40
26	11.56	22.29	58.50
24	12.00	24.00	72.00
22	12.77	26.40	99.00
20	13.33	30.00	180.00
18	14.40	36.00	∞
16	16.00	48.00	—144.00
14	18.67	84.00	— 63.00
12	24.00	∞	— 36.00
11	29.33	—132.00	— 28.29
10	40.00	— 60.00	— 22.50
9	72.00	— 36.00	— 18.00
8	∞	— 24.00	— 14.40
7	—56.00	— 16.80	— 11.45
6	—24.00	— 12.00	— 9.00
5	—13.33	— [illegible]	— 5.92
4.5	—10.29	— 7.20	— 6.00
4.0	— 8.00	— 6.00	— 5.14
3.5	— 6.22	— 4.94	— 4.34
3.0	— 4.80	— 4.00	— 3.69

Table III. *For concave glasses.*

Number.	Focus.	Number.	Focus.	Number.	Focus.
1	24	8	7	15	2.75
2	18	9	6	16	2.50
3	16	0	5	17	2.25
4	12	11	4.5	18	2.00
5	10	12	4.0	19	1.75
6	9	13	3.5	20	1.50
7	8	14	3.00		

V. Being convinced of the advantage of making every observation with as little assistance as possible, I have endeavoured to confine most of my experiments to my own eyes; and I shall, in general, ground my calculations on the supposition of an eye nearly similar to my own. I shall therefore first endeavour to ascertain all its dimensions, and all its faculties.

For measuring the diameters, I fix a small key on each point of a pair of compasses; and I can venture to bring the rings into immediate contact with the sclerotica. The transverse diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its versed sine, 11 hundredths. To ascertain the versed sine, I looked with the right eye at the image of the left, in a small speculum held close to the nose, while the left eye was so averted, that the margin of the cornea appeared as a straight line, and I then compared the projection of the cornea with the image of a cancellated scale held in a proper direction behind the left eye, and close to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence the radius of the cornea is 31 hundredths. It may be thought, that I assign too great a convexity to the cornea; but I have verified it by a number of concurrent observations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the sclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: so that the cornea has an eccentricity of one fortieth of an inch, with respect to the section of the eye perpendicular to the visual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at least this is its apparent size, which must be somewhat diminished, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when most contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the sclerotica: so that the cornea is perpendicularly intersected by the visual axis.

My eye, in a state of relaxation, collects, to a focus on the retina, those rays which diverge vertically from an object at the distance of ten inches from the cornea, and the rays which diverge horizontally from an object at seven inches distance. For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches; if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments; and I believe I can examine minute objects with as much accuracy as most of those whose eyes are differently formed. On mentioning it to Mr. Cary, he informed me that he had frequently taken notice of a similar circumstance; that many persons were obliged to hold a concave glass obliquely, in order to see with distinctness, counterbalancing, by the inclination of the glass, the too great refractive power of the eye in the direction of that inclination, and finding but little assistance from common spectacles of the same focal length. The difference is not in the cornea, for it exists when the effect of the cornea is removed, by a method to be described hereafter. The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimensions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference observed is found to require an inclination of about 13 degrees; and the remaining three degrees may easily be added, by the greater obliquity of the posterior surface of the crystalline opposite the pupil. There would be no difficulty in fixing the glasses of spectacles, or the concave eye glass of a telescope, in such a position as to remedy the defect.

In order to ascertain the focal distance of the lens, we must assign its probable distance from the cornea. Now the versed sine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior surface of the lens must probably be somewhat behind the chord of the cornea; but by a very inconsiderable distance, for the uvea has the substance of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. Petit found the axis almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{9}$ of an inch*. The optical centre will be therefore $\left(\frac{18\times30}{30+22}=\right)$ about one tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. But the actual joint focus is $(91-22=)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. For the index of its refractive power in the eye, we have $\frac{145}{115}$. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the sines. It is well known that the refractive powers of the humours are equal to that of

* Mém. de l'Acad. de Paris. 1730. 6. Ed. Amst.

water; and, that the thickness of the cornea is too equable to produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. Wollaston. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that, the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but, the central part being the most dense, the whole acts as a lens of smaller dimensions: and it may be found by calculation (M. E. 465.) that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere, thus constituted, will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapidly externally than internally. Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portions of such a sphere, the refractive density at the centre of this lens must be as 18 to 17. On the whole, it is probable that the refractive power of the centre of the human crystalline, in its living state, is to that of water nearly as 18 to 17; that the water, imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable density of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole size. Dr. Wollaston has ascertained the refraction out of air, into the centre of the recent crystalline of oxen and sheep, to be nearly as 143 to 100; into the centre of the crystalline of fish, and into the dried crystalline of sheep, as 152 to 100. Hence, the refraction of the crystalline of oxen, in water, should be as 15 to 14: but the human crystalline, when recent, is decidedly less refractive.

These considerations will explain the inconsistency of different observations on the refractive power of the crystalline; and, in particular, how the refraction which I formerly calculated, from measuring the focal length of the lens*, is so much greater than that which is determined by other means. But, for direct experiments, Dr. Wollaston's method is exceedingly accurate.

When I look at a minute lucid point, such as the image of a candle in a small concave speculum, it appears as a radiated star, as a cross, or as an unequal line, and never as a perfect point, unless I apply a concave lens, inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it spreads into a surface nearly circular, and almost equably illuminated, except some faint lines, nearly in a

* Phil. Trans. 1793. 174.

radiating direction. For this purpose, the best object is a candle or a small speculum, viewed through a minute lens at some little distance, or seen by reflection in a larger lens. If any pressure has been applied to the eye, such as that of the finger keeping it shut, the sight is often confused for a short time after the removal of the finger, and the image is in this case spotty or curdled. The radiating lines are probably occasioned by some slight inequalities in the surface of the lens, which is very superficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longest, and the lines a little more distinct than before, the light being strongest in the neighbourhood of the centre; but immediately at the centre there is a darker spot, owing to such a slight depression at the vertex as is often observable in examining the lens after death. The situation of the rays is constant, though not regular; the most conspicuous are seven or eight in number; sometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a short vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are still separate. In the next stage, which is the most perfect focus, the line spreads in the middle, and approaches nearly to a square, with projecting angles, but is marked with some darker lines towards the diagonals. The square then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater distance, the line lengthens, and acquires also breadth, by radiations shooting out from it, but does not become a uniform surface, the central part remaining always considerably brightest, in consequence of the same flattening of the vertex which before made it faintest. Some of these figures bear a considerable analogy to the images derived from the refraction of oblique rays, and still more strongly resemble a combination of two of them in opposite directions; so as to leave no doubt, but that both surfaces of the lens are oblique to the visual axis, and cooperate in distorting the focal point. This may also be verified, by observing the image delineated by a common glass lens, when inclined to the incident rays. (Plate 12. Fig. 92. n. 28. . 40.)

The visual axis being fixed in any direction, I can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. These internal limits of the field of view nearly correspond with the external limits formed by the different parts of the face, when the eye is directed forwards and somewhat downwards, which is its most natural position; although the internal limits are a little more extensive than the external: and both are well calculated for enabling us to perceive, the most readily, such objects as are the most likely to concern us. Dr. Wollaston's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed. It has been said, that a faint light, like the tail of a comet, is more observable by a lateral than by a direct view. Supposing the fact certain, the reason probably is, that general masses of light and shade are more distinguishable when the parts are somewhat confused, than when the whole is rendered perfectly distinct; thus I have often observed the pattern of a paper or floor cloth to run in certain lines, when I viewed it without my glass; but these lines vanished as soon as the focus was rendered perfect. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation, and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction: so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the successive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens, from this point, will always tend to correct the unequal refraction of oblique rays. The first curve (Plate 10. Fig. 80.) is the image formed by the furthest intersection of rays refracted at the cornea; the second, the image formed by the nearest intersection; the distance, between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might easily be estimated; and probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In Bernoulli's eye, a greater deviation was required for the direction of the axis*; and the obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest distances from the cornea to the perforation made by the nerve, and their difference was exactly one fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears, that the visual axis is five hundredths, or one twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible, that this distance may be different in different eyes: in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, when looking at any object before it, although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly, as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two sevenths of the distance between them. Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed intersection of the principal rays;

* Comm. Petrop. I. 314.

(Plate 9. Fig. 75.) and the distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express, in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Plate 11. Fig. 81.) is a vertical section; the second (Fig. 82.) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 83 and 84.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens, as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical* or elliptical: in the interior laminæ indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts must greatly tend to correct the aberration, arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of rays: and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts; and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. Ramsden's observation†, on the advantage of having no reflecting surface, may be well founded: but it has not been demonstrated, that less light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; although such a conclusion may certainly be inferred, from the only hypothesis which affords an explanation of the cause of a partial reflection in any case. But neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this, long ago‡, from observing the colour bordering the image of an object seen indistinctly. Dr. Wollaston pointed out to me, on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space. The observation is confirmed, by placing a small concave speculum in dif-

* Petit. Mém. de l'Acad. 1725. 20.

† Phil. Trans. 1795. 2. ‡ Smith, c. 96.

ferent parts of a prismatic spectrum; and ascertaining the utmost distances, at which the eye can collect the rays of different colours to a focus. By these means I find, that the red rays, from a point at 12 inches distance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays, in a lens of crown glass, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the dispersive power of the eye, collectively, is one third of the dispersive power of crown glass, at an equal angle of deviation. I cannot observe much aberration in the violet rays. This may be, in part, owing to their faintness; but yet I think their aberration must be less than that of the red rays. I believe it was Mr. Ramsden's opinion, that since the separation of coloured rays is only observed where there is a sudden change of density, such a body as the lens, of a density gradually varying, would have no effect whatever in separating the rays of different colours. If this hypothesis should appear to be well founded, we should be obliged to attribute the whole dispersion to the aqueous humour; and its dispersive power would be half that of crown glass, at the same deviation. But we have an instance, in the atmosphere, of a very gradual change of density; and yet Mr. Gilpin informs me, that the stars, when near the horizon, appear very evidently coloured; and Dr. Herschel has even given us the dimensions of a spectrum thus formed. At a more favourable season of the year, it would not be difficult to ascertain, by means of the optometer, the dispersive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the dispersive power of the whole eye been equal to that of flint glass, the distances of perfect vision would have varied from 12 inches to 7, for different rays, in the same state of the mean refractive powers.

VI. The faculty of accommodating the eye to various distances appears to exist in very different degrees in different individuals. The shortest distance of perfect vision, in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. Wollaston can see at seven inches, and with rays slightly converging; the difference answering to 6 inches focal length. Mr. Abernethy has perfect vision from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can see at 2 inches and at 4; the difference being equivalent to 4 inches focus: a middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reason to think, that the faculty diminishes, in some measure, as persons advance in life; but some also of a middle age appear to possess it in a very small degree. I shall take the range of my own eye, as being probably about the medium, and inquire what changes will be necessary, in order to produce it; whether we suppose the radius of the cornea to be diminished, or the distance of the lens from the retina to be increased, or these two causes to act conjointly, or the figure of the lens itself to undergo an alteration.

1. We have calculated, that when the eye is in a state of relaxation, the refraction of the cornea is such as to collect rays diverging from a point ten inches distant, to

a focus at the distance of $13\frac{1}{7}$ tenths. In order that it may bring, to the same focus, rays diverging from a point distant 29 tenths, we shall find that its radius must be diminished from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Supposing the change from perfect vision at ten inches, to perfect vision at 29 tenths, to be effected by a removal of the retina to a greater distance from the lens, this will require an elongation of 135 thousandths, or more than one seventh of the diameter of the eye. In Mr. Abernethy's eye, an elongation of 17 hundredths, or more than one sixth, is requisite.

3. If the radius of the cornea be diminished one sixteenth, or to 29 hundredths, the eye must at the same time be elongated 97 thousandths, or about one ninth of its diameter.

4. Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. This is more than double the actual change. But it is impossible to determine precisely, how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed. If it were always a spheroid, more or less oblate, the focal length of each surface would vary inversely as the square of the axis: but, if the surfaces became, from spherical, portions of hyperbolic conoids, or of oblong spheroids, or changed from more obtuse to more acute figures of this kind, the focal length would vary more rapidly. Disregarding the elongation of the axis, and supposing the curvature of each surface to be changed proportionally, the radius of the anterior must become about 21, and that of the posterior 15 hundredths.

VII. I shall now proceed to inquire, which of these changes takes place in nature; and I shall begin with a relation of experiments, made in order to ascertain the curvature of the cornea in all circumstances.

The method, described in Mr. Home's Croonian Lecture for 1795*, appears to be far preferable to the apparatus of the preceding year†; for a difference in the distance of two images, seen in the cornea, would be far greater, and more conspicuous, than a change of its prominency, and far less liable to be disturbed by accidental causes. It is nearly, and perhaps totally, impossible to change the focus of the eye, without some motion of its axis. The eyes sympathize perfectly with each other; and the change of focus is almost inseparable from a change of the relative situation of the optic axes; so much, that, in my eye this sympathy causes a slight imperfection of sight; for, if I direct both my eyes to the same object, even if it is beyond their furthest focus, I cannot avoid contracting, in some degree, their focal distance: now while one axis moves, it is not easy to keep the other perfectly at rest; and, besides, it is not impossible, that a change in the proportions of some eyes may render a slight alteration of the position of the axis absolutely necessary. These considerations may partly explain the trifling difference in the place of the cornea that was observed in 1794. It appears that the experiments of 1795 were made with considerable accuracy, and no doubt, with excellent instruments; and their failing to ascertain the existence of any change induced Mr. Home

* Phil. Trans. 1796. 2. † Phil. Trans. 1795. 12.

and Mr. Ramsden to abandon, in great measure, the opinion which suggested them, and to suppose, that a change of the cornea produces only one third of the effect. Dr. Olbers, of Bremen, who in the year 1780 published a most elaborate dissertation on the internal changes of the eye*, which he lately presented to the Royal Society, had been equally unsuccessful in his attempts to measure this change of the cornea, at the same time that his opinion was in favour of its existence.

Room was however still left for a repetition of the experiments; and I began with an apparatus nearly resembling that which Mr. Home has described. I had an excellent achromatic microscope, made by Mr. Ramsden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microscope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microscope was compared with this scale, care was taken to place the head of the observer so that the relative motion of the image on the micrometer, caused by the unsteadiness of the optic axes, should always be in the direction of the horizontal lines, and that there could be no error from this motion, in the dimensions of the image taken vertically. I placed two candles so as to exhibit images in a vertical position in the eye of Mr. König, who had the goodness to assist me; and, having brought them into the field of the microscope, where they occupied 35 of the small divisions, I desired him to fix his eye on objects at different distances in the same direction: but I could not perceive the least variation in the distance of the images.

Finding a considerable difficulty in a proper adjustment of the microscope, and being able to depend on my naked eye in measuring distances, without an error of one 500th of an inch, I determined to make a similar experiment without any magnifying power. I constructed a divided eye glass of two portions of a lens, so small, that they passed between two images reflected from my own eye: and, looking in a glass, I brought the apparent places of the images to coincide, and then made the change requisite for viewing nearer objects; but the images still coincided. Neither could I observe any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye glass. But, not being at that time aware of the perfect sympathy of my eyes, I thought it most certain to confine my observation to the one with which I saw. I must remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance.

I also stretched two threads, a little inclined to each other, across a ring, and divided them, by spots of ink, into equal spaces, I then fixed the ring, applied my eye close behind it, and placed two candles in proper situations before me, and a third on one side, to illuminate the threads. Then, setting a small looking glass, first at four inches distance, and next at two, I looked at the images reflected in it, and observed at what part of the threads they exactly reached across in each case; and with the same result as before.

* De Oculi Mutationibus internis. 4. Gotting. 1780.

I next fixed the cancellated micrometer at a proper distance, illuminated it strongly, and viewed it through a pin hole, by which means it became distinct in every state of the eye; and, looking with the other eye into a small glass, I compared the image with the micrometer, in the manner already described. I then changed the focal distance of the eye, so that the lucid points appeared to spread into surfaces, from being too remote for perfect vision; and I noted, on the scale, the distance of their centres; but that distance was invariable.

Lastly, I drew a diagonal scale, with a diamond, on a looking glass, (Plate 9. Fig. 76.) and brought the images into contact with the lines of the scale. Then, since the image of the eye occupies, on the surface of a glass, half its real dimensions, at whatever distance it is viewed, its true size is always double the measure thus obtained. I illuminated the glass strongly, and made a perforation in a narrow slip of black card, which I held between the images; and was thus enabled to compare them with the scale, although their apparent distance was double that of the scale. I viewed them in all states of the eye; but I could perceive no variation in the interval between them.

The sufficiency of these methods may be thus demonstrated. Make a pressure along the edge of the upper eyelid with any small cylinder, for instance a pencil, and the optometer will show that the focus of horizontal rays is a little elongated, while that of vertical rays is shortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here described, but even the eye unassisted, will be capable of discovering a considerable change in the images reflected from the cornea, although the change be much smaller than that which is requisite for the accommodation of the eye to different distances. On the whole, I cannot hesitate to conclude, that if the radius of the cornea were diminished but one twentieth, the change would be very readily perceptible by some of the experiments related; and the whole alteration of the eye requires one fifth.

But a much more accurate and decisive experiment remains. I take, out of a small botanical microscope, a double convex lens, of eight tenths radius and focal distance, fixed in a socket one fifth of an inch in depth; securing its edges with wax, I drop into the socket a little water, nearly cold, till three fourths full, and then apply it to my eye, so that the cornea enters half way into it, and is every where in contact with the water. (Plate 9. Fig. 77). My eye immediately becomes presbyopic, and the refractive power of the lens, which is reduced by the water to a focal length of about 16 tenths, is not sufficient to supply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, restores my eye to its natural state, and somewhat more. I then apply the optometer, and I find the same inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. At first sight indeed, the accommodation appears to be somewhat less, and only able to bring the eye from the state fitted for parallel rays to a focus at five inches distance; and this made me once imagine, that the cornea might have some slight effect in the natural state; but, considering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly sufficient to account for the diminution of the range of vision. I cannot ascertain the distance of the glass lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either side.

After this, it is almost necessary to apologize for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence.

VIII. Having satisfied myself, that the cornea is not concerned in the accommodation of the eye, my next object was, to inquire if any alteration in the length of its axis could be discovered; for this appeared to be the only possible alternative: and, considering that such a change must amount to one seventh of the diameter of the eye, I flattered myself with the expectation of submitting it to measurement. Now, if the axis of the eye were elongated one seventh, its transverse diameter must be diminished one fourteenth, and the semidiameter would be shortened a thirtieth of an inch.

I therefore placed two candles so that when the eye was turned inwards, and directed towards its own image in a glass, the light reflected from one of the candles by the sclerotica appeared upon its external margin, so as to define it distinctly by a bright line: and the image of the other candle was seen in the centre of the cornea. I then applied the double eye glass, and the scale of the looking glass, in the manner already described; but neither of them indicated any diminution of the distance, when the focal length of the eye was changed.

Another test, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as possible, and confined at the same time by a strong oval iron ring, pressed against it at the internal angle. The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by a moderate pressure, between the eye and the bone. In this situation, the phantom, caused by the pressure, extended within the field of perfect vision, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a distinct perception of the objects actually seen in that direction; and a straight line, coming within the field of this oval phantom, appeared somewhat inflected towards its centre; (Plate 9. Fig. 78.) a distortion easily understood by considering the effect of the pressure on the form of the retina. Supposing now the distance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye must have been either totally or very nearly prevented; and, instead of an increase of the length of the eye's axis, the oval spot, caused by the pressure, would have spread over a space at least ten times as large as the most sensible part of the retina. But no such circumstance took place. the power of accommodation was as extensive as ever; and there was no perceptible change, either in the size or in the figure of the oval spot.

Again, since the rays which pass through the centre of the pupil, or rather through the anterior vertex of the lens, may be considered as delineating the image; and, since the divergence of these rays, with respect to each other, is but little affected by the refraction of the lens, they may still be said to diverge from the centre of the pupil; and the image of a given object on the retina must be very considerably enlarged, by the removal of the retina to a greater distance from the pupil and the lens. To ascertain the real magnitude of the image, with accuracy, is not so easy as at first sight appears; but, besides the experiment last related, which might be employed as an argument to this purpose, there are two other methods of estimating it. The first is too hazardous to be of much use; but, with proper precautions, it may be attempted. I fix my eye on a brass circle placed in the rays of the sun, and, after some time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given distance, I endeavour to discover whether there is any difference in the apparent magnitude of the spectrum on the scale; but I can discern none. I have not insisted on the attempt; especially as I have not been able to make the spectrum distinct enough without inconvenience; and no light is sufficiently strong to cause a permanent impression on any part of the retina remote from the visual axis. I therefore had recourse to another experiment. I placed two candles so as exactly to answer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Plate 9. Fig. 79.) But this did not happen: the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole.

It may be inquired, if no change in the magnitude of the image is to be expected on any other supposition; and it will appear to be possible, that the changes of curvature may be so adapted, that the magnitude of the confused image may remain perfectly constant. Indeed, to calculate from the dimensions which we have hitherto used, it would be expected that the image should be diminished about one fortieth, by the utmost increase of the convexity of the lens. But the whole depends on the situation of the refracting surfaces, and the respective increase of their curvature, which, on account of the variable density of the lens, can scarcely be estimated with sufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image must, on any supposition, have been very variable: at present, this inconvenience is avoided by the situation of the pupil; so that we have here an additional instance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place: and it is almost impossible to conceive by what power such a change could be effected. The straight muscles, with the adipose substance lying under them, would certainly, when acting independently of the socket, tend to flatten the eye: for, since their contraction would necessarily lessen the circumference or superficies of the mass that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye must therefore be brought nearer together. (Plate 11. Fig. 85, 86.) Dr. Olbers compares the muscles and the eye to a cone, of which the sides are protruded, and would by contraction be brought into a straight line. But this would require a force to preserve the cornea as a fixed point, at a given distance from the origin of the muscles; a force which certainly does not exist. In the natural situation of the visual axis, the orbit being conical, the eye might be somewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either side, the same action would rather shorten its axis: nor is there any thing about the human eye that could supply its place. In quadrupeds, the oblique muscles are wider than in man; and in many situations might assist in the effect. Indeed a portion of the orbicular muscle of the globe is attached so near to the nerve, that it might also cooperate in the action: and I have no reason to doubt the accuracy of Dr. Olbers, who states, that he effected a considerable elongation, by tying threads to the muscles, in the eyes of hogs and of calves; yet he does not say in what position the axis was fixed; and the flaccidity of the eye after death might render such a change very easy, as would be impossible in a living eye. Dr. Olbers also mentions an observation of Professor Wrisberg, on the eye of a man whom he believed to be destitute of the power of accommodation in his life time, and whom he found, after death, to have wanted one or more of the muscles: but this want of accommodation was not at all accurately ascertained. I measured, in the human eye, the distance of the attachment of the inferior oblique muscle from the insertion of the nerve: it was one fifth of an inch; and from the centre of vision, not a tenth of an inch; so that, although the oblique muscles do, in some positions, nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reason to agree with Winslow, in attributing to them the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shows that there is none.

Dr. Hosack alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object *: it does not appear that he made use of very accurate means for ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea, and in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. Olbers had treated it, and from being the last result of the investigations of Mr.

* Phil. Trans. 1794. 212.

Home and Mr. Ramsden. But either of the series of experiments, which have been related, appears to be sufficient to confute it.

IX. It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection, to the efficacy of a change of figure in the lens, was derived from the experiments, in which those, who have been deprived of it, have appeared to possess the faculty of accommodation.

My friend Mr. Ware, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. Porterfield, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described, as an improvement of Dr. Porterfield's optometer: and I must here acknowledge my great obligation to Mr. Ware, for the readiness and liberality, with which he introduced me to such of his numerous patients, as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrarily to the expectation with which I entered on the inquiry, that, in an eye deprived of the crystalline lens, the actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time that I saw him, a convenient apparatus.

I afterwards provided a small optometer, with a lens of less than two inches focus, adding a series of letters, not in alphabetical order, and projected into such a form as to be most legible at a small inclination. The excess of the magnifying power had the advantage of making the lines more divergent, and their crossing more conspicuous; and the letters served for more readily naming the distance of the intersection, and, at the same time, for judging of the extent of the power of distinguishing objects, too near, or too remote, for perfect vision. (Plate 11. Fig. 87.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to distinguish the letters at $2\frac{1}{2}$ inches, and at less than an inch. This at first persuaded me, that he must have a power of changing the focal distance: but I afterwards recollected that he had withdrawn his eye considerably, to look at the nearer letters, and had also partly closed his eyelids, no doubt contracting at the same time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The slider was not applied.

3. Miss H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the small optometer; but when she once saw an object double through the slits, no exertion could make it appear single at the same distance. She used for distant objects a glass of $4\frac{1}{2}$ inches focus; with this she could read as far off as

12 inches, and as near as five: for nearer objects she added another of equal focus, and could then read at 7 inches, and at $2\frac{1}{2}$.

4. Hanson, a carpenter, aged 63, had a cataract extracted a few years since from one eye: the pupil was clear and large, and he saw well to work with a lens of $2\frac{1}{2}$ inches focus; and could read at 8 and at 15 inches, but most conveniently at 11. With the same glass, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they crossed, the line being too strong, and the intersection too distant.

The experiment was afterwards repeated with the small optometer: he read the letters from 2 to 3 inches; but the intersection was always at $2\frac{1}{2}$ inches. He now fully understood the circumstances that were to be noticed, and saw the crossing with perfect distinctness: at one time, he said it was a tenth of an inch nearer; but I observed that he had removed his eye two or three tenths from the glass, a circumstance which accounted for this small difference.

5. Notwithstanding Hanson's age, I consider him as a very fair subject for the experiment. But a still more unexceptionable eye was that of Mrs. Maberly. She is about 30, and had the crystalline of both eyes extracted a few years since, but sees best with her right. She walks without glasses; and, with the assistance of a lens of about four inches focus, can read and work with ease. She could distinguish the letters of the small optometer from an inch to $2\frac{1}{2}$ inches; but the intersection was invariably at the same point, about 19 tenths of an inch distant. A portion of the capsule is stretched across the pupil, and causes her to see remote objects double, when without her glasses nor can she, by any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. Ware, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided that it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation, attributed, in the Croonian Lecture for 1794, to the eye of Benjamin Clerk. And it appears, that the distinction long since very properly made by Dr. Jurin, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided, at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced. Benjamin Clerk, with an aperture of $\frac{1}{20}$ of an inch, could read with the same glass at $1\frac{1}{2}$ inch, and at 7 inches*. With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches, without any real change of refractive power,

* Phil. Trans. 1795. 9.

and this is as great a difference as was observed in Benjamin Clerk's eye. It is also a fact of no small importance, that Sir Henry Englefield was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it †. This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

Hitherto I have endeavoured to show the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearances of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Plate 12. Fig. 92. n. 36. . 39.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides this necessary consequence, the light is also very differently distributed; the central part becomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (N. 41.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments: (N. 42, 44.) but, when the accommodation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which their concavity is directed. (N. 43, 45.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the relaxed state of the eye, are found to be in all parts straight; and, that neither the form nor the relative situation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by inspection of the optometer. When I look through four narrow slits, without exertion, the lines always appear to meet in one point: but when I make the intersection approach me, the two outer lines meet considerably beyond the inner ones, and the two lines of the same side cross each other at a still greater distance. (Plate 11. Fig. 88.)

The experiment will not succeed with every eye; nor can it be expected that such an imperfection should be universal: but one case is sufficient to establish the argu-

† Phil. Trans. 1795. 8.

ment, even if no other were found. I do not however doubt, that in those who have a large pupil, and great power of changing the focus, the aberration may be very frequently observable. In Dr. Wollaston's eye, the diversity of appearance is imperceptible; but Mr. König described the intersections exactly as they appear to me, although he had received no hint of what I had observed. The lateral refraction is the most easily ascertained, by substituting for the slits a tapering piece of card, so as to cover all the central parts of the pupil, and thus determining the nearest crossing of the shadows transmitted through the marginal parts only. When the furthest intersection was at 38, I could bring it to 22 parts with two narrow slits; but with the tapered card only to 29. From these data we may determine pretty nearly, into what form the lens must be changed, supposing both the surfaces to undergo proportional alterations of curvature, and taking for granted the dimensions already laid down: for, from the lateral aberration thus given, we may find the subtangents at about one tenth of an inch from the axis; and the radius of curvature, at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence, the anterior surface must be a portion of a hyperboloid, of which the greater axis is about 50; and the posterior surface will be nearly parabolical. In this manner, the change will be effected, without any diminution of the transverse diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the supposition with which we set out, the protrusion will be chiefly at the posterior vertex. The form of the lens, thus changed, will be nearly that of Plate 11. Fig. 90; the relaxed state being nearly as represented in Fig. 89. Should, however, the rigidity of the internal and more refractive parts, or any other considerations, render it convenient to suppose the anterior surface more changed, it would still have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any visible alteration of the external appearance of the eye.

Why, and in what cases, such an imperfection must exist in the lateral refraction, is easily understood, from the marginal attachment of the lens to its capsule. For, if the curvature at the axis be increased in any considerable degree, it cannot be continued far towards the margin, without lessening the diameter of the lens, and tearing the ramifications which enter it from the ciliary processes. Nor does there appear to be any other reason for the very observable contraction of the pupil, which always accompanies the effort to view near objects, than that by this means the lateral rays are excluded, and the indistinctness is prevented, which would have arisen from the insufficiency of their refraction.

From this investigation of the change of the figure of the lens, it appears that the action, which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The necessary effect of such an action would be, to produce a figure approaching to that of an oblate spheroid; and, to say nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increased than the central; nor would the slight change of density, at an equal distance from the axis, be at all equivalent to the increase of curvature: we must therefore suppose some different mode of action in the power producing the change. Now, whether we call the lens a muscle or not, it seems demonstrable, that such a change of figure takes place as can be produced by no external cause; and we may at least illustrate it by a comparison with the usual action of muscular fibres. A muscle never contracts, without at the same time swelling laterally, and it is of no consequence which of the effects we consider as primary. I was induced, by an occasional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but on a more accurate examination, nothing really analogous to tendon can be discovered. And, if it were supposed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction must be principally effected by the lateral parts of the fibres; so that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each surface to the belly of the muscle, it is easy to conceive their thickness to be immediately increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts cooperate more or less, according to their distance from the centre, and in different individuals in somewhat different proportions. On this supposition, we have no longer any difficulty in attributing a power of change to the crystalline of fishes. M. Petit, in a great number of observations, uniformly found the lens of fishes more or less flattened: but, even if it were not, a slight extension of the lateral part of the superficial fibres would allow those softer coats to become thicker at each vertex, and to form the whole lens into a spheroid somewhat oblong; and here, the lens being the only agent in refraction, a less alteration than in other animals would be sufficient. It is also worthy of inquiry, whether the state of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. Goddard attempted to show that muscles become more dense as they contract, such an effect might naturally be expected. That experiment is, however, very indecisive, and the opinion is indeed generally exploded, but perhaps too hastily; and whoever shall ascertain the existence or nonexistence of such a condensation, will render essential service to physiology in general. Some interesting experiments, on this subject, have been promised to the public by a very ingenious physiologist, who has probably employed a more decisive method of investigation in his researches. Swammerdam professes to have found such a condensation in the contraction of a muscle; but it is obvious, that what he has attributed to the heart properly belonged only to the air which it contained, and one of his experiments, which was free from this source of fallacy, does not appear to have shown any satisfactory result, although conducted with some accuracy, by inclosing a muscle in a bottle filled with water, communicating with a narrow open tube *.

Dr. Pemberton, in the year 1719, first systematically discussed the opinion of the muscularity of the crystalline lens †. He referred to Leeuwenhoek's microscopical observations; but he so overwhelmed his subject

* Book of Nature, II. 126, 127.

† De Facultate Oculi qua ad diversas rerum distantias se accommodat. L. B. 1719. Ap. Hall. Disp. Anat. IV. 301.

with intricate calculations, that few have attempted to develope it: he grounded the whole on an experiment borrowed from Barrow, which, with me, has totally failed; and I cannot but agree with Dr. Olbers in the remark, that it is easier to confute him than to understand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more just than the reasons adduced for its support. Lobé, or rather Albinus *, decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which he says that even the best microscopes can discover no fibres. Camper also mentions the hypothesis with considerable approbation †. Professor Reil published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor Gren's Journal ‡, he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect, that Professor Reil's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect, that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. Hunter's death, I pursued the experiment which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Plate 11. Fig. 91.) was executed by Mr. Jones. It consisted of a wooden vessel, blackened within, which was to be filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass, for receiving the image, was supported by a bracket, which was moved by a pinion, in connexion with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. Wilkinson, whose residence was near a slaughter house: but we could obtain, by this method, no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another gentleman, a member of this Society, was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

X. In man, and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that, sometimes at least, in the human crystalline, there are ten on each side, (Plate 12. Fig. 93.) not three, as I once, perhaps from a too hasty observation, concluded *. Those who find any difficulty, in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes ima-

* De quibusdam Oculi Partibus, L. B. 1746. Ap. Hall. Disp. Anat. IV. 301.

† De Oculo Humano, L. B. 1742. Ap. Hall. Disp. Anat. VII. ii. 108, 100.

‡ 1794. 352, 354.

* De Corp. Hum. Vir. Cons. 68.

gined, that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two, three, or more branches, at the spot where the ciliary zone begins, and seem indeed to furnish the choroid with some fine filaments at the same place. The branches often reunite, with a slight protuberance, that scarcely deserves the name of a ganglion: here they are tied down, and mixed with the hard whitish brown membrane, that covers the compact spongy substance, in which the vessels of the ciliary processes anastomose and subdivide. (Plate 12. Fig. 94.) The quantity of the nerves, which proceeds to the iris, appears to be considerably smaller than that which arrives at the place of division; hence there can be little doubt, that the division is calculated to supply the lens with some minute branches; and it is not improbable, from the appearance of the parts, that some fibres may pass to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be supplied from without. But the subdivisions, which probably pass to the lens, enter immediately into a mixture of ligamentous substance, and of a tough brownish membrane; and I have not hitherto been able to develope them. Perhaps animals may be found, in which this substance is of a different nature; and I do not despair that, with the assistance of injections, for more readily distinguishing the blood vessels, and of an acid for whitening the nerves, it may still be possible to trace them in quadrupeds. Our inability to discover them is scarcely an argument against their existence: they must naturally be delicate and transparent; and we have an instance, in the cornea, of considerable sensibility, where no nerve has yet been traced. The capsule adheres to the ciliary substance, and the lens to the capsule, principally in two or three points; but, I confess, I have not been able to observe that these points are exactly opposite to the trunks of nerves; so that, probably, the adhesion is chiefly caused by those vessels which are sometimes seen passing to the capsule in injected eyes. We may, however, discover ramifications from some of these points, upon and within the substance of the lens, (Plate 12. Fig. 95.) generally following a direction near to that of the fibres, and sometimes proceeding from a point opposite to one of the radiating lines of the same surface. But the principal vessels of the lens appear to be derived from the central artery, by two or three branches at some little distance from the posterior vertex; which I conceive to be the cause of the frequent adhesion of a portion of a cataract to the capsule, about this point: they follow nearly the course of the radiations, and then of the fibres; but there is often a superficial subdivision of one of the radii, at the spot where one of them enters. The vessels coming from the choroid appear principally to supply a substance, hitherto unobserved, which fills up the marginal part of the capsule of the crystalline, in the form of a thin zone, and makes a slight elevation, visible even through the capsule. (Fig. 96.. 98.) It consists of courser fibres than the lens, but in a direction nearly similar; they are often intermixed with small globules. In some animals, the margin of the zone is crenated, especially behind, where it is shorter: this is observable in the partridge; and, in the same bird, the whole surface of the lens is seen to be covered with points, or rather globules, arranged in regular lines, (Plate 13. Fig. 99.) so as to have somewhat the appearance of a honeycomb, but towards the vertex less uniformly disposed. This regularity is a sufficient proof that there could be no optical deception in the appearance; although it requires a good microscope to discover it distinctly; but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain: but it may possibly secrete the liquid of the crystalline; and it as much deserves the name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens; (Plate 12. Fig. 97.) and indeed, it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. Petit should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood vessel. (Plate 13. Fig. 100.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as Leeuwenhoek imagined, across the line of division: yet there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But, since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, as the motion can meet with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule, has elasticity enough to reassume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must cooperate in restoring the lens to its flattest form. If it be inquired, why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds, and even of the human eye *. This remark has indeed been made by Leroi, Albinus, and others, but the circumstance is not generally understood. It is so difficult to obtain a distinct view of these bodies, undisturbed, that I am partly indebted to accident, for having been undeceived respecting them: but, having once made the observation, I have learnt to show it in an unquestionable manner. I remove the posterior hemisphere of the sclerotica, or somewhat more, and also as much as possible of the vitreous humour, introduce the point of a pair of scissors into the capsule, turn out the lens, and cut off the greater part of the posterior portion of the capsule, and of the rest of the vitreous humour. I next dissect the choroid and uvea from the sclerotica; and, dividing the anterior part of the capsule into segments from its centre, I turn them back upon the ciliary zone. The ciliary processes then appear, covered with their pigment, and perfectly distinct both from the capsule and from the uvea; (Plate 13. Fig. 101.) and the surface of the capsule is seen shining, and evidently natural, close to the base of these substances. I do not deny that the separation between the uvea and the processes, extends somewhat further back than the separation between the processes and the capsule; but the difference is inconsiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the processes is wholly irreconcileable with muscularity; and their being considered as muscles attached to the capsule, is therefore doubly inadmissible. Their lateral union with the capsule commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be, cannot easily be determined: if it were necessary to have any peculiar organs for secretion, we might call them glands, for the percolation of the aqueous humour; but there is no reason to think them requisite for this purpose.

The marsupium nigrum of birds, and the horseshoe like appearance of the choroid of fishes, are two substances which have sometimes, with equal injustice, been termed muscular. All the apparent fibres of the marsupium nigrum are, as Haller had very truly asserted, merely duplicatures of a membrane, which, when its ends are cut off, may easily be unfolded under the microscope, with the assistance of a fine hair pencil, so as to leave no longer any suspicion of a muscular texture. The experiment related by Mr. Home *, can scarcely be deemed a very strong argument for attributing to this substance a faculty which its appearance so little authorises us to expect in it. The red substance, in the choroid of fishes, (Plate 13. Fig. 102.) is more capable of deceiving the observer; its colour gives it some little pretension, and I began to examine it with a prepossession in favour of its muscular nature. But, when we recollect the general colour of the muscles of

* Vid. Hall. Physiol. V. 482. et Duverney, ibi citat.

* Phil. Trans. 1796. 18.

fishes, the consideration of its redness will no longer have any weight. Stripped of the membrane which loosely covers its internal surface, (Fig. 103.) it seems to have transverse divisions, somewhat resembling those of muscles, and to terminate in a manner somewhat similar; (Fig. 104.) but, when viewed in a microscope, the transverse divisions appear to be cracks, and the whole mass is evidently of a uniform texture, without the least fibrous appearance: and, if a particle of any kind of muscle is compared with it, the contrast becomes very striking. Besides it is fixed down, throughout its extent, to the posterior lamina of the choroid, and has no attachment capable of directing its effect; to say nothing of the difficulty of conceiving what that effect would be. Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity.

The bony scales of the eyes of birds, which were long ago described in the Mémoirs of the Academy, by Mery *, in the Philosophical Transactions, by Mr. Ranby †, and by Mr. Warren ‡, afterwards in two excellent Memoirs of M. Petit on the eye of the turkey and of the owl §, and lately by Professor Blumenbach ||, Mr. Pierce Smith ¶, and Mr. Home **, can, on any supposition, have but little concern in the accommodation of the eye to different distances: they rather seem to be necessary for the protection of that organ, large and prominent as it is, and unsupported by any strength in the orbit, against the various accidents to which the mode of life and rapid motion of those animals must expose it; and they are much less liable to fracture than an entire bony ring of the same thickness would have been. The marsupium nigrum appears to be intended to assist in giving strength to the eye, to prevent any change in the place of the lens, by external force: it is so situated as to intercept but little light, and that little is principally what would have fallen on the insertion of the optic nerve: and it seems to be too firmly tied to the lens, even to admit any considerable elongation of the axis of the eye, although it certainly would not impede a protrusion of the cornea. There is a singular observation of Poupart, respecting the eyes of insects, which requires to be mentioned here. He remarks, that the eye of the libellula is hollow; that it communicates with an air vessel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this cavity: he supposes that the insect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different distances *. There is no difficulty in supposing that the means of producing the change of the refractive powers of the eye, may be, in different classes of animals, as diversified as their habits, and the general conformation of their organs. But an examination of the eyes of libellulae, wasps, and lobsters, induces me not only to reject the suggestion of Poupart, but to agree with those naturalists, who have called in question the pretensions of these organs to the name usually applied to them. Cuvier has given a very fair state-

* II. 13.
† Phil. Trans. XXXIII. 223. Abr. VII. 435.
‡ Phil. Trans. XXXIV. 113. Abr. VII. 437.
§ Mém. de l'Acad. 1735. 163. 1736. 100. Ed. Amst.
|| Comm. Gott. VII. 62.
¶ Phil. Trans. 1795. 263.
** Phil. Trans. 1796. 14.

* Phil. Trans. XXII. 673. Abr. II. 762.

ment of the case, in his valuable work on comparative anatomy; and his descriptions, as well as those of Swammerdam, agree in general with what I have observed. We are prejudiced in favour of their being eyes, by their situation and general appearance. The copious supply of nerves seems to prove, at least, that they must be organs of sense. In the hermit crab, Swammerdam says, that their nerves even decussate, but this is not the case in the crawfish. The external coat is always transparent; its divisions are usually more or less lenticular. Many insects have no other organs at all resembling eyes; and when these eyes have been covered, the insects appear to have been either wholly or partially blinded*. But, on the other hand, many insects are without these eyes, and of those who have them, many have others also, more unquestionably fitted for vision. The neighbouring parts of the hard skin or shell are often equally transparent with these, when the crust lining them is removed. In the apis longicornis, the antennae, as Mr. Kirby first informed me, have somewhat of the same reticulated appearance, but not enough for the foundation of any argument respecting its use. This reticulated coat is always completely lined by an obscure and opaque mucus, which appears perfectly unfit for the transmission of light; nor is there any thing like a transparent humour in the whole structure: and the convexity of the lenticular portions is by no means sufficiently great, to bring the rays of light to a very near focus; indeed, in lobsters, the external surface is perfectly equable, and the internal surface is only divided into squares by a cancellated texture adhering to it. There is nothing in any way analogous to a retina, and there can be no formation of such an image, as is depicted in the eyes of all other animals, not excepting even the vermes: nor does there appear to be room to allow with Bidloo that there is a perforation, admitting light, under the centre of each hexagon. If they are eyes, their manner of perceiving light must rather resemble the sense of hearing than that of seeing, and they must convey but an imperfect idea of the form of objects. And it may be remarked that beetles, which have no other eyes, fly much by night, and are proverbially dull-sighted. The stemmata, which are usually 3, 6, 8, or 12 in number, have much more indisputably the appearance of eyes. In the wasp, they consist externally of a thick double convex lens, firmly fixed in the shell, perfectly transparent, and externally very hard, but internally softer; behind this appears to be a vitreous humour, and probably behind that, there is a retina. Here we must consider the crystalline lens as united to the cornea, without any uvea or aqueous humour. In the reticulated eyes, there is nothing resembling a crystalline lens. The stemmata have never any motion, but they are capable of comprehending, conjointly, a very extensive field of view; and it is possible that the posterior part of the lens may have a power of changing its convexity for the perception of objects at different distances.

XI. I shall now finally recapitulate the principal objects and results of the investigation, which I have taken the liberty of detailing so fully to the Royal Society. First, the determination of the refractive power of a variable medium, (M.E. 465.) and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for

* Hooke Microgr. 178.

ascertaining, upon inspection, the exact focal distance of every eye, and the remedy for its imperfections. Thirdly, to show the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry, on the principles suggested by Dr. Porterfield; and to confirm his opinion of the utter inability of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself, that I shall not be deemed too precipitate, in denominating this series of experiments satisfactorily demonstrative.

EXPLANATION OF THE FIGURES.

PLATE 9. Fig. 71. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 72. The scale of the optometer. The middle line is divided, from the lower end, into inches. The right hand column shows the number of a concave lens requisite for a short sighted eye; by looking through the slider, and observing the number opposite to which the intersection appears when most remote. At the other end, the middle line is graduated for extending the scale of inches, by means of a lens four inches in focus: the negative numbers implying that such rays, as proceed from them, are made to converge towards a point on the other side of the lens. The other column shows the focal length of convex glasses, required by those eyes, to which the intersection appears, when nearest, opposite to the respective places of their numbers.

Fig. 73. A side view of the optometer, half its size.

Fig. 74. The appearance of the lines through the slider.

Fig. 75. Method of measuring the magnitude of an image on the retina.

Fig. 76. Diagonal scale drawn on a looking glass.

Fig. 77. The method of applying a lens with water to the cornea.

Fig. 78. The appearance of a spectrum occasioned by pressure; and the inflection of straight lines seen within the limits of the spectrum.

Fig. 79. An illustration of the enlargement of the image, which would be the consequence of an elongation of the eye: the images of the candles, which, in one instance, fall on the insertion of the nerve, falling, in the other instance, beyond it.

PLATE 10. Fig. 80. The successive forms of the image of a large distant object, as it would be delineated by each refractive surface in the eye; to show how that form at last coincides with the retina. E G is the distance between the foci of horizontal and vertical rays in my eye.

PLATE 11. Fig. 81. Vertical section of my right eye, seen from without; twice the natural size.

Fig. 82. Horizontal section, seen from above.

Fig. 83. Front view of my left eye, when the pupil is contracted; of the natural size.

Fig. 84. The same view when the pupil is dilated.

Fig. 85. Outline of the eye and its straight muscles when at rest.

Fig. 86. Change of figure, which would be the consequence of the action of those muscles upon the eye, and upon the adipose substance behind it.

Fig. 87. Scale of the small optometer.

Fig. 88. Appearance of four images of a line seen by my eye when its focus is shortest.

Fig. 89. Outline of the lens, when relaxed ; from a comparison of M. Petit's measures with the phenomena of my own eye, and on the supposition that it is found in a relaxed state after death.

Fig. 90. Outline of the lens sufficiently changed to produce the shortest focal distance.

Fig. 91. Apparatus for ascertaining the focal length of the lens in water.

Plate 12. Fig. 92. n. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a spherical surface, when received on planes at distances progressively greater.

Fig. 92. n. 29. Image of a minute lucid object held very near to my eye.

Fig. 92. n. 30. The same appearance when the eye has been rubbed.

Fig. 92. n. 31.. 37. Different forms of the image of a lucid point at greater and greater distances ; the most perfect focus being like n. 33, but much smaller.

Fig. 92. n. 38. Image of a very remote point seen by my right eye.

Fig. 92. n. 39. Image of a remote point seen by my left eye ; being more obtuse at one end, probably from a less obliquity of the posterior surface of the crystalline lens.

Fig. 92. n. 40. Combination of two figures similar to the fifth variety of n. 28; to imitate n. 38.

Fig. 92. n. 41. Appearance of a distant lucid point, when the eye is adapted to a very near object.

Fig. 92. n. 42, 44. Shadow of parallel wires in the image of a distant point, when the eye is relaxed.

Fig. 92. n. 43, 45. The same shadows rendered curved by a change in the figure of the crystalline lens.

Fig. 93. The order of the fibres of the human crystalline.

Fig. 94. The division of the nerves at the ciliary zone ; the sclerotica being removed. One of the nerves of the uvea is seen passing forwards and subdividing. From the calf.

Fig. 95. Ramifications from the margin of the crystalline lens.

Fig. 96. The zone of the crystalline faintly seen through the capsule.

Fig. 97. The zone raised from its situation, with the ramifications passing through it into the lens.

Fig. 98. The zone of the crystalline detached.

Plate 13. Fig. 99. The crenated zone, and the globules regularly arranged on the crystalline of the partridge.

Fig. 100. The order of the fibres in the lens of birds and fishes.

Fig. 101. The segments of the capsule of the crystalline turned back, to show the detached ciliary processes. From the calf.

Fig. 102. Part of the choroid of the cod fish, with its red substance. The central artery hangs loose from the insertion of the nerve.

Fig. 103. The membrane covering this substance internally, raised by the blowpipe.

Fig. 104. The appearance of the red substance, after the removal of the membrane.

PLATE 9.

Fig. 72.

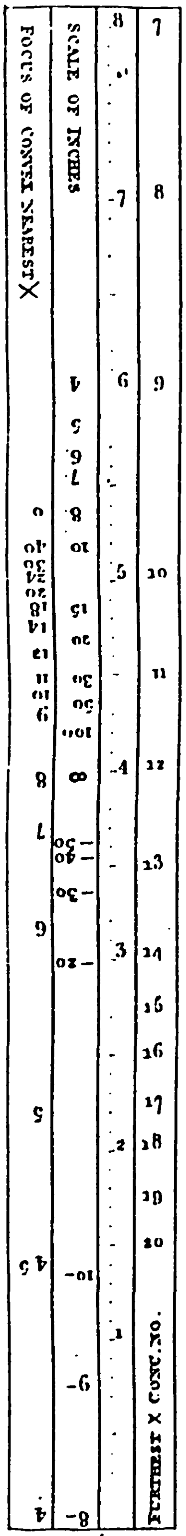


Fig. 71.

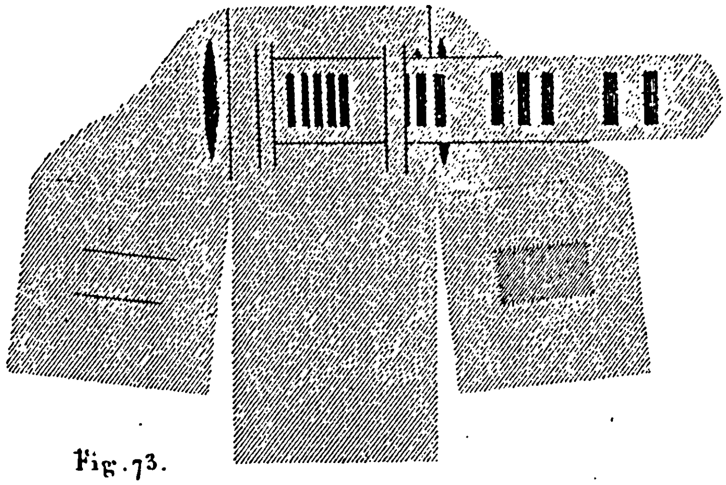


Fig. 75.

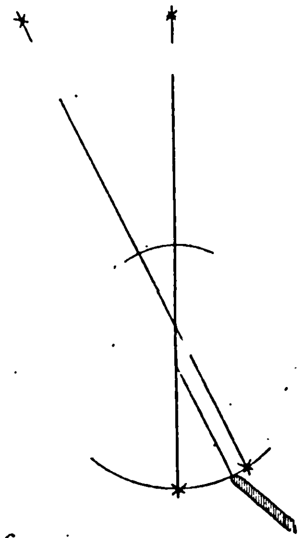


Fig. 73.

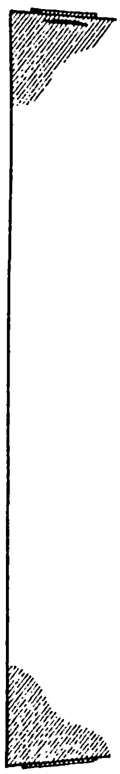


Fig. 74.

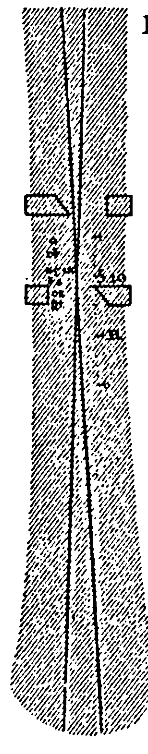


Fig. 76.

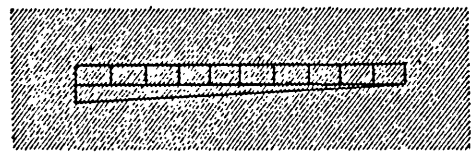


Fig. 77.

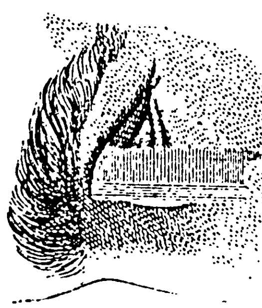


Fig. 78.

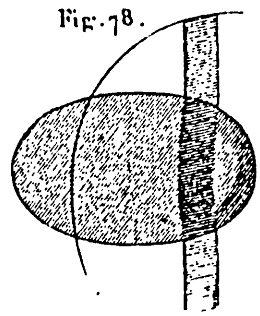
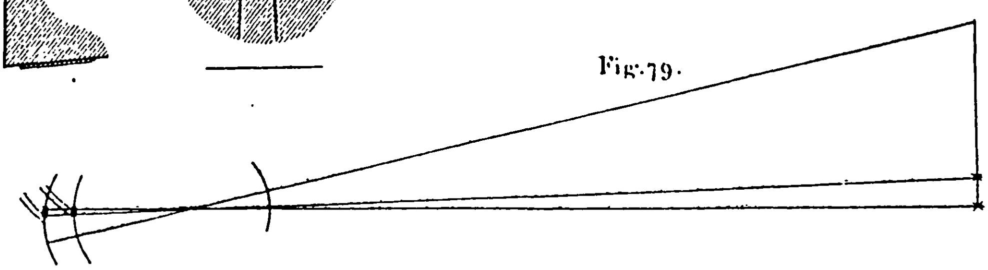


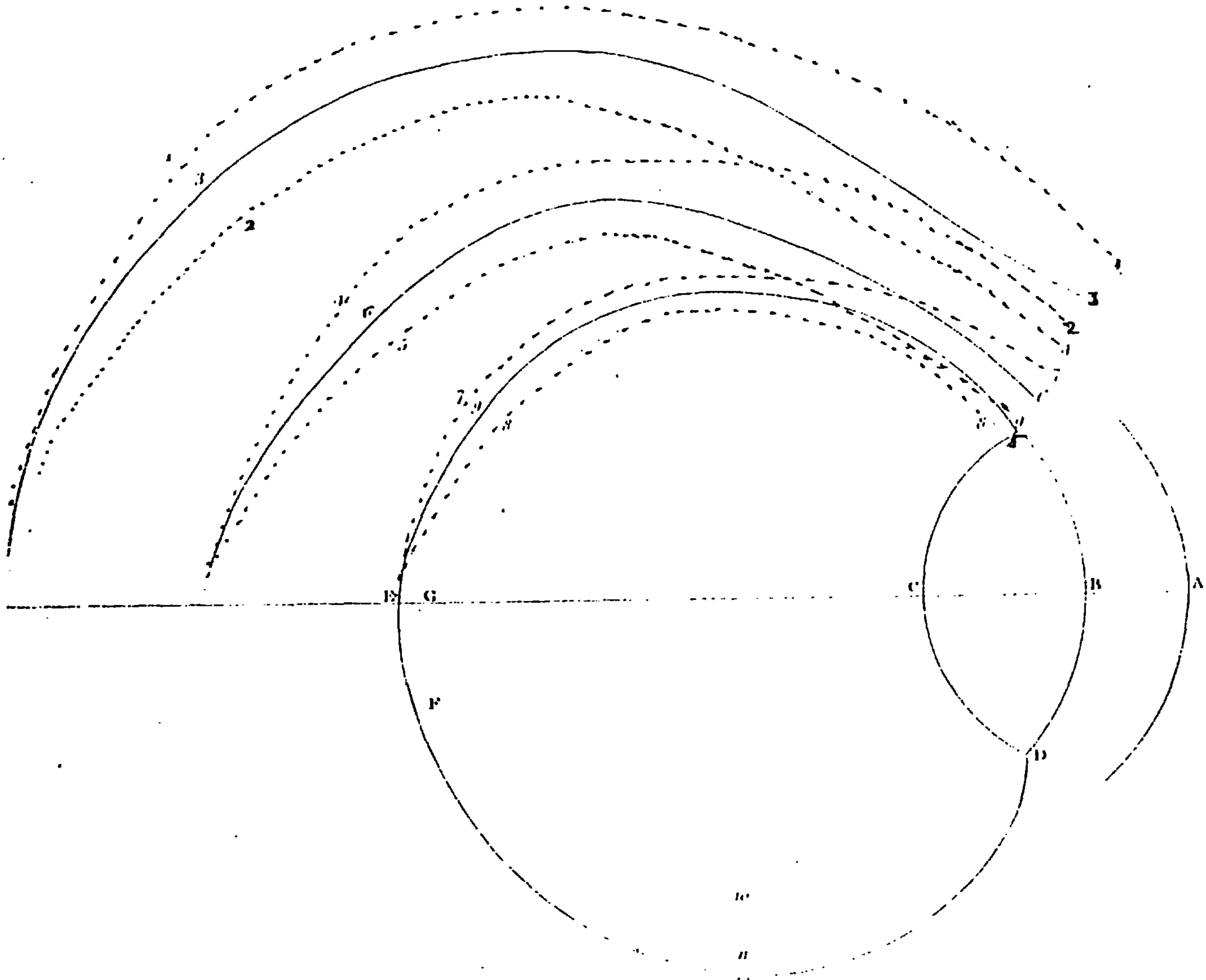
Fig. 79.



Pub. by J. Johnson, London 1 July 1806.

Joseph Skelton

Fig 80.



Pub. by J. Johnson London 1 July 1800

PLATE II.

Fig. 81.

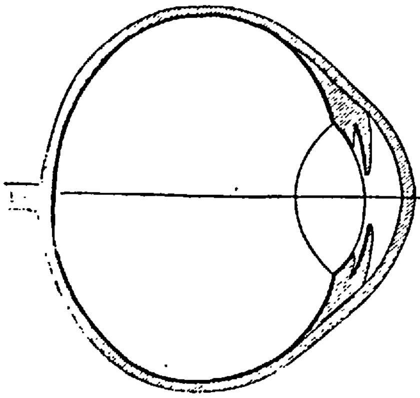
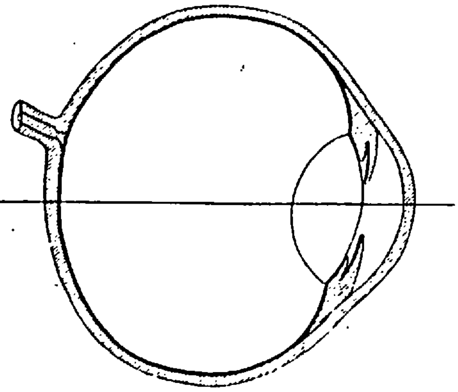


Fig. 82.



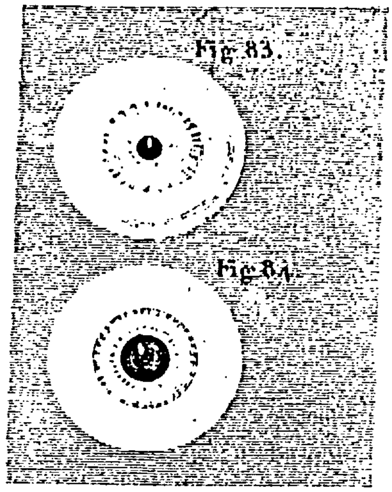


Fig. 83.

Fig. 84.

Fig. 85.

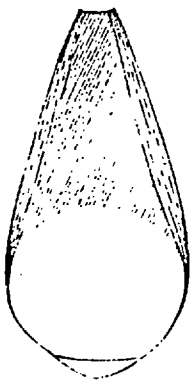


Fig. 86.

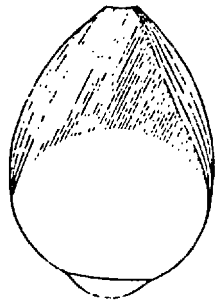


Fig. 89.

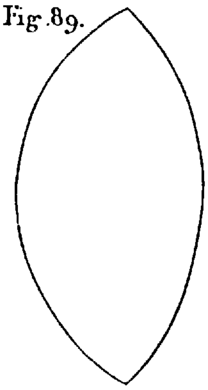


Fig. 87.

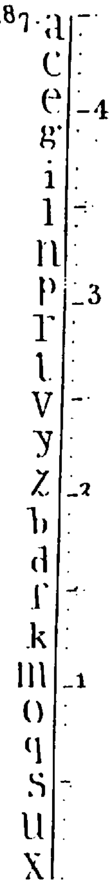


Fig. 88.

Fig. 91.

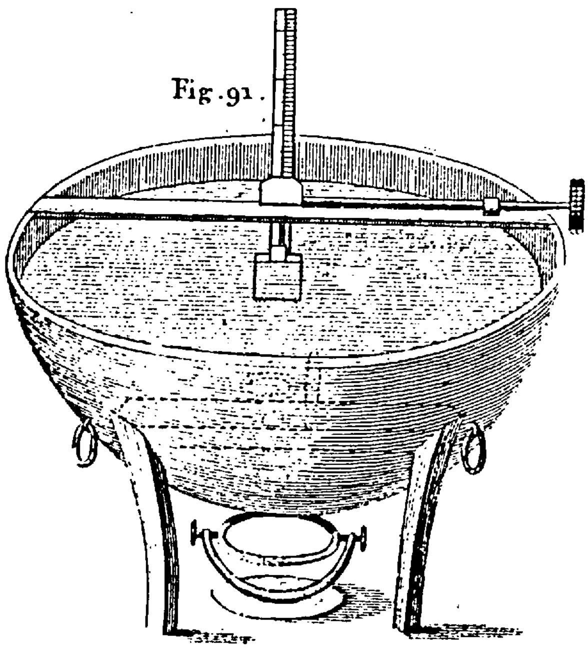
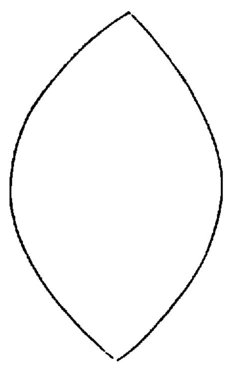


Fig. 90.



Pub. by J. Johnson, London 1 July 1801.

Joseph Skelton sculp

PLATE 12.

Fig. 92.

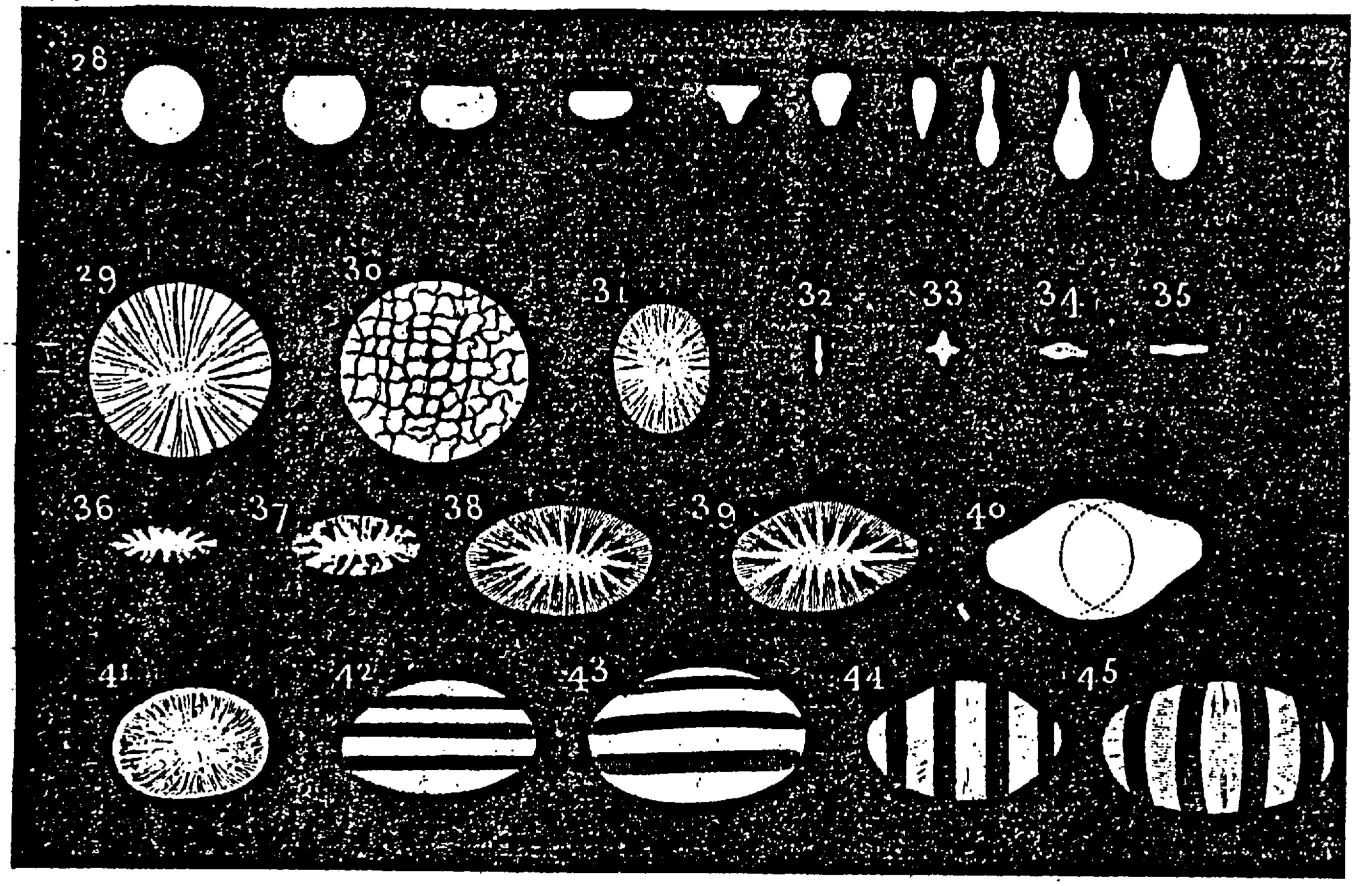


Fig. 93.

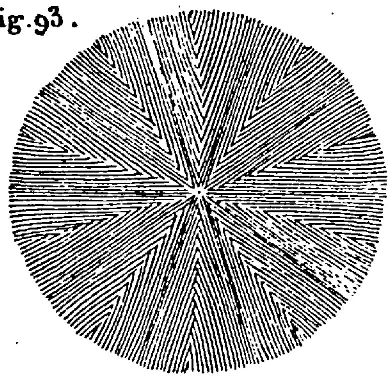


Fig. 94.

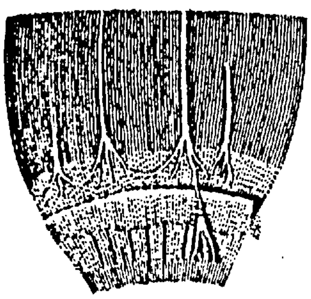


Fig. 95.

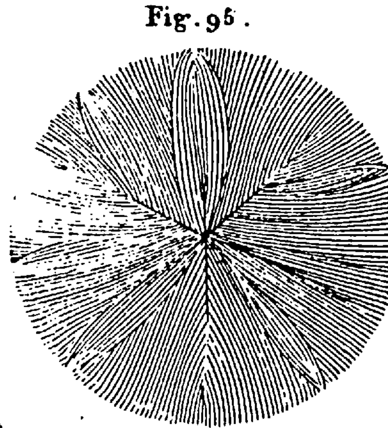


Fig. 97.

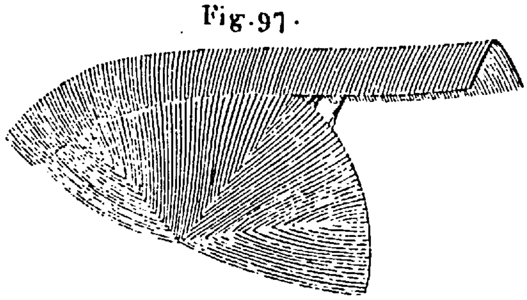


Fig. 96.

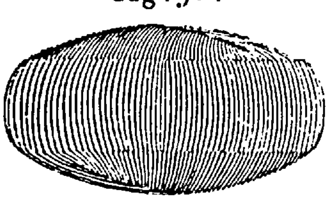
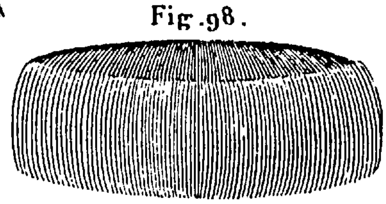


Fig. 98.



Pub. by J. Johnson London 2 July 1806.

Joseph Skelton sc.

PLATE 13.

Fig. 99.

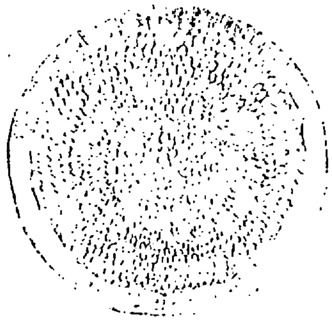


Fig. 100.

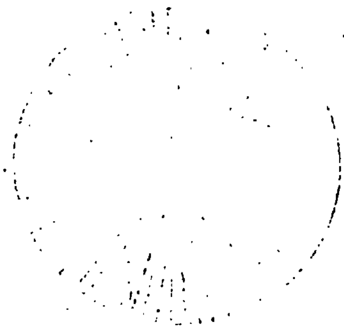


Fig. 101.

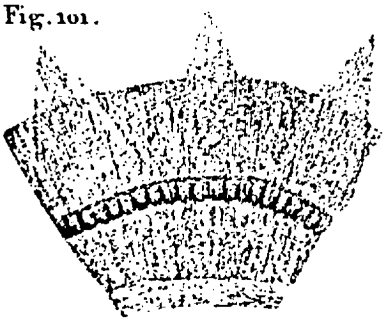


Fig. 102.

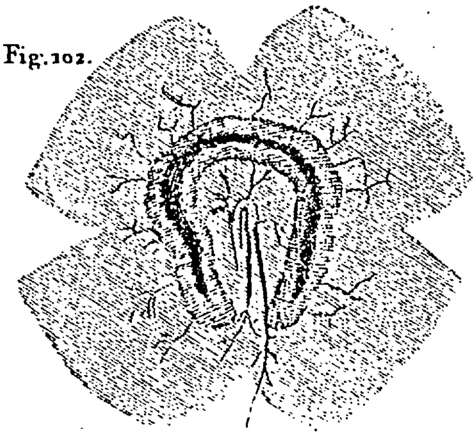


Fig. 103.

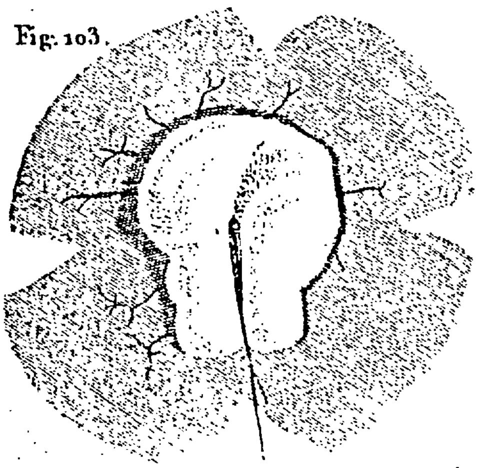
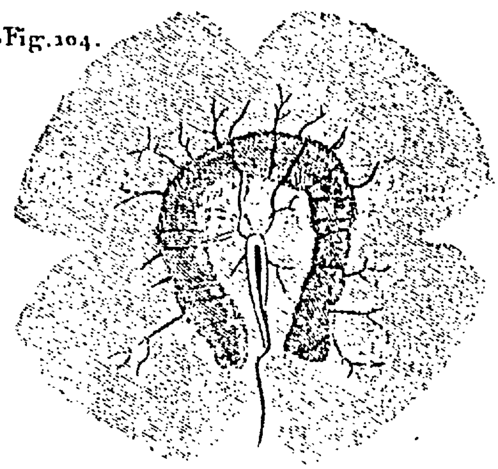


Fig. 104.



Pub. by J. Johnson London 1 July 1800.

Joseph Mitan sculp.

Fig. 109

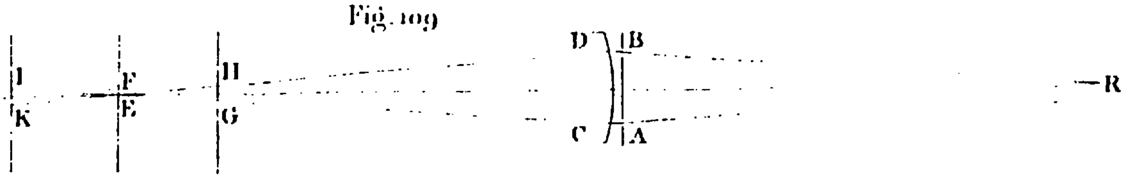


Fig. 110

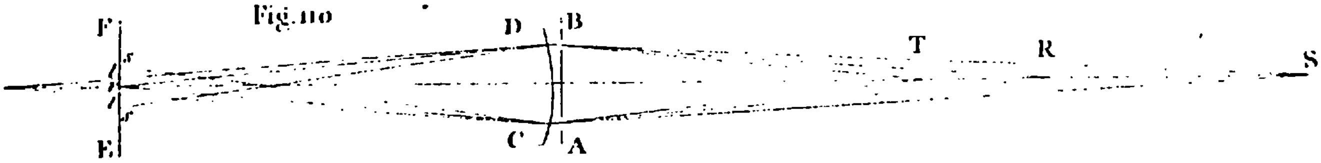


Fig. 111

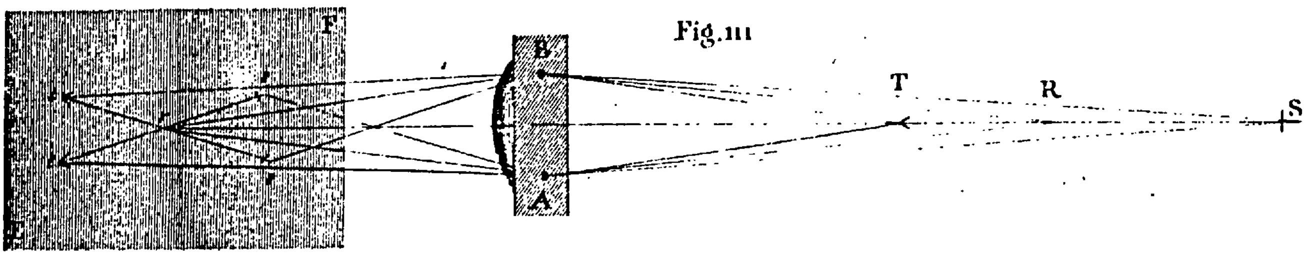


Fig. 112

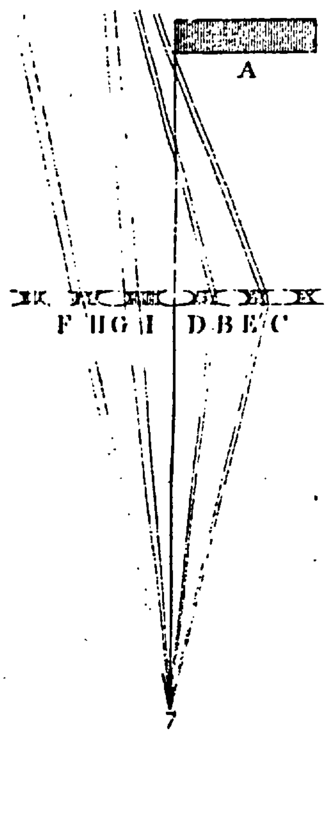


Fig. 113

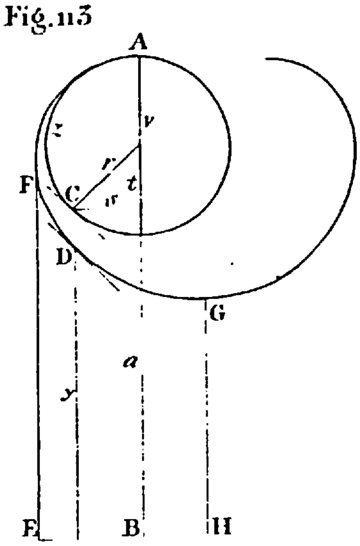
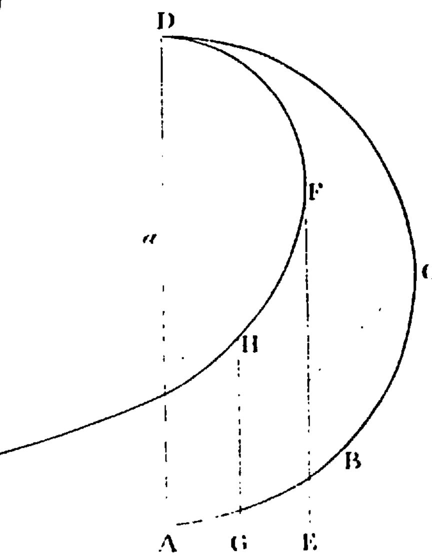


Fig. 114



COLOUR VISION

Knowledge about the mechanism of colour vision advanced during the seventeenth and eighteenth centuries mainly owing to the work of three men. In addition to his brilliant work on the nature of white light, Newton carried his theories forward to offer an explanation of the way in which the various colours produced their effects upon the retina. After Newton, the subject was largely neglected until Dalton published the unique self-analysis of his own colour-blindness in 1794. This was followed in 1802 by Thomas Young's brief paper, in which he outlined his own theory of colour vision, a theory which has received experimental verification only in recent years. The neglect of this topic is not surprising, since, until the time of Dalton's discovery, the basic problems of vision and the basic problems of the nature of colour were proving difficult to resolve separately, without the additional complication of combining the problems.

The major problem in vision had been solved theoretically nearly two hundred years before Dalton's work, when Kepler had mathematically established the retina as the seat of vision. His theory had rapidly been confirmed experimentally by Scheiner and Descartes. However, the concept of the retina as the seat of vision rather than the crystalline lens, which until the end of the sixteenth century had been considered to be the light-sensitive medium, was one that required considerable thought and consolidation before it was fully accepted. This acceptance occurred gradually over a period of two hundred years, as alternative theories for the seat of vision were explored and rejected. During roughly the same period, the nature of colour and the related topic, the nature of light (whether it was corpuscular or a wave motion), were also being investigated. Newton's discovery of the nature of white light can probably be considered to have the same significance in this area as did Kepler's establishment of the retina as the seat of vision. Probably the major problem which had to be resolved before a satisfactory foundation could be established for Young's three-colour theory of vision, was the explanation of the different effects obtained when pigments and lights of the same colours were mixed, e.g. red, green and blue pigments giving black, while red, green and blue lights give white.

During the seventeenth and eighteenth centuries, three scientists dominated this field. Chronologically, they were Newton, Dalton and Young, but in fact the work of Newton was strongly linked to that of Young, so that it will be convenient to consider their work together. There were also other articles published from time to time during this period and they will be considered chronologically. These articles can be considered to give a convenient introduction to Dalton's work, since they deal largely with accounts of defective sight, although the quality of their descriptions do not match those of Dalton. Newton and Young adopted a theoretical approach in the main, while Dalton's approach to colour-vision was, inevitably, a subjective description of his own colour-blindness. It was not until Young's work was published that the two separate approaches to the subject began to merge.

Robert Boyle in 1664 published the first article on colour-blindness that I have been able to discover. He made the interesting observation that we see colours because of some motion produced in the retina by the colours. This motion is then transmitted to the brain. He said that if some other cause - say, a dream - caused the same motion, then we would see the same colours.[1]

"Colour is so far from being an inherent quality of the object in the sense that is wont to be

> declared by the schools, or even in the sense of some Modern Atomists, that if we consider the matter more attentively, we shall see cause to suspect, if not conclude, that though Light do no more immediately affect the organ of sight than do the bodies that send it thither, yet Light itself produces the sensation of a colour, but as it produces such a determinant kind of local motion in some part of the brain; which, though it happen most commonly from the motion whereinto the slender strings of the Retina are put, by the appulse of light, yet if the like motion happen to be produced by any other cause wherein the light concurs not at all, a man shall think he sees the same colour."

A few years later, Boyle again returned to this topic in an article in which he mentioned two people who could not differentiate some colours[2]. The article is very superficial, however, and does no more than mention the inability to differentiate certain colours. The first subject mentioned was a woman whose sight had gradually returned after she had gone blind. It was said that she could see more distinctly in dull light than in bright, and could not differentiate between some colours. No other details were given, but it would appear that this was not a case of normal colour-blindness, since first, it followed some major defect of the eye leading to blindness; and second, it affected a woman, and women are subject to this defect of vision far less frequently than men. The second case could well be a case of colour-blindness although, regrettably, Boyle gave few details. The case concerned a mathematician who saw some colours differently from other people. It is indeed surprising that further details were not given, and from Boyle's account we are not even given the colours which were mistaken.

The first experiment on colour vision which I have been able to find was carried out by De la Hire[3]. He wanted to find out whether our two eyes see an object as the same colour. The experiment was very simple and, not surprisingly, De la Hire used an adaptation of the experiment using pin holes in two sheets of card. A coloured paper was viewed by both eyes, each looking through one of the holes. Two circular images were seen, and by moving the cards the two images were arranged to be adjacent to each other. If the eyes were seeing a different colour, then the images seen in the two holes would be easily seen to be of different colours. Although this experiment was extremely simple, it nevertheless marks one of the few occasions during this period when experiments in colour vision were carried out.

The first time that the actual symptoms of colour-blindness were described with adequate detail was nearly half a century later[4]. Joseph Huddart was a hydrographer and manufacturer living in Cumberland, and he reported a case which had come to his notice of a man who apparently saw colours differently from other people. The subject of Huddart's paper was a man called Harris who lived at Maryport. It would appear that Harris was very observant, since it was reported that he first noticed that his sight was different at the age of four. He found a stocking and noted that people called it 'red'; he could not understand this and could only call it a stocking. Huddart spoke with Harris frequently and learned that he could see well, but could not distinguish colours. It is extremely unlikely that Harris could see no colours at all, and he was probably a dichromat, or anomalous trichromat. A dichromat is a person who cannot distinguish one of the three prime colours; red, green and blue; the most common deficiency is red-blindness. Anomalous trichromats can distinguish three prime colours, but they see them differently from normal people. A fuller account of the current state of knowledge of colour-defectiveness is given in Appendix A There are other clues to be found from Huddart's paper, however, which indicate that Harris was probably a protanope - someone who is unable to see red colours. Harris specifically mentioned that he was unable to distinguish cherries on a tree from the leaves, except by shape. Straw-coloured articles he called white, which would indicate a deficiency at the red end of the spectrum. These two pieces of evidence taken together indicate that he was probably red-blind, a protanope, the most common type of dichromat. Apart from his difficulty in distinguishing between colours, he felt that he could see as clearly as other people. Most of the tests of colour-matching that were given as examples used ribbons, although there is a mention of one of Harris' brothers, who had seen a rainbow but had been unable to differentiate the colours. Details of the vision of Harris' immediate family were also given, so that from the beginning it was possible to see that an inherited characteristic might be responsible for colour-blindness. Two of Harris' brothers had the same defect of vision, two other brothers and sisters had normal vision, and both parents had normal vision.

The next account of a case of colour-blindness followed very shortly indeed, and was contained in a letter from a J. Scott to a Rev. Whisson of Trinity College, Cambridge[5]. Scott stated initially that this was a family failing. He himself had a disability concerning colours, his father had the same impediment; his mother and a sister were perfect; his other sister had the same defect as himself. This last sister had two sons, both defective, and a daughter who was perfect. Scott himself had a son and daughter, both perfect, as was their mother. The brother of Scott's mother had a similar impediment to Scott. This article must have helped to confirm that colour-blindess was inherited, but unfortunately it contained two pieces of evidence which would have been extremely misleading to anyone attempting to establish the basic rules governing the transmission of the disability. Cases of women suffering from colour-blindness are . much rarer than those of men, so that Scott's sister who also had the defect would have tended to obscure this fact. Also, colour-blindness is almost always inherited through the female line, and it is therefore difficult now to evaluate exactly what part Scott's father's defect played in passing on the colour-deficiency to his children. The explanation which is given below is probably the correct one, although it is impossible to be certain that the transmission pattern might not have been due to some very rare type of dominant colour-blindness.

It can be considered. most unfortunate indeed that this second reliably-reported case of colour-blindness contained complications which were due to a very rare occurrence. This is that Scott and his brothers and sisters were the offspring of a colour-blind father and a mother who was a transmitter of the defect. This must have given the Scott children a very rare genetic inheritance, and yet, owing to the few reported cases of colour-blindness, they must have been very influential in formulating the early tentative theories about how the defect was transmitted. Indeed it would have been most logical to assume that both Scott and his sister had inherited the deficiency from their father; it was probably also assumed that colour-blindness among women was more common than it is.

The explanation for the pattern of the colour-deficiency of the Scott children is probably as follows:

> There are two types of chromosomes - X and Y
> A man has the following pattern: XY.
> A woman has XX.
> A chromosome which produces defective colour vision is deficient in a particular gene and can be referred to as X'; the Y chromosomes are not responsible for colour deficiencies.
> Therefore, if a man inherits an X' chromosome, he has the chromosome pattern X'Y and is colour deficient. If a woman inherits an X'

chromosome, she has the following pattern, $X'X$ and is not deficient. She would be colour-defective only in the far less likely event of her inheriting two X' chromosomes and having the following pattern - $X'X'$.
The chromosome inheritance pattern of the Scott children was probably as follows:

Scott's father	Scott's mother
$X'Y$	$X'X$

Scott $X'Y$ (X' from mother)
Defective sister $X'X'$ (X' from mother and X' from father)
Normal sister $X'X$ (X' from father X from mother)

By a fortunate chance the next person in England to write on the subject of colour vision was the brilliant young scientist, John Dalton. His discovery of his own colour-blindness, which he reported in 1794 to the Literary and Philosophical Society of Manchester[6], brought considerable interest to the subject and ironically led to the name 'Daltonism' being given to the deficiency of colour-blindness. Dalton first discovered the peculiarity of his vision at the age of twenty six, which emphasises the remarkable nature of Harris' observation, who, as we have already seen, first noticed his colour-blindness at the age of four.

Dalton was a Quaker, and it was his mother's reaction to his purchase of some bright red silk stockings for her, which he thought were of a sober drab colour, together with his observations of the change in colour of a pink geranium, that led him to be certain that there was some peculiarity in his sight. Dalton's account of his vision was a masterly description of a defect which had been described previously only in the briefest way. There is, however, a more light-hearted side to Dalton's colour-blindness. This can be seen in the accounts of his contemporaries on the effect of his colour-blindness on his manner of dress. They took great delight in recounting occasions when Dalton was dressed in the brightest colours, while thinking that he was soberly arrayed, as befitted a Quaker. Some examples of these articles are appended to the end of this section on colour vision. Dalton's main contribution to this subject was, however, his description of his own colour-blindness as told to the Literary and Philosophical Society of Manchester, which contained his observations of the apparent change in colour of a pink geranium[7]:

> "The flower was pink, but it appeared to me an almost exact sky-blue by day; in candle-light, however, it was astonishingly changed, not having then any blue in it, but being what I called red, a colour which forms a striking contrast to blue. Not then doubting but that the change in colour would be equal to all, I requested my friends to observe the phenomenon; when I was surprised to find that they all agreed, that the colour was not materially different from what it was by daylight, except my brother, who saw it in the same light as myself. This observation clearly proved that my vision was not like that of other persons; and, at the same time, that the difference between day-light and candle-light, on some colours, was indefinitely more perceptible to me than to others."

Dalton then proceeded to give a detailed description of his vision, which was undoubtedly of considerable help to those studying the phenomenon of Daltonism at the time.

The details of his vision given by Dalton are fascinating, since they represent an attempt to describe what he saw, using names for colours of which in many cases he could not possibly have been aware. This in turn leads to many difficulties when a normally-sighted person attempts to interpret what Dalton was trying to say. He described the solar spectrum as yellow (which covered red, orange, yellow and green, as seen by others), blue and purple (which coincided with colours seen by others). Red, orange, yellow and green simply appeared to him to be shades of yellow. One point of considerable interest which was made by Dalton was that, under certain conditions, colour-blind persons see some objects as being very different in colour, when to others they are hardly distinguishable. It is normal to think of colour-blindness resulting in diminished colour-sensitivity, and it is important to note that under certain circumstances the colour-blind can be far more sensitive to changes of shade than can the normally-sighted. Dalton had previously been aware of some confusion in naming colours, but it was only after experiencing the dramatic changes of colour in the pink geranium that he became convinced of his peculiarity of vision.

Perhaps it would be best first of all to explain how Dalton's unusual experience with the changing colour of the geranium arose, and then to deal with the other phenomena of his colour vision which he listed in his paper. A geranium looks pink because it possesses two main reflection bands, one at the blue end of the spectrum and the other at the red end. Thus, in daylight it reflects a minxture of red and blue light which combine to produce a pink colour. Candlelight contains far more red in its spectrum than daylight; therefore to the normally sighted the flower will appear redder in candlelight than in daylight; but it will still reflect some blue light, and will therefore still appear to have a pink hue. The fact that the flower appeared to be sky-blue by day indicates that Dalton could not have been very sensitive to the red light reflected by the geranium, and that he could distinguish the reflection from only the blue end of the spectrum. Therefore he was what would now be called a protanope (a red-blind person), and the reflected blue light was seen by him as the dominant component. In candlelight with its deficiency of blue light and excess of red, less blue was reflected and this colour was no longer the dominant component. Therefore Dalton saw the geranium as 'red'. It was not of course red, but only what he had come to call that colour. What had happened was that the protanope's red-deficiency made him far more sensitive to the change in the proportion of red light in the reflected light, and he saw a quite dramatic change in the apparent colour of the geranium when viewed in daylight and when viewed in candlelight.

It is now possible to return to Dalton's account of his colour vision and to attempt an explanation of what he saw in terms of the vision of a protanope. He mostly used ribbons for the artificial colours.
Red, by daylight. All crimsons appeared mainly as dark blue, although some have a tinge of dark brown. Crimson wool appeared much the same as dark blue wool. Pink appeared light blue, but with just a little red - in the proportion of 9:1, the red simply made the light blue appear a little faded.
Red, by candlelight. It appeared more vivid than by day. Crimson lost its blue and became yellow-red. Pink lost its blue and appeared red-yellow (1:3).
By daylight blood appeared red: "not unlike the colour called bottle green."

The last sentence above, apart from being a delightful summary of the difficulties of a protanope when trying to communicate with the fully-sighted, must really give cause to consider what it was that Dalton saw, which he thought to call red. Red is such a vivid colour, and yet to the protanope it can differ only very slightly from such colours as bottle-green, perhaps a slight dulling of the green colour with dark brown. It is perhaps puzzling that Dalton should call crimson dark blue, and yet blood appeared to him to be similar to dark green. Once again the explanation is to be found probably in the greater sensitivity of the protanope to some changes in shades of colour. The crimson ribbons obviously contained a greater proportion of blue in their colour composition than blood, and this is borne out by the fact that crimson became yellow-red by candlelight which contains far less blue in its spectrum. Yet there was some inconsistency in this area, since Dalton listed three colours only in his visual spectrum; yellow, blue and purple, where the yellow encompassed all shades of red, orange, yellow and green. It is to be expected therefore that he would have described at least some of the examples of red as appearing yellow rather than blue. The reason is, of course, that he used the solar spectrum to define the colours he could see, while he was using examples of coloured materials to describe his reaction to everyday colours. The coloured dyes used in the materials obviously contained bands of colour in their spectra which made them appear very different to Dalton from the colours he saw in the solar spectrum. The use of ratios as an attempt to explain colour, e.g. pink being blue-red 9:1, was an ingenious attempt to establish a common language with the normally sighted, although with the fundamental difficulties of his sight the device was bound to have limited use only.

Orange and yellow: Dalton saw these similarly to the normally sighted, both by daylight and candlelight.

Green: By daylight green appeared little different from red. The front of a laurel leaf he considered a good match to a stick of sealing wax. Green and orange appeared very similar. A sample of green woollen cloth appeared as a dull brown-red (mud:red 2:1).

By candlelight blue and green appeared similar, as they were to most people.

Blue: Dalton saw blue normally under all conditions.

Purple: This seemed to be only a slight modification of blue both by daylight and candlelight.

Dalton gave details of the appearance of objects in moonlight and by lightning and by what he called electric light, i.e. the light of an electric spark. He said that colours by moonlight appeared the same as by candlelight. Common experience would now lead us to say that objects seen by moonlight had little, if any, colour. Lightning and the electric spark were said to give the same colour-rendering as daylight. Once again, common experience would probably lead us to say that under these conditions objects possessed little or no colour.

Dalton also attempted to contact other people suffering from colour-blindness so that as wide a range as possible of symptomscould be recorded. He contacted Harris' brother (see reference 4) who had similar vision to Harris himself, and obtained his observations on certain colour samples. This convinced Dalton that their vision was similar. The fact that four out of the six brothers in the Harris family suffered a similar defect in sight, and that Dalton's own brother also had the defect, led Dalton to believe that there was an element of heredity in colour-blindness. He also found cases of colour-blindness among his students, but found only one case where the parents were also affected. He had been unable to find any female with the defect. In all he had heard of nearly twenty people with a defect of colour vision, although it is not clear to what extent he had had the opportunity to investigate their colour deficiencies personally. It would appear that Dalton was mainly concerned to establish what deficiencies they had in common, and this perhaps closed his mind to the possibility that there might have been more than one type of colour deficiency.

Within a sample of nearly twenty it is almost certain that there would be someone suffering from another type of colour-blindness. Dalton did not notice any such difference if it existed, although, to be fair to him, this may have been due to the fact that he was not able to investigate the entire sample personally. In fact, he was reasonably satisfied that they had very similar types of colour defect:[8]

"From a great variety of observations made with many of the abovementioned persons, it does not appear to me that we differ more from one another than persons in general do. We certainly agree in the principal facts which characterize our vision, and which I have attempted to point out below. It is but justice to observe here, that several of the resemblances and comparisons mentioned in the preceding parts of this paper were first suggested to me by one or other of the parties, and found to accord with my own ideas.

CHARACTERISTIC FACTS OF OUR VISION.

1. In the solar spectrum three colours appear, yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

2. Pink appears, by day-light, to be sky-blue a little faded; by candle-light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. Crimson appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue.

4. Red and Scarlet have a more vivid and flaming appearance by candle-light than by day-light.

5. There is not much difference in colour between a stick of red sealing wax and grass, by day.

6. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

7. The colour of a florid complexion is dusky blue.

8. Coats, gowns, etc. appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues, browns with reds; and drabs with greens.

9. In all points where we differ from other persons, the difference is much less by candle-light than by day-light."

Having carried out experiments where he viewed objects through light-blue transparent liquids, Dalton was led to conclude that the changes in colour that he saw were due to the fact that one of the humours of his eye was coloured, probably some modification of blue. He supposed that it must be the vitreous humour that was coloured; otherwise the effect would have been visible from the front of his eye. Dalton was wrong, as post-mortem examination of his eyes showed, and in any case his theory would not explain all the peculiarities of his vison. Neverthless, it was an attempt to explain his colour deficiency, an attempt which was based upon experimental observation. Later, Young's explanation of colour-blindness will be dealt with, and it will be seen that, although his theory was almost completely hypothetical, it was in fact the correct one. Perhaps Dalton was too concerned with finding an explanation for his colour deficiency, which would satisfactorily explain the peculiar change he saw in the pink geranium. His theory of the blue humour in his eye would satisfactorily explain the changes of colour that he had reported, but the blue humour would have had no effect upon the red, orange, yellow and green colours in the spectrum (which he saw as one colour) other than to reduce their intensity somewhat. However, Dalton was inconsistent in that he sometimes used spectral colours to describe his vision, but more often used coloured materials and ribbons. His explanation of his vision, which he based upon his coloured-humour theory, was in fact incorrect, but is reasonable and deserves detailed consideration. He was considering coloured materials as a basis of his explanation[9]:

> "It appears therefore almost beyond a doubt, that one of the humours of my eye, and of the eyes of my fellows, is a coloured medium, probably some modification of blue. I suppose it must be the vitreous humour; otherwise I apprehend it might be discovered by inspection, which has not been done. It is the province of

physiologists to explain in what manner the humours of the eye may be coloured, and to them I shall leave it; and proceed to shew that the hypothesis will explain the facts stated in the conclusion of the second part.

1. This needs no further illustration.

2. Pink is known to be a mixture of red and blue; that is, these two colours are reflected in excess. Our eyes only transmit the blue excess, which causes it to appear blue; a few red rays pervading the eye may serve to give the colour that faded appearance. In candle-light, red and orange, or some other of the higher colours, are known to abound more proportionably than in day-light. The orange light reflected may therefore exceed the blue, and the compound colour consist of red and orange. Now, the red being most copiously reflected, the colour will be recognized by a common eye under this small modification; but the red not appearing to us, we see chiefly the orange excess; it is consequently to us not a modification but a new colour.

3. By a similar method of reasoning, crimson, being compounded of red and dark blue, must assume the appearances I have described.

4. Bodies that are red and scarlet probably reflect orange and yellow in greatest plenty, next after red. The orange and yellow, mixed with a few red rays, will give us our idea of red, which is heightened by candle-light, because the orange is then more abundant.

5. Grass-green is probably compounded of green, yellow, and orange, with more or less blue. Our idea of it will then be obtained principally from the yellow and orange mixed with a few green rays. It appears, therefore, that red and green to us will be nearly alike. I do not, however, understand, why the greens should assume a bluish appearance to us and to every body else, by candle-light, when it should seem that candle-light is deficient in blue.

6. The green rays not being perceived by us, the remaining rays may, for aught that is known, compound a muddy red.

> 7. The observations upon the phænomena of pink and crimson will explain this fact.
>
> 8. Suppose a body to reflect red rays as the number 8, orange rays as the number 6, and blue as 5; and another body red 8, orange 6, and blue 6; then it is evident that a common eye, attending principally to the red, would see little difference in those colours; but we, who form our ideas of the colours from the orange and blue, should perceive the latter to be bluer than the former.
>
> 9. From the whole of this paper it is evident that our eyes admit blue rays in greater proportion than those of other people; therefore when any kind of light is less abundant in blue, as is the case with candle-light compared to day-light, our eyes serve in some degree to temper that light, so as to reduce it nearly to the common standard. This seems to be the reason why colours appear to us by candle-light, almost as they do to others by day-light.
>
> I shall conclude this paper by observing, that it appears to me extremely probable, that the sun's light and candle-light, or that which we commonly obtain from combustion, are originally constituted alike; and that the earth's atmosphere is properly a blue fluid, and modifies the sun's light so as to occasion the commonly perceived difference."

In 1807[10] Thomas Young put forward a theory to explain Dalton's colour deficiency, which has recently been shown to be correct (see Appendix A), but which, ironically, was at the time based upon little or no experimental evidence. Young said:

> "He cannot distinguish blue from pink by daylight, but by candle-light pink appears red; in the solar spectrum red is scarcely visible, the rest appears to consist ot three colours, yellow and blue, or yellow, blue, and purple. He thinks it probable that the vitreous humour is of a deep blue tinge: but this has never been observed by anatomists, and it is much more simple to suppose the absence or paralysis of those fibres of the retina, which are calculated to perceive red; this supposition

> explains all the phenomena except that greens appear to become blue when viewed by candle-light; but in this circumstance there is perhaps no great singularity."

Young's explanation of colour-blindness was based upon his three-colour theory of vision[11] which he had presented to the Royal Society on November 12th 1801. Young described his lecture in the most modest, and in view of the importance of his theory, inaccurate terms. He said, "The object of this present dissertation is not so much to propose any opinions which are absolutely new, as to refer some theories, which have already been advanced, to their original inventors, to support them by additional evidence, and to apply to them a great number of diversified facts, which have hitherto been buried in obscurity." It has been suggested that Young, a physician, was concerned that his patients would lose confidence in him if his interest in science became widely known. This would explain why his far-reaching theory is almost hidden in an article which contains lengthy extracts from Newton. In fact Newton's work provided a foundation for Young's hypothesis, but Young's genius lay in his concept that three primary colour receptors in the eye were responsible for our ability to see the entire colour spectrum.

Newton's ideas on colour vision are scattered among several of his publications. In a letter to Oldenburg[12] he expressed himself as follows:

> "And for the same reason I chose to speak of colours according to the information of our senses, as if they were qualities of light without us. Whereas by that hypothesis I must have considered rather as modes of sensation, excited by the mind by various motions, figures, or sizes of the corpuscles of light, making various mechanical impressions on the organ of sense."

> Later, in the same letter he said:[12a]

> "That fundamental supposition is 'That the parts of bodies, when briskly agitated, do excite vibrations in the æther, which are propagated every way from those

bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing.' Now the most free and natural application of this hypothesis, to the solution of phænomena, I take to be this: that the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the æther of various depths or sizes, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal sizes be separated from one another, the largest beget a sensation of red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various sizes, which, according to those sizes, make several tones in sound: that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with the least refraction; whence the vibrations of several sizes, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction; and so cause the phænomena of prisms and other refracting substances; and that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that according to the number of vibrations, interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And since the vibrations which make blue and violet, are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phænomena of those fragments of such plates.

These seem to be the most plain, genuine, and necessary conditions of this hypothesis: and they agree so justly with my theory, that if the animadversor think fit to apply them, he need not on that account apprehend a divorce from it. But yet how he will defend it from other difficulties I know not."

In another letter to Oldenburg, Newton said:[13]

"Thus much of refraction, reflection, transparency, and opacity; and now to explain colours; I suppose, that as bodies of various sizes, densities, or sensations, do by percussion or other action excite sounds of various tones, and consequently vibrations in the air of various bigness; so when the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the æther, those rays, whatever they be, as they happen to differ in magnitude, strength or vigour, excite vibrations of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta through the optic nerves into the sensorum (which light itself cannot do) and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all with white, much after the manner, that in the sense of hearing, nature makes use of aereal vibrations of several bignesses to general sounds of divers tones; for the analogy of nature is to be observed. And further, as the harmony and discord of sounds proceed from the proportions of the æthereal. And possibly colour may be distinguished into its principal degrees, red, orange, yellow, green, blue, indigo, and deep violet, on the same ground, that found within an eighth is graduated into tones."

Again, in his Opticks[14] Newton returned to the idea of different colours exciting vibrations in the retina:

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, - the least refrangible the largest? May not the harmony and discord of colours

> arise from the proportions of the vibrations propagated through the fibres of the optic nerve into the brain, as the harmony and discord of sounds arise from the proportions of the vibrations of the air?"

As has already been stated, Young's genius lay in the abstraction of the three-colour theory from the treasure house of ideas produced by Newton. Newton's concept of the operation of the retina and the optic nerve was almost unbelievably advanced for his time, and therefore, perhaps not surprisingly, was neglected until used by Young. It is interesting to note in the above excerpts from Newton's work, that the balance of emphasis when discussing the nature of light is clearly in favour of some sort of vibration. He speaks of colours as "modes of sensation, excited by the mind by various motions, figures, or sizes of the corpuscles of light, making various mechanical impressions on the organ of sense", but this is the only mention of the possible corpuscular nature of light. Far more often in these quotations he writes of light as some sort of vibration:- "That fundamental supposition is 'That the parts of bodies, when briskly agitated, do excite vibrations in the æther, which are propagated every way from those bodies in straight lines, and cause a sensation of light by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound by beating against the organs of hearing'." He even stated that the vibrations which were responsible for blue and violet light were supposed to be shorter than those which made red and yellow. It is probably correct to assume that Newton was very reluctant to commit himself firmly to any hypothesis on the nature of light. He said[15]: "It is true from my theory I argue for the corporeity of light; but I do so without any absolute positiveness.... and make it at most but a very plausible consequence of the doctrine and not a fundamental supposition."

Later,[16] Newton showed that it was not so much that he was committed to a corpuscular theory of light, but that there was an overwhelming difficulty which prevented his acknowledging that light was a wave motion. "For to me the fundamental supposition itself seems impossible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way."

Newton's explanation of how light affects the nerve endings of the retina sounds almost modern, and he clearly understood that the light stimulated a reaction in the retina which was transmitted to the brain through the nerves:[16a]

> "The ends of the capillamenta of the optic nerve, which pave or face the retina,.... when rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta through the optic nerves into the sensorum (which light itself cannot do) and there, I suppose, affect the sense with various colours...."

Newton's view that the retina was the seat of vision was in agreement with contemporary thinking, since attempts by Mariotte and others to involve the choroid in the process of vision did not meet with much success. It is fair to say, however, that until the end of the eighteenth century, no one had apparently improved upon his explanation of the action of the retinal nerves. His explanation of colour vision was also unique during this period, since no other scientist had strayed from the most basic explanation of vision based upon the action of the retinal nerves, and none had made any significant mention of colour vision.

Thomas Young was therefore the first for a century to attempt to develop the idea originally expounded by Newton. His explanation of his three-colour theory of vision was almost painfully brief, to the point, and as was subsequently discovered, accurate. Newton, by comparison, in spite of his well-known reluctance to commit himself to print, perhaps appears almost verbose. As has already been said, however, Young's reasons for his excessive brevity were not, as Newton's often were, an attempt to avoid intellectual conflict, but were an attempt to prevent his scientific discoveries undermining the confidence of his patients.

Young stated his three-colour theory of vision in the Bakerian Lecture read on 12th November 1801[17]. He acknowledged his debt to Newton by quoting extensively from his works, and agreed that the retina was caused to vibrate by the light falling upon it. However, he could not conceive of the retina's being able to contain sufficient points to be able to vibrate in unison with every vibration or colour. He therefore postulated three principal colours, red, yellow and blue, and that the points on the retina would be put into vibration by the colour which most nearly matched their own frequency. He thought that each nerve contained three portions, each one sensitive to the vibrations of one of the principal colours:[17a]

> "Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance,

to the three principal colours, red, yellow and blue, of which the undulations are related in magnitude nearly as the numbers, 8, 7, and 6; and that each of the particles is capable of being put in motion less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed if those two species; and each sensitive filament of the nerve may consist of three portions, one for each principal colour. Allowing this statement, it appears that any attempt to produce a musical effect from colours, must be unsuccessful, or at least that nothing more than a very simple melody could be imitated by them; for the period, which in fact constitutes the harmony of any concord, being a multiple of the periods of the single undulations, would in this case be wholly without the limits of sympathy of the retina, and would lose its effect; in the same manner as the harmony of a third or a fourth is destroyed, by depressing it to the lowest notes of the audible scale. In hearing, there seems to be no permanent vibration of any part of the organ."

The above passage can be considered to be the entire initial statement of Young's three-colour theory. He made no attempt to justify his choice of colours, by showing experimentally that together they could produce all available hues, including white, and his statement that the undulations were related approximately as the numbers 8, 7, and 6, came from a later passage in his paper, in which he gave a table of colours in terms of their wave-lengths, but using Newton's experimental results.[18] However, it must said in fairness to Young that the idea that white light could be produced by an addition of fewer than the entire seven spectral colours had been known since the time of Newton. In fact it could be fairly said that there was a widespread belief by the end of the eighteenth century that there were three primary colours, from which all other colours could be made.

Young was the first to interpret this fact, not in the nature of colours themselves, but in the nature of man. Perhaps it is possible to deduce that common opinion had it that the three primary colours were red, yellow and blue, since Young referred to them as "the three principal colours", and not merely as "three principal colours". This would certainly help to explain his choice of the wrong primary colours. Young later changed to his famous choice of red, green and violet. He was led to do this by some experimental results obtained by William Hyde Wollaston (1766 - 1828), and it is ironic to note that it was a mis-interpretation of the significance of the results by both Wollaston and Young that led him to choose three colours which would give the correct result.

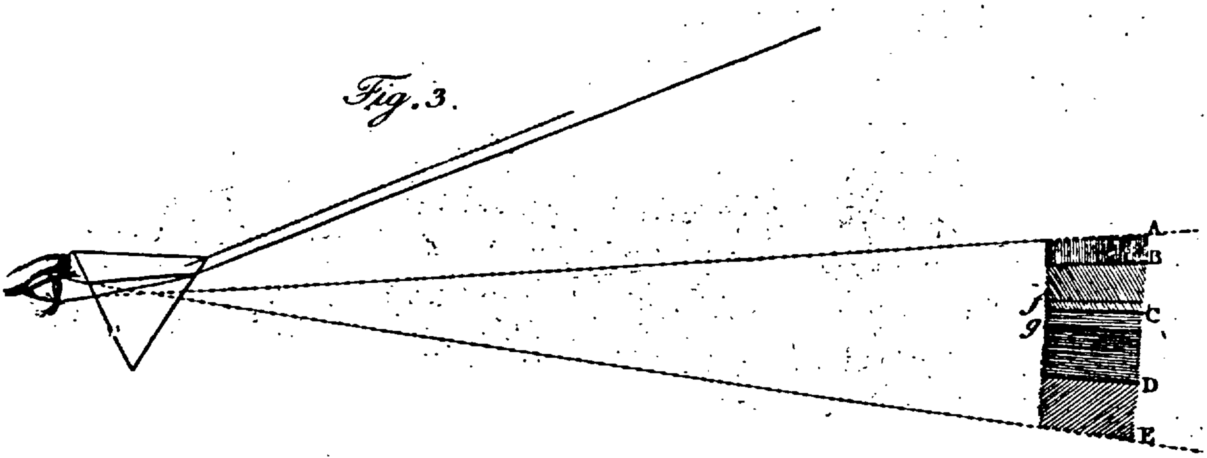
Wollaston read the results of his researches into the refractive and dispersive powers of prisms to the Royal Society on June 24th, 1802.[19] In a section right at the end of the main paper Wollaston commented upon some lines he had seen dividing the solar spectrum:[20]

> "I cannot conclude these observations on dispersion, without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducibly by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, 4 primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.
>
> If a beam of day-light be admitted into a dark room by a crevice 1/20 of an inch broad, and received by the eye at the distance of 10 or 12 feet, through a prism of flint-glass, free from veins, held near the eye, the beam is seen to be separated into the four following colours only, red,

yellowish green, blue, and violet; in the proportions represented in Fig. 3.

The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, f and g, either of which in an imperfect experiment, might be mistaken for the boundary of these colours."

Fig. 3.



What Wollaston had invented, by allowing the light to strike the prism after passing through a slit only 1/20th of an inch wide, was a primitive spectrometer. This had enabled him to see some lines dividing the spectrum into four divisions. Wollaston thought that their function was to delineate the main colour bands of the solar spectrum. In fact the lines represented certain absorption bands in the solar spectrum, and were later called Fraunhofer lines. Wollaston's interpretation of his results can be considered to be very reasonable, and he must have imagined that he had clarified a significant point in the composition of the solar spectrum. No more would there have to be arguments over the number of principal colours, whether seven, four, or three. The solar spectrum had been divided all the time, but it had required more sophisticated apparatus to resolve the dividing lines clearly. Therefore Wollaston was able to say of the spectrum[20] "The beam is separated into the four following colours only, red, yellowish-green, blue and violet."

Seven days after Wollaston read his paper to the Royal Society, Young presented another paper[21] in which he mentioned incidentally that he had modified his choice of principal colours from red, yellow and blue, to red, green and violet, after reading the results achieved by Wollaston. Young said that he had repeated Wollaston's experiments on the spectrum with "perfect success"[22]. In view of the very short time between the presentation of Wollaston's and Young's papers, it is almost certain that Young had prior knowledge of Wollaston's results before they were presented to the Royal Society. Young said that the new description of the solar spectrum had led him to modify his original supposition on the nature of the sympathetic fibres of the retina.[23]

> "In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian lecture, respecting the proportions of the sympathetic fibres of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6."

There is no doubt that, in the statement and subsequent development of his three-colour theory of vision, Young poses a considerable problem. Was the theory evolved through inspired scientific insight, or was it the product of a series of lucky guesses? A fairly superficial examination of the evidence would probably indicate that Young's theory was based upon no experimental evidence. It would have to be admitted that Wollaston had certainly provided some experimental results, but that his interpretation of these results was incorrect. In any case Young, while acknowledging the influence of Wollaston's experiment in causing him to modify his theory, had in fact largely ignored his results. Wollaston had postulated four colours, while Young had used only three, only two of which were included in Wollaston's four. Therefore, on the balance of probabilities, in this area Young could be considered to be lucky rather than inspired.

Looking beyond the bald facts, however, it is possible to argue strongly for inspiration, and in fact to propose that perhaps Young's inspiration in putting forward his hypothesis was greater than it at first appears; indeed it can be seen that Young might well have endeavoured to disguise his own genius in this matter. It can be imagined that Young had an inspired insight into the heart of the problem, and became convinced that there were only three types of colour receptors in the retina. In order to communicate his inspiration to others - a difficult task when one is dealing with the less inspired - he dressed up his ideas in other men's clothes. First, Newton, and it must be added here that Young chose a man whose inspiration in its own time was as great as his own. Second, Wollaston, and here it can be understood why Young deferred to Wollaston's experiment (which nominated four colours to comprise the solar spectrum: red, yellowish-green, blue and violet), and then, without any explanation, put forward red, green and violet as his three principal colours. If Young really intended to modify his hypothesis in the light of Wollaston's experiment, this change is difficult to comprehend. If he were merely using Wollaston's results to add some experimental respectability to his unsupported hypothesis, then his course of action is somewhat clearer. In this case it could be supposed that he first made the change in his hypothesis, substituting green and violet for the original yellow and blue, and then used Wollaston's experiment as a justification for such a change.

It may well be that this latter, rather tortuous, explanation is far from the truth, and indeed the truth does not have to lie in either of the two explanations given above. Perhaps the correct conclusion about whether the hypothesis was lucky or inspired can be found in an assessment of two facets of Young's character. First, we should consider the degree of determination with which the medical practitioner Young was willing to hide the brilliant scientist Young from his patients. Second, there is the scientific stature of the man acknowledged through the quality of his other scientific work. These facts alone would probably lead towards a conclusion that it would be unwise to label Young as the instigator of a lucky guess; but one has also to consider the subsequent embracing of the three-colour theory by other, later scientists, such as Helmholtz and Maxwell. This, together with the slow, steady gathering of experimental evidence in favour of the theory, culminating in the isolation of the three-cone pigments just over ten years ago, which at last proved the theory to be basically correct, must surely mean that it is easier to believe in Young's genius than in his luck.

COLOUR VISION

APPENDIX

Dalton's colour-blindness was of great interest to nineteenth century scientists, and there were a number of humorous anecdotes published about him during this time. Accounts were also written, which differ slightly in detail, about the examination of his eyes which was made after his death.

The Post-Mortem Examination of Dalton's Eyes.

Concerning the fact that Dalton thought that his vitreous humour was coloured blue, William Henry wrote[24]:-

> "This theory was not however verified by an examination of the eye which was carefully made after Dalton's death in conformity with his strongly-expressed wish by his skilful medical attendant, Mr. Ransome. The vitreous humour was of a pale yellow colour, and when used as a lens, it caused no modification of tint in red and green objects."

Lyon Playfair in his Memoirs and Correspondence gave a similar account of the examination of Dalton's eyes, although he stated that Dalton himself thought that his defective colour-vision was due to a peculiarity of the retina.

This was not true, since Dalton had clearly attributed his colour-blindness to a blue coloration of his vitreous humour. Playfair wrote[25]:-

> "When Dalton died on 27th July, 1844, Manchester gave him the honours of a king. His body lay in state and his funeral was like that of a monarch. It is well known that Dalton was colour-blind and he was the first person to investigate this defect of vision. He always ascribed it to a peculiarity of the structure in the retina. When he died, his medical man, Mr. Ransome, took one of his eyes and brought it to my laboratory. I took two

> powders, chrome green and scarlet potassium bichromate, as being the colours which he could not distinguish, but we saw them of the natural colours when Dalton's eye intervened. Ransome, who was a most accomplished physician and great friend of Dalton, assured me that the philosopher when alive would have approved of this experiment being made on his death."

Another account, which differed in a significant detail from the two given above, was written by Sir Henry Roscoe[26]. Roscoe said that it was the lenses of Dalton's eyes which were examined, whereas it is generally accepted that it was the vitreous humour which was suspected of being coloured blue. Perhaps the explanation of this misunderstanding is contained in the quotation from Henry above, when he said: "The vitreous humour was of a pale yellow colour, and <u>when used as a lens</u> it caused no modification of tint in red and green objects." (my underlining). Roscoe's full account of the examination was as follows[27]:

> "The above explanation (that is the one given to the Manchester Philosophical Society) of his peculiar vision was shown after his death to be erroneous. Mr. Ransome, who made the post-mortem, examined the lenses of Dalton's eyes and found them to be normal to a man of his age. The cause lies much deeper and the question whether it is due to a defective condition of the retina or to the optic nerve or the brain substance itself is still a matter of doubt."

<u>Anecdotes Concerning Dalton's Colour-Blindness</u>

Contemporaries of Dalton could not resist the temptation to recount numerous anecdotes about his colour-blindness, especially when his defect led him to wear brightly coloured clothes which his Quaker faith forbade. Sir Henry Roscoe recounted an early mistake made by Dalton when he bought his mother, Deborah, some silk stockings[28]:

> "Thou has bought me a pair of grand hose, John, but what made thee fancy such a bright colour? Why, I can never show myself at meetings with them." Dalton, much disconcerted, told her that to his eyes the stockings were a dark bluish drab, a very proper sort of go-to-meeting colour. "Why, they are as red as a cherry, John." Neither he nor his brother Jonathon could see anything else than drab in the colour of the stockings and they both came to the conclusion that the old lady's sight was strangely out of order, until Deborah, having consulted neighbouring wives on this singular difference of opinion, returned with the reply, "Varra fine stuff, but uncommon scarlety." This was the first event that opened Dalton's eyes to the fact that his sight and that of his brother were not as other men's."

Roscoe also mentions a letter written by Dalton to his "Dear Cousin" Elihu Robinson, in which he remarked[29]:-

> "I was the other day at a friend's house who is a dyer; there was present himself and wife, a physician and a young woman. His wife brought me a piece of cloth; I said I was there in a coat of just of the colour a few weeks before, which I called a reddish snuff colour. They told me that they had never seen me in any such coat for that cloth was of the finest grass green that they had seen. I saw nothing like grass about it. They tell me my table cloth is green, but I say not and that I never saw a green table cloth in my life, but one, and everybody said it had lost its green colour. In short, my observations have afforded a diversion to all and something more to philosophers, for they have been puzzled beyond measure, as well as myself, to account for the circumstances. I mean to communicate my observations to the world through the channel of some philosophical society. The young women tell me they will never suffer me to go into the gallery (of the meeting house) with a green coat; and I tell them that I have no objection to their going on with me in a crimson (that is a dark drab) gown."

It would appear that colour-blindness might even have affected Dalton and others with the same infirmity in matters of the heart, if this light-hearted letter is to be believed[30]:

> "I find by your accounts you must have very imperfect ideas of the charms which in a great measure constitute beauty in the female sex; I mean that rosy blush of the cheeks which you so much admire for being light blue - I think a complexion nearly as exceptional in the fair sex as the sunburnt Moors or the sable Ethiopians, consequently (if real) a fitter object for show than for a wife."

C. Babidge, who was involved in preparations for Dalton to be presented to William IV, gave an account of the difficulties caused both by Dalton's colour-blindness and his Quaker principles:[31]

> "Dr. Dalton, as a Quaker, could not go in a Court dress because he must wear a sword. To this I replied that being aware of this I had proposed to him to let him wear the robes of Doctor of Law of Oxford. Mr. Wood remarked that those robes being scarlet were not of a colour admissible by Quakers. To this I replied that Dr. Dalton had the kind of colour-blindness and that all red colours appeared to him to be the colour of dirt."

One wonders whether Dalton was embarrassed more by wearing ungodly colours which he could not himself distinguish, or by the public curiosity which mistook his scientific distinction![32]

> "The dress of a Doctor of Law is rarely made use of, except at University address and Dr. Dalton's costume attracted much attention and compelled me to gratify the curiosity of many of my friends by explaining who he was. The prevailing opinion was that he was a Mayor of some corporate town come up to get knighted. I informed my enquirer that he was a much more eminent person than any Mayor of any city."

COLOUR REFERENCES

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CONCLUSION

The period covered by this thesis was a fascinating one in the development of science. It saw the foundation of the learned societies, quite often having their origins in England in the coffee houses, and in France in the salons, which allowed ideas to be circulated in a relaxed and informal atmosphere. The Botanical Society (1721), then Linnæan Society (1788) and the Royal Society of Arts (1754) all had their origins in London coffee houses.

In the early part of the eighteenth century, under the presidency of the ageing Newton, the Royal Society in particular brought together the élite of the nation, uniting in its ranks the country's men of letters and science. Pope in his 'Dunciad' has somewhat satirically captured the feeling of this time:

"His children first of more distinguished sort,
Who study Shakespeare at the Inns of Court,
Impale a glow-worm or Vertu profess,
Shine in the dignity of F. R. S."

The time was therefore ripe for the development of science as 'the gentleman's hobby' and it became the age of the scientific dilettante. Science invaded the drawing-rooms of the wealthy, and flourished. One can imagine the experiment to detect the blind-spot, the testing of visual acuity and looking through 'prickled' card, all being established favourites in the after-dinner ritual. During this time the emergence of the encyclopædia could have only added a spur to the interest in science which was so obviously apparent.

It was, therefore, a great age for the development of new ideas, and experimental science has probably not achieved such widespread popularity at any time before or since. However, its popularity was bound to be comparatively short-lived, since the very stimulus which was given to the subject soon carried the bounds of new discoveries beyond the capacity of the dilettante to comprehend, and by the end of the eighteenth century the age of the specialist had arrived; for example, it could not be imagined that Young's detailed and often painful experiments on accommodation would lend themselves easily to the drawing-room technique.

The future of scientific investigation in the nineteenth century was inevitably going to lie in the hands of fewer men, the specialists; hence its progress was likely to be more spasmodic, as it awaited the arrival of the particular man of insight and/or genius to lead it in any specific direction.

During the seventeenth and eighteenth centuries the study of physiological optics was carried forward by such men as Kepler, Descartes, De la Hire, Robert Smith, James Jurin, William Porterfield, Joseph Priestley, John Dalton and Thomas Young. One might also include in this list the name of Newton, since it appears that it was his work which might well have stimulated Young in his search for the three-colour theory of vision. None of these men, except Kepler (for the proof that the image on the retina was inverted) and Young (for the three colour theory of vision and work on accommodation), was responsible for any great step forward. Therefore it can be said that the development of physiological optics during the period from 1600 to 1800 mirrored the spirit of the age, and was carried forward step-by-step by the enthusiasm and interest of a large number of men of wide interests, rather than by the insight or genius and specialised knowledge of one or two.

The study of the internal operation of the eye, however, was not one which lent itself to advance through the frequent repetition of simple experiments. As has already been stated in the section which deals with the part played by speculation and experiment, speculation played a far more important part in the advancement of the subject than did experiment. Therefore physiological optics was not a subject which could be expected to benefit greatly from the enthusiastic experiments of a large number of gifted amateurs; and while topics such as the seat of vision and accommodation attracted widespread interest, progress towards an understanding of how the eye worked was gradual rather than rapid. For example, for most of the eighteenth century there was only one major experiment which had been devised in the field of accommodation - that with the 'prickled' card - and only one phenomenon in the eye which was linked to the act of accommodation - the contraction of the iris for close vision. Similarly, there was only one experiment of the drawing-room kind which was associated with the seat of vision - the locating of the blind spot. These experiments would have ensured that the subject of vision was not neglected, but could have done little to advance knowledge of the working of the eye.

Therefore throughout the eighteenth century, knowledge about the working of the eye moved forward gradually, mainly through speculation based upon a few experiments. In its turn the nature of the speculation differed for the different functions of the eye. In the case of accommodation the speculation was extremely wide, and led to the exploration of virtually all possible methods of causing accommodation. This in turn led to a very gradual hardening of opinion in favour of the correct basic method of achieving a change in focus of the eye, a change in the curvature of the crystalline lens. In the case of the seat of vision the speculation revolved around one possibility only, the retina. In spite of Mariotte's well-founded hypothesis in favour of the choroid, opinion never really wavered from the belief that the retina was the seat of vision; and therefore there was very little discussion of other possible alternatives, but discussion only of how the retina itself could function.

The reasons for the predominance of speculation over experiment are not difficult to find, since the eye does not lend itself readily to experiments designed to discover how it functions optically. Modern techniques have reversed this trend, but in the seventeenth and eighteenth centuries significant optical experiments with the eye were extremely rare. Dissections also had limited value since the delicacy of the structure of the eyeball was such that it was difficult not to damage the membranes during the process of dissection. In any case, in order to discover the nature of operation of the eye it was necessary to examine the living rather than the dead eye; since the dead eye degenerated rapidly in many ways, so that it soon bore only limited resemblance to the living eye.

Nevertheless during this period of two hundred years considerable steps were taken in the solution of the fundamental puzzles of the way in which the eye operated. By the end of the eighteenth century the process of accommodation awaited only the discovery of the peculiar operation of the ciliary muscles before it was completely understood; the basis of colour vision had been explained by Young's brilliant hypothesis; and the fundamental operation of the retina had been established, awaiting only the results of the very sophisticated techniques which were not developed until the twentieth century, before it could be better understood.

The period covered by this thesis was remarkable for the widespread surging interest in scientific matters. So much knowledge of the immediate world was apparently on the verge of discovery and revelation. And yet, in the case of physiological optics, the discoveries when they have come, have proved harder and more complicated than might have been imagined in the eighteenth century. Perhaps an apt analogy is to be found in the investigation of atomic physics in the early decades of this century; the secrets of the atom were then being discovered with apparent ease, and it appeared that the whole problem of atomic structure might be solved with far less difficulty than has subsequently been the case.

Nevertheless the eighteenth century was probably the last period of time when knowledge on all scientific topics lay within the reach of any man's own rational thought and patient observation, virtually unaided by technology and inspired only by his own desire to understand the complexity of a perfectly ordered universe.

Appendix A

A MODERN EXPLANATION OF THE WORKING OF THE EYE

THE OPTICAL SYSTEM.

A modern diagram of the eye is shown in fig. I overleaf. The front, called the cornea, is transparent and has a great curvature than the rest of the eye (about 8mm or 0.3 in.). The remaining surface is opaque, and is called the scleral segment. It has a radius of 12mm. or 0.5 in. It is perhaps surprising that the dimensions of the eye differ only slightly from person to person, regardless of their size. The covering of the eye is made up of three layers called coats. The outer consists of the cornea and sclera; the middle contains the main blood supply to the eye and consists, from the back to the front of the eye, of the choroid, the ciliary body and the iris. The innermost layer is the retina, lying on the choroid, and receiving most of its nourishment from the vessels within the choroid, although some comes from vessels within the retina itself which can be seen with an ophthalmoscope.

The cornea contains no blood vessels, since these would impair its transparency. This means that it is virtually isolated from the rest of the body, and has to obtain its nourishment from the aqueous humour. It is this isolation which makes it possible to transplant corneas from other individuals, since antibodies will not be carried to the transplanted organ and destroy it. The aqueous humour is constantly secreted and absorbed, being renewed about every four hours. Its main purpose is to keep the eyeball reasonably firm, and it is produced by the ciliary body. The other major part of the optical system of the eye is the crystalline lens. The cornea, aqueous humour and lens act together to

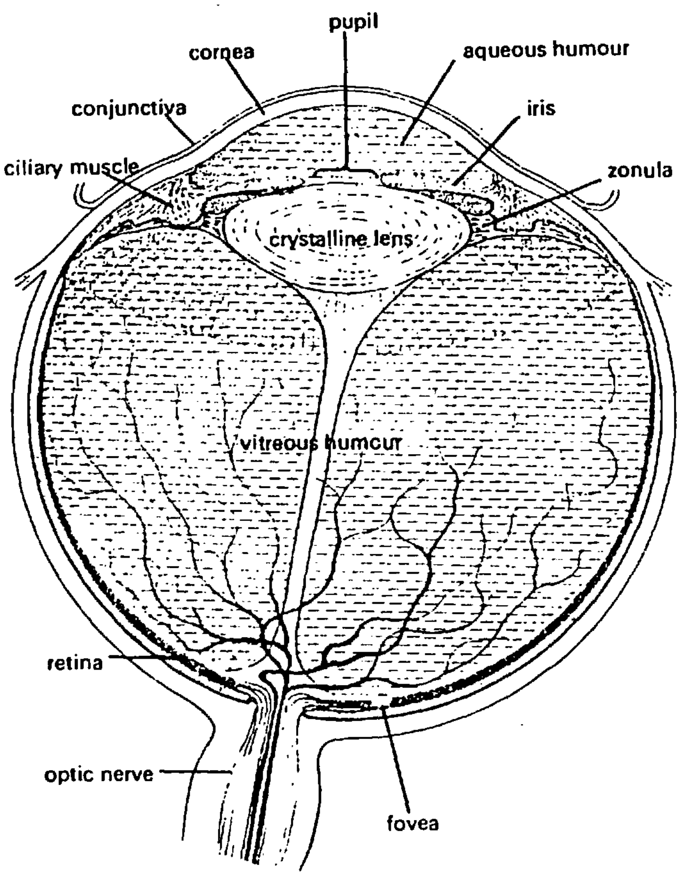


Figure I

A Modern Diagram of the eye

produce an inverted image on the most sensitive part of the retina. The greatest refraction takes place at the cornea, which has a refractive index of 1.3376; the aqueous humour has a refractive index of 1.336, so that very little refraction takes place between the posterior of the cornea and the aqueous humour. The lens has a greater refractive index than the aqueous or vitreous humours, a necessary factor for accommodation to be able to take place; it lies between 1.386 and 1.406. The posterior surface of the lens is more curved than the anterior, and thus contributes more to the refraction of the rays as they pass through the lens. Accommodation is the means by which an eye can focus upon objects at different distances. The closest point which can be clearly seen is called the near point of accommodation, and this changes with age. In the very young it is about three inches; at forty it has increased to about six inches; and at sixty it is about thirty nine inches.

The mechanism of accommodation is essentially an increase in the anterior curvature of the lens. The lens is enclosed in a transparent bag, called the capsule, and this is held around its periphery by zonular fibres, which in turn are attached to the ciliary muscles. It is the pull of the zonular fibres on the elastic capsule that holds the anterior surface of the lens relatively flat. When this pull is relaxed, the elasticity of the capsule causes the anterior surface to become more convex. The ciliary muscle presented early researchers in the mechanism of accommodation with a great deal of difficulty. Initially they found it difficult to decide which part of the suspensory system was a muscle, since no part had the characteristics which were thought to be essential in muscles, e.g. a red colour.

In addition, the action of the muscle is the reverse of that which would seem to be obvious. As the muscle tenses, the effect is to relax the tension on the zonular fibres. The early scientists associated the existence of a muscle with a pulling power; thus they fell into the error of assuming that the suspensory ligaments-the zonular fibres-pulled upon the edge of the capsule, and stretched the lens into a less convex shape. Thus the eye was assumed to be at rest when viewing close objects, and the lens stretched by the zonular fibres- to view distant objects. This was contrary to common experience, which was that the eye was at rest for distant objects, close objects being viewed clearly only by means of some effort (e.g. the eyes becoming tired with continued close work). This back-to-front working of the ciliary muscle, together with its lack of red pigmentation, must be considered to be responsible for many of the ingenious theories of accommodation which are dealt with in the text. The diminution of accommodating power with age, which has been mentioned earlier, is not due to a failure of the ciliary muscles, but to a hardening of the substance of the lens with age.

In spite of these difficulties, it was thought by Scheiner as early as the seventeenth century, that a change in shape of the lens might have a part to play in accommodation. One hundred and fifty years later, however, the position was still in considerable doubt, and eminent scientists were arguing that accommodation was still possible in those who had their lens removed during cataract operations; and that therefore the lens could not be the agent by which accommodation was achieved. The ciliary muscle was eventually isolated in the middle of the nineteenth century by Brücker and Müller; initially it was thought to be two muscles acting at right angles, but eventually it was acknowledged to be one muscle with two sets of fibres, one radial, the other forming a ring. When the muscle tenses, the whole ciliary body moves forward slightly, so that the suspensory ligament which holds the lens in place is loosened and the lens, owing to its natural elasticity, becomes fatter. In this way the eye is accommodated for close vision, and with the ciliary muscle contracted. Therefore close vision is associated with effort, distant vision with the eye being at rest, and the hypothesis agrees with everyday experience. Accommodation is a reflex action, and its stimulus is the nearness of the object. However, it is not clearly understood, since an object which appears blurred may be too far away, or too close to the eye; therefore something else is required in addition to an object being blurred, to instruct the eye to accommodate further away, or closer.

The quantity of light entering the eye is regulated by the hole in the centre of the iris, the pupil. The iris reacts almost instantaneously to a change in light intensity, enlarging the pupil to about eight millimetres in dull light, and contracting to about three millimetres diameter in bright light. There is an additional cause of a constriction of the pupil, and this occurs when a close object is viewed. This is called the near reflex, and thus the pupil also plays a part in accommodation. The effect is to reduce the aberration caused by rays of light passing through the edge of the lens, and to increase the depth of focus of the eye when it views close objects. The near reflex was recognised in the seventeenth century. Dilation of the pupil also occurs during strong psychical stimuli. This has been recognised for a long time; merchants used to watch for dilation of the pupils of their customers, indicating particular interest in what was being sold. Women used to artificially dilate their pupils with 'belladonna' (beautiful woman), to make themselves more attractive. Recent research has discovered many more subtle variations of this theme. Given two pictures of an attractive girl, identical except that one picture has been retouched to enlarge the pupils, men tend to choose the one with the enlarged puipls as being prettier, although they are unable to offer any logical explanation for their choice. In another series of experiments, women and married men with children show a dilation of the pupils when shown a picture of a mother and baby. It would appear then that the role of the pupil as an indicator of emotional response has long been subconsciously appreciated, but only very recently has it been the subject of detailed research.

The role of the iris is, therefore, extremely varied, and its main function has been widely held to be the regulation of the amount of light entering the eye; it is now thought that its influence on the aberrations of the optical system of the eye is probably of greater significance than its light-regulating role. The smaller the pupil, the less serious, in general, are the aberrations. If, however, the pupil becomes too small, then the effects of diffraction become significant. It has been found that in bright light, a decrease below a diameter of three millimetres for the pupil, does not improve the visual acuity. It has also been found that the diameter of the pupil,at any given level of light, gives the best compromise possible between visual acuity and the amount of light entering the eye. This is possible because the increased amount of light entering through the pupil helps to compensate for the reduced acuity produced by the enlarged pupil.

THE RETINA

Sensitivity to light.

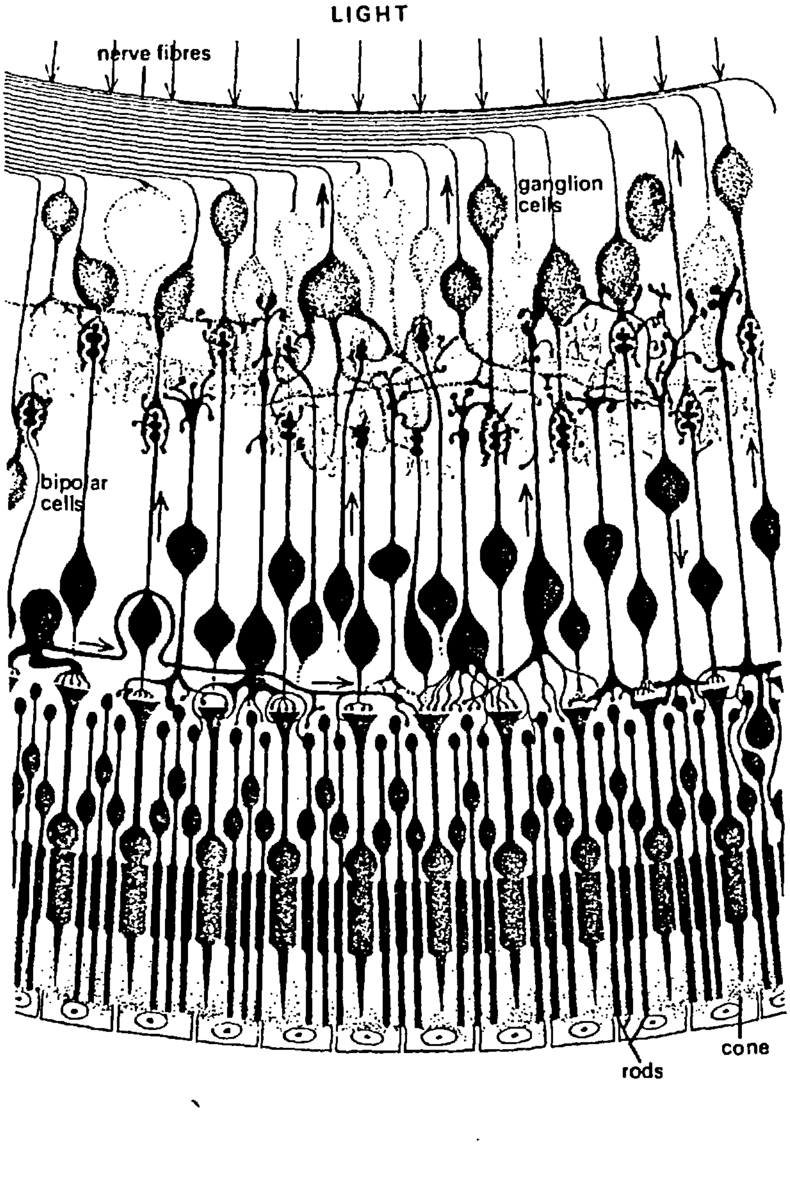
It is reasonably apparent from everyday observation that the eye is far more sensitive in dull light than in bright light. For example, on entering a darkened room, details which at first were not seen, slowly become visible. This gradual increase in sensitivity is not due to the enlargement of the pupil, since this takes place almost instantaneously. Experiments have shown that after about thirty minutes in the dark, the eye may become about 10,000 times more sensitive than in bright light; and it has also been shown that under these conditions one must look away from the object to gain maximum sensitivity, so that the image does not fall on the centre of the retina, the fovea centralis. The other factor that must be mentioned when dealing with vision in very dull light is that it is without colour. Therefore we know that the sensitivity of the retina to light varies with the amount of light falling upon it, and that the sensitivity of the fovea is less than that of other parts of the retina. Moreover the eye has the ability to see colour in bright light, but not in dull light. The receptors in the retina which are not receptive to colour, but which can possess a greater sensitivity to light, are called rods; the colour-sensitive receptors are cones.

Since colour vision contains many more complications than the monotone vision from rods, it will probably be simplest to start with an explanation of the function of rods. Rods are found only on the edge of the fovea centralis, and in increasing density as the distance from the fovea increases. Both rods and cones are found towards the outer layer of the retina, and thus light has to filter through the retina, before reaching the light-sensitive cells. The part of the retina used for accurate vision, the fovea centralis, overcomes this difficulty by containing no blood vessels, and having the intervening layers of the retina made extremely thin, so that light has almost unrestricted access to the cones it contains.

Rods, even though they give no sensation of colour, are more sensitive to blue-green light than to orange. It has been possible to extract from the retinas of animals, whose eyes were dark-adapted, a chemical, originally called visual purple, but now called rhodopsin. If the same experiment is now carried out, but with the eyes not dark-adapted, then little of the pigment is obtained. Thus it appears that in looking at bright light rhodopsin is changed to some other compound, and as the eye becomes dark-adapted, rhodopsin is manufactured. Since it is not possible to extract rhodopsin from the eyes of animals containing only cones, such as chickens, it is assumed that rhodopsin is the visual pigment of the rods, and in absorbing light energy it is changed into another compound. Thus rhodopsin absorbs light, and converts its vibrational type of energy into some other form that is eventually changed into electrical charges which are transmitted to the cells to which the rods are connected. It is not known why rods have a greater sensitivity to light than cones. They might have a greater concentration in them of pigment, which would enable them to produce a greater change for a given amount of light, or they might be more efficient at transforming light energy into electrical energy.

Another facet of vision which varies with the intensity of light is visual acuity; this is the power to distinguish detail, but for measuring purposes it is considered to be the power to resolve a simple test

4·9 The retina. Light travels through the layers of blood vessels, nerve fibres and supporting cells to the sensitive receptors ('rods' and 'cones'). These lie at the back of the retina, which is thus functionally inside-out. The optic nerve is not, in vertebrate eyes, joined directly to the receptors, but is connected via three layers of cells, which form part of the brain externalised in the eyeball. 47



A Cross-section of the Retina

pattern, say a pattern of white lines on a black background. The angle subtended at the eye by two adjacent lines at the point where they are just able to be resolved, is called the resolving power of the eye; the reciprocal of this angle in minutes of arc, is called visual acuity. Thus a visual acuity of unity indicates a power of resolving detail subtending one minute of arc at the eye, and a visual acuity of two indicates a resolution of one half a minute, or thirty seconds of arc. The best possible acuity is about two. When the illumination is reduced the acuity falls, so that under ordinary daylight conditions it is not much better than unity; in very poor light conditions it might be only .04 so that the lines at the eye would have to subtend about twenty five minutes in order to be resolvable. One could imagine that the limit of the resolving power of the eye would be determined by the 'grain' of the retina mosaic; thus if we wished to resolve two white lines separated by a black line, the image of one white line would have to fall on one row of receptors, the image of the black line would have to fall on the next, and the next white line would have its image on the next row of receptors. Thus the limit of resolution, which would be best in the fovea centralis, would depend upon the diameter of foveal cone; in fact this corresponds to a resolving power of about thirty seconds of arc. Therefore we can see that the eye produces in practice a visual acuity as good as its theoretical limit, i.e. two or thirty seconds of arc. However, all the preceding arguments have assumed that each rod or cone possesses a direct connection to an individual nerve fibre, which will pass its message to the brain. Unfortunately, the situation is not as simple as this. In fact foveal cones do have such direct individual connections, but they also share connections with each other. Under ideal viewing conditions these other 'cross' connections would be inhibited, and each foveal cone would have its unique connection to a nerve fibre. In very dull light, the opposite would tend to occur, with a number of receptors being connected to one nerve fibre. In fact it is this ability to vary the connections of the receptors, which leads both to the greater sensitivity to poor light of the rods, and also to the poorer visual acuity of the eye under poor lighting conditions. When the quantity of light is very small, the rods join together to send a combined message of numerous rods. Thus the retina has a resolving power based upon much larger areas of sensitivity than the size of the individual receptor.

Strangely enough, this very property of cells having links with others, can under certain conditions, increase visual acuity of the eye. If an image of alternate black and white lines is projected on the retina, the sharpness will be far from perfect. The defects in the optics in the eye, and diffraction at the pupil, will all help to create an image where the black lines tend to be rather blurred areas of varying shades of grey, which in turn, fade into the areas of white. The receptors themselves improve the definition of the image; the receptors that receive the most light tend to inhibit those that receive less, and the result is a physiological 'sharpening up' of the image seen by the observer.

Colour Vision

On November 12th 1801, during the course of delivering the Bakerian Lecture to the Royal Society, Thomas Young put forward a theory of colour vision. In this he supposed that the retina was sensitive to only three principal colours, red, yellow, and blue. On July 1st in the following year, he modified this theory slightly, so that the principal colours became red, green and violet. The theory[1] was put forward almost as an aside in the course of a lecture which Young said was not "so much to propose any opinions which are absolutely new, as to refer some theories, which have been already advanced, to their original inventors, to support them by additional evidence, and to apply to them a great number of diversified facts, which have hitherto been buried in obscurity." It would be difficult to imagine a more inaccurate description of his theory, the cumulative impact of which has, over the intervening years, been immense. The theory was based upon little, if any, experimental evidence, and probably for this reason was not immediately developed. It was taken up by Helmholtz in 1852, and eventually came to be widely accepted as the 'Young-Helmholtz' theory. Nevertheless, it remained as a theory and one which was extremely difficult to confirm by direct scientific experiment; and it is not until very recent years that the theory has received experimental verification. In 'Scientific American' December 1964[2] we have the following statement:- "Spectrophotometric measurements of individual cone cells in the retinas of the goldfish, the rhesus monkey and man, conducted in our laboratory at John Hopkins University and also at Harvard University and the University of Pennsylvania, have now confirmed Young's three-receptor hypothesis." It can therefore be correctly inferred that colour vision is a topic which, for its experimental investigation, involves the use of the most sophisticated scientific techniques.

Cone pigments are difficult to extract, and it has not yet proved possible to extract them from the retinas of mammals by biochemical means. However, it has been possible to identify one cone pigment from an analysis of a solution of chicken-retina pigments; this has been called iodopsin. However, the method which has been most successful in the search to see whether there are three distinct cone pigments is not chemical, but has depended upon the analysis of the light reflected from the cones in the retina. The technique uses a well-known phenomenon, namely that the light reflected from the retinas of some animals is coloured green. Thus it has suffered some kind of absorption as it was reflected from the retina. By analysing this light it should be possible to discover something about the absorption spectra of the cone pigments. The difficulties in carrying out this technique have only just been overcome. The light which is projected on to the retina must be of very low intensity, otherwise the cone pigments will be bleached, since it is by the bleaching of the pigments that the eye 'sees'. The analysis of the light has to be carried out electronically, and, until recently, the sensitivity of the equipment has not enabled it to distinguish the very small signal produced by the reflected light, from the background noise produced by the equipment. Another technique involves the preparation of sections of the fovea centralis on microscope slides, and analysing the spectra of light transmitted through the cone cells in the section. Both methods have had to overcome an initial difficulty, that of obtaining a pencil of light narrow enough to fall within the fovea, and so fall only on cones. A wider pencil which fell also on the rods around the fovea, would reflect light which also contained the absorption spectrum of rhodopsin, the rod pigment. While rods are not able to transmit information about the colour of objects, rhodopsin does have a definite absorption spectrum, being much more sensitive at the blue-green end of the spectrum, than at the red end.

The first experiments using the technique of analysing light reflected from the central area of the retina were carried out in 1955 at Cambridge by F.W. Campbell and W.A.H. Rushton. They succeeded in identifying two different pigments present in the fovea. One pigment, which they called chlorolabe, was most sensitive in the green part of the spectrum, and another, erythrolabe, had its peak sensitivity in the yellow. Other similar experiments carried out at Harvard identified a green-sensitive and a red-sensitive pigment. However, the method was not able to identify a blue-sensitive pigment, neither was it able to establish whether the pigments were mixed together in the same cones, or were contained in different cones.

In order to discover whether cones contain more than one pigment it would be necessary to analyse the transmission, or reflection spectrum from a single cone. This would involve shining a pencil of light on to a target which had a diameter of between five and two microns. This extremely difficult technique was developed by E. F. MacNichol and W. B. Marks of Johns Hopkins University, and their results were published in 1964.[2] They found that by preparing a section of goldfish retina sandwiched between two microscope slides they were able to compare the light transmitted through a single cone cell. with that which passed through no cones. The results showed that there were three distinct pigment-absorption spectra present in goldfish cones, with peaks of absorption in the blue, green and red bands of the spectrum. Subsequent work with human retinas shows similar results. They were also able to show that the blue absorption curve corresponded to the absorption spectrum of iodopsin, which, as has been mentioned before, was the pigment extracted by chemical means from chicken retinas. The same research team were also able to show that the pigments were not mixed within the cones, but that single cones contained single pigments. Therefore it is now possible to say that we see colour because our retinas contain cones which are sensitive to red, green, and blue light, and that the theory originally put forward by Thomas Young has, after one hundred and sixty years, been largely proved correct.

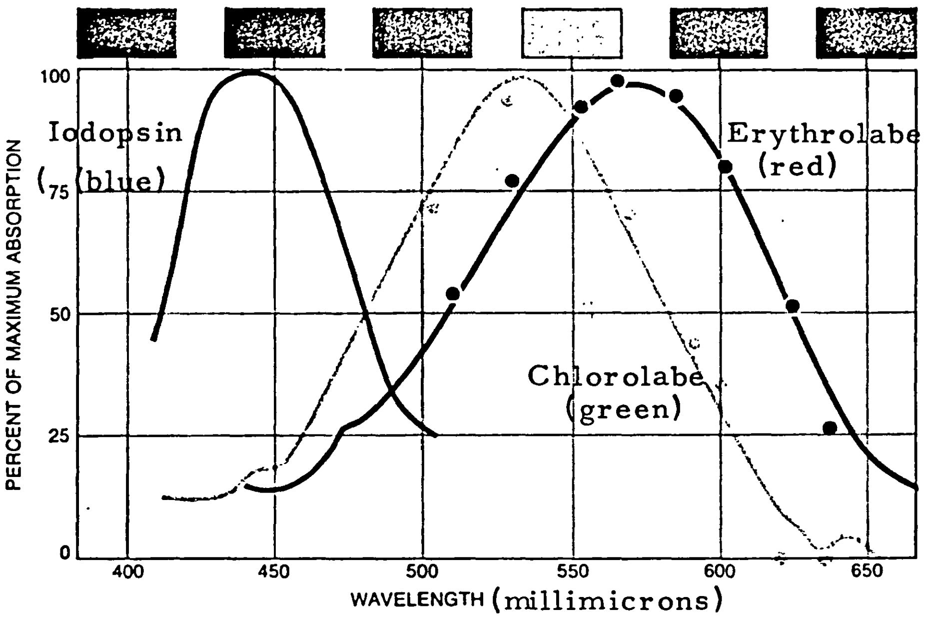
Colour Defectiveness.

Those who suffer from this defect cannot discriminate between certain colours as well as the majority of people, and they see many colours as identical that normal people would see as different. The defect is much rarer in women than in men, and is caused by a defective gene in the X chromosome. Women have two X chromosomes, and are colour-defective only if both lack the necessary gene. Men are colour-defective if their one X chromosome lacks the necessary gene. One type of colour-defective is the dichromat, and about one percent of men are dichromats. They can mix all the colours of the spectrum, as they see them, with only two primaries instead of three. Thus the protanope - the red blind - requires only blue and green to make his matches; since for the normal - trichromatic - subject the various reds, oranges, yellows and many greens are the result of mixing red and green, the protanope matches these with a green. He cannot distinguish between them on the basis of their colour, and if he does make a distinction, it is on the basis of their different brightness. The deuteranope - green blind - matches all his colours with a mixture of red and blue. He is unable to discriminate reds, oranges, yellows, and many greens, so that both types of dichromat are classed as red-green blind. The protanope, however, has a more limited spectrum, since he is unable to appreciate red, and, as we shall see later, this makes for a more noticeable deficiency. The tritanhope - blue blind - is extremely rare, constituting between one in 13,000 to 65,000 of the population.

It has been possible recently to establish that colour defectiveness is due to a lack of a cone pigment, or the presence of anomalous cone pigments. W.A.H. Rushton[3], using the method mentioned in the previous section, analysed the spectra of light reflected from various men suffering from colour defectiveness, and compared it with the spectra from normal eyes. He discovered that protanopes were deficient in the cone pigment erythrolabe; and that deuteranopes were deficient in chlorolabe. Thus, since the protanope lacks the pigment responsible for red vision, it is clear that they find red lights dim. Deuteranopes, however, do not find green lights dim, even though they lack the pigment which absorbs most light in the green section of the spectrum. This is because the spectra of the three pigments overlap, as can be seen from the diagram overleaf. It can be seen that a person lacking chlorolabe will still receive a signal within the chlorolabe wavelengths, since his erythrolabe pigment is reasonably sensitive within this range. However, he still will not be able to distinguish between red and green colours.

Another significant discovery which was made during the course of these experiments was that the cone pigments of dichromats were identical to those of normally-sighted persons.

The majority of colour defectives are not dichromats, however. They can see three colours, and using a colour-mixing device, need a mixture of three spectral lights to establish colour matches, but they do not mix the colours in the normal proportions. They are called anomalous trichromats. Using the same techniques as before of spectrum analysis, Rushton was able to show that anomalous trichromats possess



THREE CONE PIGMENTS of normal color vision absorb lights of different wavelengths as plotted here. The curves are the average spectral absorbance from single cones in excised eyes of humans or monkeys scaled to the same maximums. The measurements were made by Edward F. MacNichol, Jr., and his colleagues at Johns Hopkins University. The colored dots represent the pigments in the green-sensitive and red-sensitive cones as they were measured in the living human eye by H. D. Baker and the author at the University of Cambridge. The coincidence of the two sets of measurements demonstrates that single cones contain single pigments. The color patches that appear above the curves approximate the colors of the spectrum at the wavelengths indicated at the bottom of illustration.

Curves showing the spectral absorption of the three Cone Pigments

pigments which are different from those possessed by normally-sighted people, but whose absorption spectra differ only slightly. Nevertheless, this slight difference is sufficient to explain why colour discrimination is difficult for these subjects.

Erect Vision

Since the sixteenth century, when it was demonstrated by Scheiner and Descartes that the image formed on the retina was inverted, it has been commonly assumed that we learned by experience in infancy to invert mentally the retinal image to correspond with the outside world, which our other senses told us, was erect. Some recent research has thrown some doubts on our ability to learn in this way.[4] R.W. Sperry found that in experiments in which he rotated the eyes of newts through 180°, the newts clearly saw the world as inverted - striking upwards for food held below them - and that even after as long a period as two years, they had not adapted to this new mode of vision. He obtained similar results with frogs, with the same reversal of the eye. If we do learn by experience that what the retina shows us is the world inverted, then the sense of touch must play an important part in this education. In another recent article[5] I. Rock and C.S. Harris have described experiments which clearly show that the sense of touch is dominated by our visual perception; and that when these two senses transmit conflicting evidence, then it is vision which dominates. However, these articles should probably only restrain us from a too-glib acceptance of the traditional explanation of how we interpret the inverted image presented to us. Other recent research, in its turn, tends to underline the importance of our early visual experience; particularly the way in which certain commonly occurring visual patterns can be strongly imprinted on our visual processes, when they occur at a very early age. For instance, it is thought that the horizontal/vertical bias of modern architecture may be indelibly imprinted on the minds of urban man. It is a fact that people in general resolve horizontal and vertical lines better than they do oblique ones. Recent experiments on a tribe of Indians living in tepees failed to show a similar preferential resolution of horizontal and vertical lines, and this has supported the theory of early imprinting.

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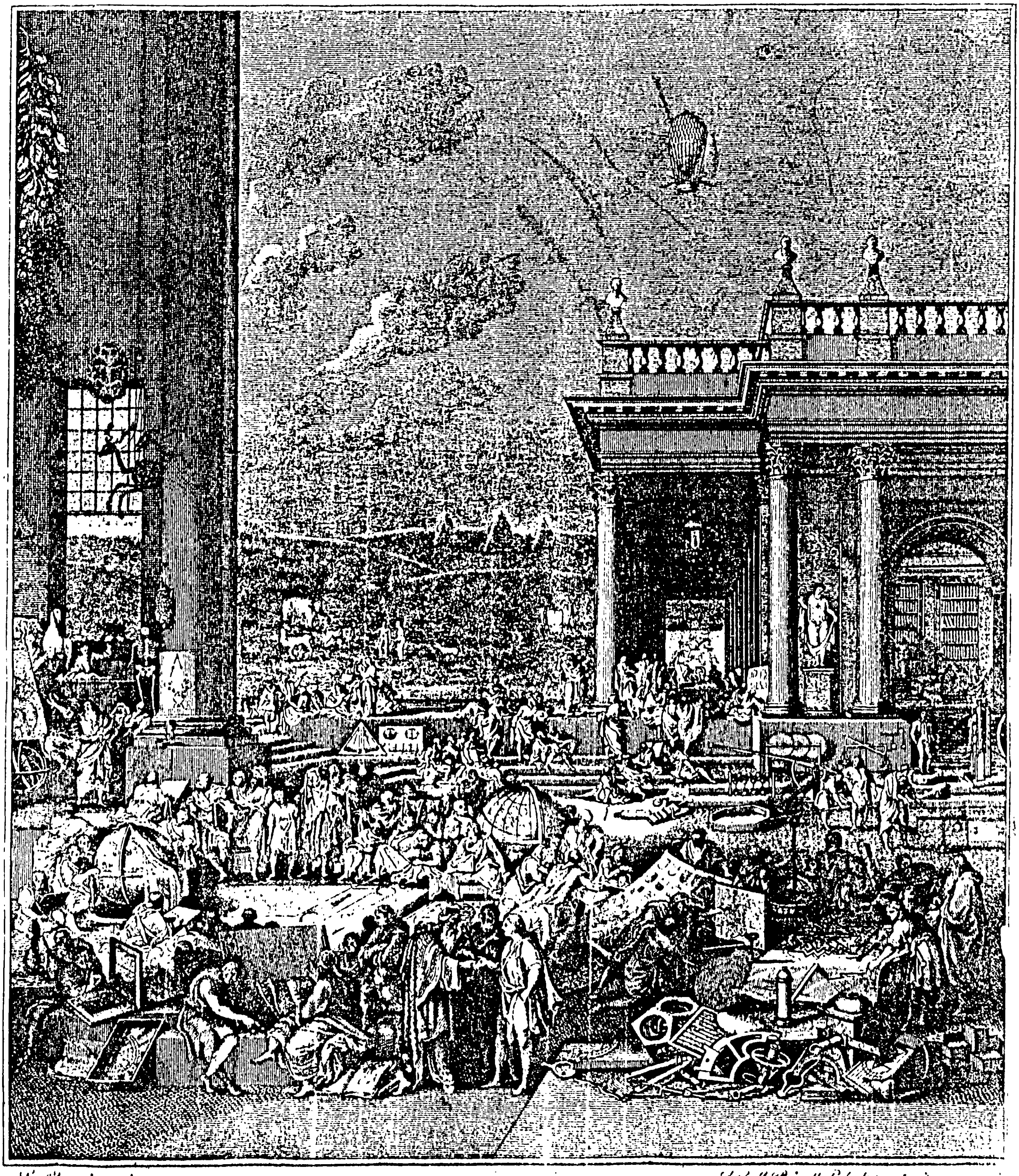

Le Clerc invenit.

Frontispiece from the 1797 Edition of the Encyclopaedia Britannica.

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OR, A

DICTIONARY

OF

ARTS, SCIENCES,

AND

MISCELLANEOUS LITERATURE;

Conſtructed on a PLAN,

BY WHICH

THE DIFFERENT SCIENCES AND ARTS
Are digeſted into the FORM of Diſtinct

TREATISES OR SYSTEMS,

COMPREHENDING

The HISTORY, THEORY, and PRACTICE of each,
according to the Lateſt Diſcoveries and Improvements;

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VOL. I.

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Appendix B

THE TRANSMISSION OF KNOWLEDGE IN THE EIGHTEENTH CENTURY THROUGH ENCYCLOPAEDIAS.

The period of the seventeenth and eighteenth centuries was characterised by the development of the encyclopaedic dictionary. The development followed two major and independent paths; there was the type of encyclopaedia that paid particular attention to history and biography; and there was a new form of encyclopaedia that devoted itself to the arts and sciences. It is this latter type which is of particular interest here, and three examples published in the eighteenth century have been chosen to give some idea of the sort of information which was available to the layman. In particular it is hoped in this appendix to give some idea of the degree to which the discoveries and theories in the field of physiological optics could have been transmitted to the well-educated person who was not a member of a learned society.

The first encyclopaedia to be chosen is John Harris' Lexicon Technicum published in 1704. The publication of this work in two volumes followed the successful publication of similar volumes in Europe. It was the first to be written in English rather than be translated from French, and it represented the powerful impact of the work of the Royal Society. The author described his work as "An universal English dictionary of arts and sciences: explaining not only the terms of art, but the arts themselves." The second encyclopaedia to be chosen is the second edition of Chambers' Cyclopaedia (1738). Ephraim Chambers continued the trend followed by Harris and included more articles on the arts and sciences while giving less prominence to people. Chambers' work was again published in two volumes. By the end of the eighteenth century, however, the number of volumes which could constitute an encyclopædia had grown considerably. Thus the third edition of the Encyclopædia Britannica (1797), which has been chosen as the third example, was published in eighteen volumes.

LEXICON TECHNICUM

There were two references to the eye in this work (which had no pagination) one in the main body of the work, and the other in the supplement.

The description of the eye, which was referred to as "the wonderful Organ of Sight" - was largely anatomical, with a good description of its parts. The functions of the parts of the eye also seem to be well understood and stated, although the importance of the retina was emphasised more here than when its function was discussed in the article headed "Vision". Here the retina was described in the following way:

> "The third Tunicle is made of the Medullary Substance of the Optick Nerve, and is called the Retina, or Retiformis, (Net-like). This seemeth to be the principal Organ of Sight. For as Dr. Briggs well argues, neither the Crystalline Humour, through which the Rays pass much refracted; nor the Tunicle Choroides, are at all fit for this Use."

Later, the function of the crystalline lens was well-described, and the action of the retina confirmed:

> "As to the Collection or Reception of the Rays of things visible, this Humour (the crystalline lens) is the primary Instrument of sight; tho' as was said before, the Tunica Retina is the Principle as to Perception, because through it the Rays are communicated to the common Sensory."

In contrast to this clear and correct interpretation of the operation of the eye, the Supplement contained only a brief and idiosyncratic account of the eye, confining its information largely to the working of birds' eyes. The eye was described as "A subject too copious for us to enter upon."

In the section on Vision there is the following good definition:

> "Vision: A sensation in the brain, proceeding from a due and various Motion of the Optick Nerve, produced in the bottom of the Eye, by the Rays of Light coming from any Object."

Although in this section the function of the retina is left in some doubt:

> "Whether the Picture of the Object be made on the Tunica Retina, or on the Choroides, there is a great Dispute between Mr. Pecquet and Mr. Mariotte, in the Philos. Trans. N. 59, etc."

The main part of this section was given over to a comprehensive account of the formation of the image in the eye, but before this there was a brief but clear account of various theories of vision which had been held since the time of the Greeks.

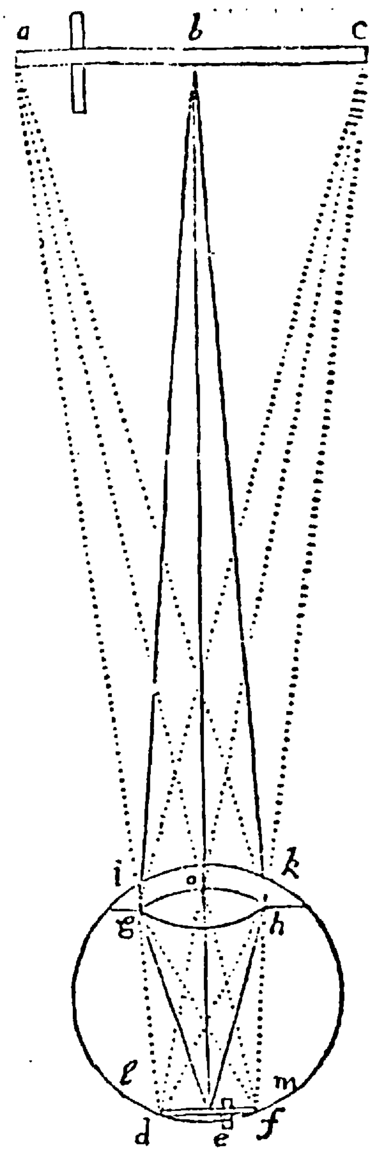
Descriptions were given of the formation of the image on the back of the eye, illuminated with clear diagrams. In spite of the work of De la Hire, who questioned the necessity for any form of accommodating power other than the contraction of the iris, and whose work had been published a few years earlier, Harris assumed that it was necessary that the eye could adjust for different distances. He put forward two possibilities without favouring either. He said that the lens could move closer to the retina for distant objects, and further from it for closer objects, or the lens could change shape:

> "Therefore Nature has so contriv'd the Eye, That it should have a Power of adapting itself in some measure to nigh and distant Objects, for they require different Conformation of the Eye, because the Rays proceeding from the Luminous Points of nigh Objects do more diverge than those from more remote Objects; But whether this Variety of Conformation consists in the Crystallines approaching nigher to, or removing farther from the Retina: Or in the Crystalline assuming a different Convexity, sometimes greater sometimes less, according as is requisite, is left to the Scrutiny of others; and particularly the Curious Anatomists."

The problem of the inverted image formed on the retina was also dealt with, although the explanation of how we recognise the erect nature of the object when the image was inverted, is not well explained:

> "But how comes it to pass, that the Eye receiving the Representation of a part of an Object on that part of its Fund which is lowermost or nighest the Center of the Earth, perceives that part of the Object as uppermost, or farthest from the centre of the Earth? In answer to this, let us imagine, that the Eye in the point f receives an Impulse or Stroke by the Protrusion forwards of the Luminous Axis aof, from the Point of the Objecta a: must not the visive Faculty be necessarily directed hereby to consider this Stroke, as coming from the Top a, rather than from the Bottom c. and consequently should be directed to conclude t the Representation of the Top?"

(Presumably the 't' mentioned in the last line of the above quotation refers to the fact that the object abc is in the shape of a 't')



Other matters mentioned were myopia and the inability of the old to see close objects clearly. The correction of myopia by concave lens was mentioned However, the inability of the old to focus on close objects was attributed to their crystalline lenses being too flat, and not, as it should be, to the inability of the lens to become more convex for close objects. This was a common and not surprising error since, at the beginning of the eighteenth century, presbyopia (the loss of accommodating power with age) was not well understood. The correction of presbyopia, however, was well comprehended, and Harris stated that the convex lens used for looking at close objects helped old people to see, not by magnifying the objects, but by making their appearance distinct.

CHAMBERS' CYCLOPAEDIA (Second Edition) 1738.

Although this encyclopædia was published in only two volumes, as was the Lexicon Technicum, the subject of the eye and vision was far more comprehensively dealt with. Harris' work was, however, superior in one area, that of diagrams. Harris included his diagrams in the text, while Chambers had pages of plates interspersed in the text, and the diagrams were small and cramped. There was again no pagination in Chambers' Cyclopædia.

The anatomical description of the eye was dealt with fully and well, and there was also a full historical review of theories of vision from ancient Greek times to the modern theories of Descartes and Newton.

The contemporary reader of Chambers' work was probably left in some doubt over the light-sensitive membrane within the eye. There were frequent references to the image being formed on the retina, but a great deal of emphasis was also placed upon the controversy that stemmed from Mariotte's theory that the choroid was the light-sensitive surface. It is probably fair to say that Chambers adopted a fence-sitting posture on this topic, and also allowed himself to be over-influenced by the work of a very few scientists whose thoughts led them to oppose the tide of very firmly held opinion in favour of the retina as the sensitive surface. Chambers said:

> "The retina is usually supposed to be the great organ of vision, which is effected by means of the rays of light reflected from each point of objects, refracted through their passage through

the aqueous, vitreous and crystalline humours, and thus thrown on the retina; where they paint the image of the object; and where they make an impression which is continued thence by the fine capillaries of the optic nerve to the sensory. Indeed whether the retina or the choroides by the principal organ of vision and that whereof the images of objects are represented has been much controverted between several members of the Royal Academy, particularly Ms. Mariotte, Pecquet, Perrault, Mèry and de la Hire - Mariotte first stood up for the choroides and was seconded by Mèry: the rest asserted the cause of the retina. The retina was always adjudged to have all the characters of the principal organ - It is situated in the focus of the refraction of the humours of the eye; and of consequence receives the vertices of the cones of rays proceeding from the several points of objects. It is very thin and consequently very sensible. It has its origin from the optic nerve and is itself wholly nervous and it is the common opinion that the nerves are the vehicles of all sensations. Lastly it communicates with the substance of the brain where all sensations terminate. As to the choroides its use was supposed to be to stop the rays which the extreme tenuity of the retina should let pass; and to do the same office to the retina which the quicksilver does for a looking glass; especially in animals wherein it is black. But from an experiment of a cat plunged in water M. Mèry conceived a different opinion, he observed the retina to disappear absolutely on that occasion as well as all the other humours of the eye while the choroides still appeared distinctly and even with all the lively colours it has in that animal, hence he concluded that the retina was as transparent as the humours but the choroides opaque: consequently the retina was not a proper instrument to terminate and stop the cones of rays or to receive the images of objects: but that the light must pass through it and could only be stopped

> by the choroides; which therefore would become the principal organ of vision."

Further analogy and physiological evidence follow, which could lead to the conclusion that Chambers favoured the choroid as the seat of vision. A few pages later, however, he again put forward the case of the retina.

> "The images of objects then are represented on the retina; which is only an expansion of the fine capillamenta of the optic nerve and from which the optic nerve is continued into the brain. Now any motion or vibrations on one extreme of the nerve will be propagated to the other: hence the impulse of several rays sent from the several points of the object will be propagated as they are on the retina (i.e. in their proper colours etc. or in particular vibrations or manner of pressure corresponding thereto) to the place where those capillamenta are interwoven into the substance of the brain. And thus is vision brought to the common case of sensation."

Accommodation was dealt with in a slightly more dogmatic manner. Three possibilities were considered, and all used the action of the muscles external to the eye. The first was that these muscles changed the shape of the eye, lengthening it for close objects and shortening it for distant objects: the second was that the change in shape of the eye-ball made the crystalline lens more or less convex: and the third was again that the change in shape of the eye produced by the exterior muscles varied the distance between the retina and the crystalline lens.

> "But nature has provided against it (the blurring of the image) either by contriving the eye so as its bulb may be lengthened or shortened as objects may be more or less distant; or, as others will have it, so as the crystalline may be made more convex or more

> flat; or according to others so as the distance between the crystalline and the retina may be lengthened or shortened. The first expedient is the most probable; on the footing of which, when we direct our eyes to an object so remote as that it cannot be distinctly viewed by the eye in its accustomed figure, the eye is drawn back into a flatter figure by a contraction of four muscles; by which means the retina becoming nearer the crystalline humour receives the rays sooner: and when we view an object too near the eye being compressed by two oblique muscles is rendered more globular; by which means the retina being set further off from the crystalline does not receive the rays from any point before they meet."

It is perhaps surprising that the extremely common theory that either the shape or position of the crystalline was changed by the interior ciliary processes or muscles, was not mentioned.

Two other points of note were mentioned. The first dealt with the distinctness of vision, and showed that the author of the encyclopaedia understood the concept of nerve endings well. The second was the production of an erect image from the inverted image produced on the back of the eye. On the subject of distinct vision Chambers said:

> "The distinctness of vision is somewhat concerned in the size of the image exhibited in the fund of the eye - for there should be at least as many extremes of capillaments or fibres of the optic nerve in the space that image possesses as there are particles in the object that sends the rays into the pupil: otherwise every particle will not move its separate capillament: and if the rays from two points fall on the same capillament, it will be the same as if only one point had fell there; since the same capillament cannot be differently moved in the same time. And hence it is that the images of very remote objects being very small may appear confused, several points of the image affecting each capillament: and hence also if the object be

> of different colours, several particles affecting the same capillament at the same time and only the brightest and most lucid will be perceived:"

On the subject of the impression of an erect object being produced from an inverted image on the retina, Chambers was, if anything, less convincing than Harris had been in the Lexicon Technicum. He quoted only Molyneux (1656 - 1698) in "Dioptrica Nova", as follows:

> "But Mr. Molyneux gives another account: the eye he observes is only the organ or instrument: it is the soul that sees. To enquire then how the soul sees the object erect by an inverted image is to enquire into the soul's faculties. Again imagine that the eye receives an impulse in its lower part by a ray from the upper part of an object; must not the visive faculty be therefore directed to consider this stroke as coming from the top rather than the bottom of the object, and consequently be determined to conclude that the representation of the top?"

ENCYCLOPAEDIA BRITANNICA (Third Edition) 1797

This encyclopaedia appears almost modern when it is compared with the two already mentioned. As has already been stated, it had eighteen volumes and was able therefore to give a far more comprehensive account of the functioning of the eye than the earlier encyclopaedias. It was also completely paged and had marginal summaries of the text.

The subject of the eye was dealt with under two main headings. In the section on Anatomy there was a detailed physiological description of the structure of the eye; and in the section on Optics there was a discussion of the way in which the eye was thought to function. It is this latter section which will be dealt with here, since, not surprisingly, the anatomy of the eye was well understood by this time and its description contained nothing of a controversial nature.

In the section on Optics there was an extensive article on vision (P. 292 - 302). This was mainly concerned with an historical survey of the arguments put forward during the previous hundred years on the seat of vision, but it also contained a brief summary of the structure of the eye, and a good explanation of the way in which we see an erect object even though the image on the retina is inverted.[1]

> "Since the image is inverted, many have wondered why the object appears upright. But we are to consider, 1. That inverted is only a relative term; and 2. That there is a very great difference between the real object and the means or image by which we perceive it. When all the parts of a distant prospect are painted upon the retina, they are all right with respect to one another, as well as the parts of the prospect itself; and we can only judge of an object's being inverted, when it is turned reverse to its natural position with respect to other objects which we see and compare it with. If we lay hold of an upright stick in the dark, we can tell which is the upper or lower part of it, by moving our hand downward or upward; and know very well that we cannot feel the upper end by moving our hand downward. Just so we find by experience, that upon directing our eyes towards a tall object, we cannot see its top by turning our eyes downward, nor its foot by turning our eyes upward; but must trace the object the same way by the eye to see it from head to foot, as we do by hand to feel it; and as the judgement is informed by the motion of the hand in one case, so it is also by the motion of the eye in the other."

The historical survey of the theories on the nature of the seat of vision started with an account of Mariotte's discovery of the blind spot, and his consequent theory of the choroides being the light-sensitive surface within the eye. The remainder of the article was an excellent summary of the subsequent development of this theory, and the swings of opinion to and from the retina or the choroides as the seat of vision. In conclusion, the encyclopædia came to a very strange opinion. It found difficulty in deciding decisively between the retina and choroid as the seat of vision, although there was an undoubted trend towards the retina apparent in the writings of the majority of the eighteenth century scientists discussed by the encyclopædia. Its opinion was that the retina was the light-sensitive surface, but that its transparency might make it necessary to postulate some role for the choroid also. The final conclusion was to put forward a theory similar to that proposed about one hundred years earlier by De la Hire: that the light was reflected back from the choroid to the retina.[2]

> "We shall conclude these remarks with observing, that if the retina be as transparent as it is generally represented to be, so that the termination of the pencils must necessarily be either upon the choroides, or some other opaque substance interposed between it and the retina, the action and reaction occasioned by the rays of light being at the common surface of this body and the retina, both these mediums (supposing them to be equally sensible to the impression of light) may be equally affected; but the retina, being naturally much more sensible to this kind of impression, may be the only instrument by which the sensation is conveyed to the brain, though the choroides, or the black substance with which

> it is sometimes lined, may also be absolutely necessary for the purpose of vision. Indeed when the reflection of the light is made at the common boundary of any two mediums, it is with no propriety that this effect is ascribed to one of them rather than the other; and the strongest reflections are often made back into the densest mediums, when they have been contiguous to the rarest, or even to a vacuum. This is not far from the hypothesis of M. De la Hire, and will completely account for the entire defect of vision at the insertion of the optic nerve."

The Encyclopædia Britannica was able to give far more space to the discussion and explanation of topicsthan either Harris or Chambers. Therefore there are lengthy discussions on such topics as the way in which we judge the distance of objects and the method by which we see an object singly rather than double, even though it is viewed through two eyes. Strangely, however, the topic of accommodation was, by comparison, neglected. The explanation of the method by which the eye focuses is given below in its entirety.[3]

> "That the rays may be collected into points exactly upon the retina, that is, that objects may appear distinct, whether they be nearer or farther off, i.e. whether the rays proceeding diverge more or less, we have a power of contracting or relaxing the ligamenta ciliaria, and thereby altering the form of the crystalline humour, and with it the focal distance of the rays. Thus when the object we view is far off, and the rays fall upon the pupil with a very small degree of divergency, we contract the ligamenta ciliaria, which being concave towards the vitreous humour, do thereby compress it more than otherwise they would do; by this means it is made to press harder upon the backside of the crystalline humour, which is thereby rendered flatter; and thus

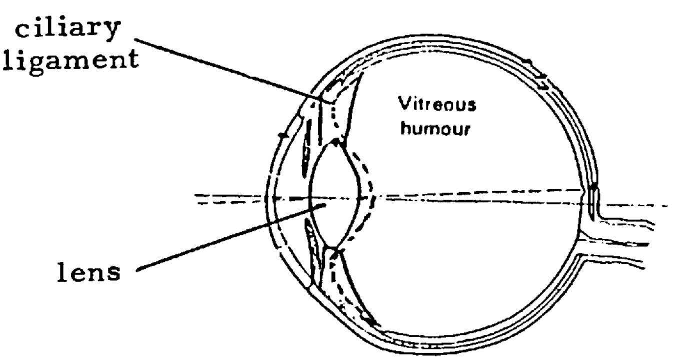
the rays proceed farther before they meet in a focus, than otherwise they would have done. Add to this, that we dilate the pupils of our eyes (unless in cases where the light is so strong that it offends the eye), and thereby admit rays into them that are more diverging than those which would otherwise enter. And, when the rays come from an object that is very near, and therefore diverge too much to be collected into their respective foci upon the retina, by relaxing the ligamentum ciliaria, we give the crystalline a more convex form, by which means the rays are made to suffer a proportionately greater degree of refraction in passing through it. Some philosophers are of the opinion that we do this by a power of altering the form of the eye; and others, by removing the crystalline forwards or backwards as occasion requires: But neither of these opinions is probable; for the coats of the eye are too hard, in some animals, for the first; and, as to moving the crystalline out of its place, the cavities of the eye seem to be too well filled with other humours to admit of such removal.

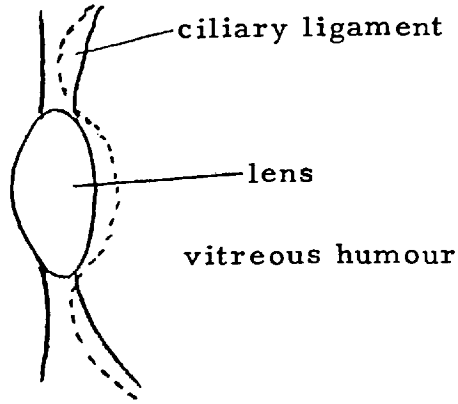
Besides this, in the case above-mentioned, by contracting the pupils of our eyes, we exclude the more diverging rays, and admit only such as are more easily refracted into their respective foci (C). But vision is not distinct at all distances, for our power of contracting and relaxing the ligamentum ciliaria is also circumscribed within certain limits.

(Footnote C. Accordingly it is observed, that if we make a small hole with a point of a needle through a piece of paper, and apply that hole close to the eye, making use of it, as it were, instead of a pupil, we shall be able to see an object distinctly through it, though the object be placed within half an inch of the eye.) "

It is clear from the excellent historical accounts that the author gave of other aspects of vision, that he had a very good knowledge of the development of the subject. Therefore this brief and inadequate account of the cause of accommodation is most surprising for two reasons. The first is that it lacked the historical perspective given to other topics. The second reason for surprise is that the topic of accommodation, which had attracted widespread attention from earlier researchers should be given far less prominence than other topics which were generally accepted to be of less importance, such as the least angle of vision and the appearance of objects when viewed through media of different forms. The topic of accommodation was again neglected in the section of the Encyclopædia devoted to anatomy.[4] In the section explaining the formation of a clear image on the retina, the defects of long- and short-sight were mentioned, but no reference was made to the ability of the eye to focus both distant and close objects clearly on the retina.

Attention should also perhaps be drawn to the rather difficult language in which this account of accommodation was couched. The method by which accommodation was said to be achieved was identical to that proposed by Gravesande in 1721, although no reference was made to this fact. The method can probably be best understood by reference to the diagrams below.





The dotted lines indicate the position of the ciliary muscles and lens when a close object is being viewed; the continuous lines indicate their position when a distant object is being viewed.

Thus, rather than proposing that the ciliary ligaments exert a direct pull on the crystalline lens, the author proposed that when they contracted they became less convex, compressed the vitreous humour, and that this pressed on the rear of the lens rendering it flatter. Other possible methods of accommodation were mentioned, such as the change in shape of the eye, and the moving of the crystalline towards and away from the retina; but these were dismissed summarily in a way that did scant justice to the calibre of the scientists who had in the past advocated these solutions to the problem of accommodation.

From this brief summary it can be seen that the educated layman in the eighteenth century was well served by the new encyclopædias. On the subject of vision the early encyclopædias gave a comprehensive and reasonably well-balanced account of current theories, based upon an excellent foundation of historical perspective. It is probable that the rapid expansion of the Encyclopædia Britannica, from its initial two volumes to eighteen in its third edition, reflected the popularity which surrounded this new form of publication. Therefore, it is a pity that the additional space made available to the author should have been used in the section on vision to give a rather more idiosyncratic account of the subject than that found in the earlier encyclopædias. Nevertheless, the importance of the subject was well represented in the publication, although the balance of importance given to topics did not always correspond to historical or current opinion; and the explanation given on such an important topic as accommodation neglected a number of theories of equal merit to the one put forward.

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1. Enc. Brit., 1797, 13, 292
2. Ibid., 297.
3. Ibid, 297.
4. Ibid. I, 768

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