MODERN VIEWS ON THE PHYSIOLOGY AND
PATHOLOGY OF ACCOMMODATION.*

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In bringing before you some modern views on accommodation I hope to be able to satisfy both those who are interested in theoretical investigations and those who prefer the discussion of more practical questions. Few problems stand in such an intimate connection both with theoretical and with practical ophthalmology, and in the last twenty years there has been much discussion on points of great importance. It is on these points that I wish to speak.

Since Thomas Young, more than a hundred years ago, first proved that accommodation must be caused by changes in the shape of the lens, and since Cramer and Helmholtz have shown us that these changes may be seen and measured by studying the Purkinje reflex images, two theories have been put forward to explain how these changes in the size of the lens might be brought about. Helmholtz believed that contraction of the ciliary muscle produces a slackening of the zonula, at least a transitory one, and that in consequence of this slackening the surfaces of the lens, especially the anterior one, become more convex. On the other hand, it has been repeatedly asserted that this increase of convexity is produced, not by slackening, but by tightening of the zonula. This theory, first put forward in 1858 and 1885 by Mannhardt and by Schön, has been supported in the last years by the careful experiments of Tscherning.

We shall see that questions which at the first glance seem to be merely practical, as, for instance, the treatment of presbyopia, of myopia, of glaucoma, etc., must be greatly influenced by the views which we hold on the mechanism of accommodation. I have, therefore, been endeavoring for a long time to invent new methods and discover facts that might enable us to come to a decision between the two hypotheses. First, I showed by experiment that when the ciliary muscle is contracted to the maximum in the normal eye the lens is displaced downward by gravity for about 0.5 mm., and whenever the eye makes the slightest movement the lens is seen to be tremulous behind the iris. It is not very difficult to see these phenomena in eyes affected with cataract. I was able to show them to some of our American colleagues who attended my clinic.

The best method of demonstration is, first, to dilate the pupil with homatropin and then to instill one or two drops of eserine. I found that if this is done the eserine acts much more quickly on the ciliary muscle than on the sphincter pupillæ, and in consequence the complete slackening of the suspensory ligament can be studied easily while the pupil is still dilated.

It is important to show that such a relaxation is to be found not only in old people with cataractous eyes, but equally in young eyes; the experiment can be made with equal success in children with lamellar cataract.

I propose this method because in cataractous eyes it is particularly easy to demonstrate the phenomena, but with a little experience it is easy to see the tremulous lens in completely healthy eyes; for this it is best to study the reflex images (Purkinje's images) of the lens. It can easily be seen that by any movement of the eye that these images are tremulous as long as the eye is ac-

commodated, and they can be seen displaced downward by gravity.

These methods are especially suitable for demonstration. A more precise method, and one which allows an exact measurement of the amount of displacement is the entoptic: If a diaphragm with a little orifice is brought about 12 mm. before the cornea the pupil is seen entoptically as a bright circle of diffusion and in it the little normal spots and opacities of the lens. If an effort of accommodation is made there is seen, first, the pupil getting smaller and the spots remaining in place, but if the contraction of the ciliary muscle increases suddenly there is seen the little spots displaced upward and returning downward with the relaxing of accommodation. This displacement is equally directed upward if the head is bent downward on to the right or the left shoulder. But no displacement is seen if the head is bent forward so that the iris lies in a horizontal plane.

Most interesting results are to be obtained if there is instilled a larger quantity of eserine so that the ciliary muscle is contracted to the maximum. Now, no displacement can be seen of the lens by *voluntary* accommodation efforts, but if the head is bent from the right to the left shoulder the lens is seen much more displaced than formerly with voluntary accommodation. I found the amount of displacement to be, under these conditions, almost one millimeter.

What may we conclude from these experiments? They irrefutably demonstrate for the first time the accommodative slackening of the zonula. Tscherning believed these facts to be incompatible with his tightening theory; he supposed that not the whole lens was displaced downward, but only the intralenticular substance, the capsule of the lens being slackened by tightening of the zonula. But I was able to refute this hypothesis by the following experiments: First, there can be seen in eyes with capsular opacities that the latter are displaced downward and made tremulous by accommodation. Second, I was able to show that, even in completely normal eyes, the anterior epithelium can, by special arrangements, be seen as a fine shagreen, and this film also is displaced downward with accommodation. Further, it is anatomically impossible that the lens fibers should be displaced without their capsule.

Recently the opinion has been put forward by Grossmann and by von Pflug that in accommodation the spherical posterior part of the lens is transformed into a posterior lenticonus. These results were obtained by the freezing method; eyes treated with eserine or nicotine and others treated with atropine were frozen by liquid carbon dioxide. When I read these papers it seemed to me very unlikely that these results could be correct; one of my assistants, Dr. Fischer, repeated the experiments of von Pflug on pigeons, with liquefied air, and he found that the freezing method is not at all suitable for the purpose. From the form of the frozen lens we can not draw any conclusion as to the shape of the living one. Therefore, I am of the opinion that the freezing method is quite valueless for the elucidation of the accommodation question.

I have repeatedly heard from one of our most distinguished American colleagues that of all Tscherning's experiments the most important, and the one which seems directly to contradict my views, is the following: Tscherning has shown that in the eyes of cows tightening of the zonula produces an increase of convexity. Against this view three things are to be said. First, the lens of

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the cow is very different from human lens and the accommodative power of these animals is relatively extremely small. Second, if by tightening of the zonula in cows' eyes the lens convexity can be increased, it still would not hold good for the human eye. Third—and this is the most important point—I was able to show in the eyes of new-born children, and of monkeys in which I had carefully removed the cornea and iris, that the images of the anterior lens surface increased by tightening of the zonula and diminished by slackening. So my results in human lenses are here in direct contradiction to those of Tscherning in cows' lenses.

My experiments show irrefutably that increase of lens convexity is caused by relaxation of the zonula, and—what is still more important—that even in children's eyes the ciliary muscle can be contracted much more than is necessary for the maximum convexity of the lens, that is, for accommodation to the punctum proximum; but we shall return to this question.

First, however, you will doubtless wish to hear something about the changes in the topography of the ciliary muscle and the ciliary body during accommodation. Here also there is considerable conflict of views.

Schön and Tscherning suppose that by contraction of the ciliary muscle the ciliary body is displaced backward toward the center of the vitreous, in this way increasing the tension of the zonula. But it is not difficult to show, with simple methods, that such a supposition is erroneous. In the case of patients with iridectomy the ciliary processes will often be found behind the coloboma near the equator of the lens. Instil eserine and after 10-15 minutes the processes can be seen advancing toward the cornea and lying quite clearly before the equator of the lens close to its anterior surface. This may be seen very readily in a case of lamellar cataract with iridectomy.

The following fact is equally interesting: I have often seen little tent-like elevations in the atropined eye near the equator of the lens at its anterior surface; if sufficient magnifying power is used it can be seen that these elevations are caused by the traction of zonular fibers at the lens surface. After instillation of eserine these elevations disappear more or less completely and the equator of the lens is seen to have taken a much more regular form. So there can be seen directly traction of the zonula in the non-accommodating eye and its slackening by contraction of the muscle.

Let us now look at some of the consequences which these facts must have on our views of accommodation. It had been generally admitted, even by the upholders of the slackening theory, that this slackening was suddenly compensated by an increased convexity of the lens, and Helmholtz himself believed that lens, zonula and chorioid form a completely closed capsule, entirely filled by the vitreous, and that the pressure of the liquid caused the tension of these parts.

We see from my experiments that this part of the Helmholtz theory must be abandoned. Indeed, we find complete slackening of the zonula and tremulous lens in the perfectly healthy eye. A normal amount of accommodation is possible in eyes in which the pressure of vitreous is eliminated by a hole in the chorioid and sclerotic.

A second fact shown by our experiments is this, that intraocular pressure must always be the same in the anterior and the posterior part of the normal eye; many authors believed, and still believe to-day, that pressure increases during accommodation in the posterior part

of the eye and diminishes in the anterior part. This view can no longer be held after we have found accommodative slackening of the zonula and a tremulous lens.

And now we come to a point of great importance in the question of the causes and treatment of myopia. We all agree, I think, as to the ill effect of too close and sustained use of the eyes on the progress of myopia, and that this ill effect is caused by increased intraocular pressure. But when we come to consider the causes of this increased pressure we at once meet with a great discrepancy of views. Many authorities believe that this increased pressure is caused by accommodation. It was, therefore, necessary to find out whether such an accommodative increase of pressure actually takes place. Sufficiently exact measuring of the intraocular pressure in the vitreous with the manometer is scarcely possible without severe lesions of the eye, but measurement of the pressure in the anterior chamber is relatively easy. And as we now know by my experiments that here the pressure is always equal to that in the vitreous, it is possible to study experimentally the influence of accommodation on the intraocular pressure. I have made a number of such experiments (together with Dr. Heine), and I was able to show that even the maximum accommodation by electric stimulation of the ciliary ganglion or the ciliary muscle has not the slightest effect on intraocular pressure. Our apparatus was so susceptible that it showed the changes of intraocular pressure produced by the pulsation of the blood vessels most exactly.

These experiments must be made on eyes whose accommodative power approaches that of human eyes. It was, therefore, necessary to study the amount of accommodation in animals. We found that almost all the mammals on which we experimented accommodate only in a small degree (1 to 4 diopters), monkeys alone showing an accommodative power approaching that of human.

As to cats, Grossmann has lately arrived at other results than ours; he has found a very considerable accommodation, and believes this to be produced by an accommodative posterior lenticonus. Grossmann's experiments have been repeated in my laboratory, but we could not confirm them. We found in several young cats only a very small range of accommodation, exactly as we had found it before.

In birds we found the amount of accommodation very high (12 to 14 diopters and more), and the mechanism is here approximately the same as in the human eye. In none of these animals could we find any accommodative increase of intraocular pressure, while the least contraction of the external eye muscles increased this pressure considerably.

These experiments show that the supposed influence of accommodation on intraocular pressure must no longer be considered as a cause of the progression in myopia. The consequence is that there is no longer any reason to object to the prescription of fully correcting glasses; on the contrary, these glasses may have a good effect on the progress of myopia, inasmuch as the patient wearing such glasses is able to read and write at a considerable distance and without appreciable convergence. The ill effects of the latter is evident from our experiments.

I am glad to say that the results, to which I have been led entirely by physiologic experiment, are in full accordance with the views which are held by so many physicians of this country, and which are mostly based on practical experience. With us in Europe there are

still a good many ophthalmologists who are opposed to full correction of myopia, chiefly in consequence of erroneous views which they hold on accommodation.

I think the experiences I have reported are, in the struggle against myopia, of decisive importance; it can not be too firmly insisted on that up to the present time there has no fact come to light which could even make it probable that full correction could do the slightest damage to the myopic eye. But a long experience of thousands of cases has shown us that the wearing of weak or even of no glasses is powerless to protect the eye from progressive myopia and all its dangers.

Allow me to insist on another point of practical interest. If the views which uphold the theory of accommodative tightening of the zonula and increase of intra-ocular pressure by accommodation were correct, eyes disposed to glaucoma ought to avoid any accommodation, for this would be most dangerous for them. And, indeed, even the most distinguished ophthalmologists prescribe for such patients strong convex glasses in order to avoid, as much as possible, accommodation. Now, in studying these questions I have found no way of explaining the ill effect of accommodation on glaucoma, and I never have seen a case in which such an influence was even likely. And what is more, is it not strange that in glaucoma on the one hand, avoidance of accommodation is insisted on, while on the other hand eserine is continually prescribed, which produces a powerful spasm of accommodation? Since I pointed out this striking contradiction careful observers have remarked that, in eyes with incipient glaucoma, strong accommodation is able to interrupt the glaucomatous attack or, at least, give a decided relief. I think that the views set forth here may be valuable in prophylaxis and therapy of incipient glaucoma.

Another important question is the following: It has been generally supposed that the more the contraction of the ciliary muscle increases, the nearer the punctum proximum approaches the eye. The maximum of lens convexity has been supposed to occur only as the result of the maximum contraction of the ciliary muscle.

This is evidently not correct. When the muscle contracts the lens changes its shape to a certain degree only, a degree which depends on the patient's age. I call this point the "true" punctum proximum, and the corresponding range of accommodation the manifest range. When the lens has attained the maximum convexity possible at a certain age, and if then the muscle contracts still more the lens may be displaced downward by gravity, but does not further change its shape. The true punctum proximum remains at the same point, but with the increased contraction of the muscle there is associated a contraction of the pupil which allows us, if we use the usual methods of determination of the punctum proximum, to bring a little nearer to the eye the object which is to be seen distinctly. So we find another proximum, which has been generally believed to be a consequence of increased lens curvature, but I have been able to show that the convexity is the same in both cases. Let us call this second point the "apparent near point."

In the same way the apparent proximum is brought nearer the eye by eserine, but the lens convexity remains the same as in the case of simple accommodation. Any patient can contract his ciliary muscle much more than is necessary for the maximum convexity of lens. Let us call this the "latent range of accommodation."

Now, it can easily be seen that the older the patient is the more his latent range of accommodation increases,

and at the age of 65 to 70 years the whole muscle contraction is latent, but the muscle may be as strong and energetic as in youth. Here again we are in contradiction with generally held views. Often has it been asserted that in old people the muscle might become atrophied by inactivity. On the other hand, it has been supposed that in the case of old patients a greater contraction of the muscle would have to take place in order to increase the lens convexity than would be required to produce the same effect in youth. Both views are erroneous.

The same contraction of the muscle may, at different periods of life, give exactly the same increase of lens convexity, and the association between convergence and accommodation being a congenital one and remaining practically unaltered during the whole life, the muscle does not become inactive and can, therefore, not become atrophied by inactivity.

Some practical consequences of this are the following: In a young patient of from 10 to 15 years a slight paresis of the ciliary muscle may be detected by the displacement of the punctum proximum to a greater distance, but a much higher degree of paresis of the muscle is not to be discovered in a patient of from 40 to 50 years if the paresis is smaller than the latent range of accommodation.

According to the views generally held, especially those of Schön and Tscherning, any paresis of the ciliary muscle should produce a displacement of proximum to a greater distance, and, indeed, one sometimes reads in scientific papers that patients of 40 or 50 years of age are supposed to have normal accommodation and a normal ciliary muscle when their proximum is found at the normal distance. Nevertheless, such patients may have a high degree of paresis. We may say that a paresis of the ciliary muscle is not to be found by objective methods before the paresis has become larger than the latent range of accommodation.

This law is important, for instance, if we want to study the physiologic action of drugs on the ciliary muscle. For example, different opinions have been put forward as to the influence of cocaine on the ciliary muscle; the fact is that at the age of from 35 to 40 years we find little or no effect on the true proximum, but its removal from the eye is evident in younger men, and the younger they are the more evident it is and the more quickly it occurs. So, studying to-day on my own eye, by determination of the proximum, the effect of cocaine, I find the drug without influence on its position, but studying its effect by the entoptic method which I have described above I find that after cocaine there is no displacement of the lens by gravity, even in maximum accommodative effort; this shows that cocaine acts on my ciliary muscle. We have then in these entoptic methods a most delicate means for studying the action of drugs on the ciliary muscle. But this method, it is evident, is only to be employed by experienced investigators and can not be used on the average patient. It is, therefore, important to know that up to date we have no means at all of finding out whether the ciliary muscle of a patient of 60 years is normal or completely paralyzed.

Let me now say a few words on the so-called "spasm of accommodation." For a long time such spasms were believed to be very common among children and to have a certain importance in the development of myopia. In Germany some authors have written that in schools 10 per cent. to 20 per cent. of the children show this spasm, and these men have believed it could pass into

true myopia. It is difficult to see how this could be possible, since we know that such a spasm, if it exists, can have no influence on intraocular pressure.

I have studied many thousand children carefully, but never have found a case of spasm of accommodation. As a rule the diagnosis of spasm is made when children require a stronger glass for seeing at a distance than that which has been determined by the ophthalmoscope. But this can not prove the presence of a spasm of accommodation. The ophthalmoscopic or even the retinoscopic determination of refraction can never, with perfect exactness, give us the true refraction at the fovea of an eye. This determination of refraction is still sometimes made on eyes with dilated pupils. But here the optic conditions are so different from the normal that it is impossible to come to a conclusion as to the normal refraction of a patient's eye. The refraction of the peripheral parts of cornea and lens may differ from the central ones, the degree of astigmatism may be another one, etc.

I have often found, and other physicians have stated similar facts, that children by the first examination with glasses seemed to be myopic, requiring glasses of from 2 to 3 diopters. But further examination shows that an equal power of vision resulted from the use of plain glasses, sometimes the frame alone, without glasses being sufficient to cause the myopia to disappear. I think it would be a mistake to call such cases spasm of accommodation. Some authorities believe this to be hysterical, and if we find such a spasm in patients with other characteristic symptoms of hysteria we may allow ourselves to call the spasm "hysterical," but if the patient seems completely healthy—and this is the rule—we ought not to call this phenomenon a hysterical one. I should most like to compare such a phenomenon with other more common ones in children, which are either simple inability or so-called "associated movements." This inability to see at a distance would then be of the same order as the movements of the tongue or faulty movements of the fingers which we often see in children when they begin to write.

The view we take of this question is important for the prescription of glasses: As soon as oculists generally admit that there is no accommodative spasm in beginning myopia we may hope no longer to meet with instillations of atropin continued for weeks or months as a treatment of incipient myopia, as still sometimes happens to-day.

Often we find our patients afraid to wear convex glasses for presbyopia, believing it necessary to exercise the eye. Such an opinion might be reasonable if the views of Tscherning and of Schön were right, but they are incompatible with what we know by our experiments about accommodation. No amount of exercise can have any effect on the maximum of lens convexity.

We still sometimes meet with the opinion that the range of accommodation in hypermetropic eyes is larger than in emmetropic ones and in myopic eyes smaller, because in the hypermetropic eye the muscle contracts more, is more "in training" than in the myopic one, and that people who read much have for the same reason a larger range of accommodation than country people. Such views also are erroneous. If they were correct it would have to be supposed that the lens in hypermetropic eyes and in the eyes of people who read much is smoother than the lens in myopic eyes and in those of country people. In fact, the range of normal accommodation is independent both of refraction and occupa-

tion and only influenced by the age of the patient. This fact is often forgotten in prescription of glasses for myopia.

I think that these statements are of importance in discussing the question of the so-called "eye-strain." An "eye-strain" caused by the ciliary muscle can occur in hypermetropic patients, but not in old emmetropic eyes; we must not forget that increased contraction of the muscle in emmetropic persons of from 40 to 50 years is of no use for reading and that, therefore, they do not contract their ciliary muscle so as to have any trouble by it.

Up to the present time, it has been generally admitted that, in youth, the index of the lens cortex is much smaller than the index of the nucleus and that this index increases slowly and continually from cortex to nucleus, so that the index curve might be represented by a parabola (Matthiessen). I believe this opinion to be incorrect. The increase of index from cortex to nucleus is not a continuous one, but there is from the age of 20 to 25 years always a most marked and sudden increase at the limit of the nucleus, and the difference between the index of cortex and nucleus increases with the age of the patient. These facts I was able to prove, having discovered that we find in the lens not only the well-known cortex or Purkinje reflex images, but from the age of about 25 years always a further two, which may be called the "nucleus images." The luminosity of these images increases with increasing age; often in absolutely normal lenses they are at the age of 40 or 50 as bright as the Purkinje images.

This statement shows that refraction of light in the human lens is still much more complicated than had up to the present been supposed. In fact, we have in the lens not two refracting surfaces, but four, and the influence of the nucleus surface increases continually with the age.

Another interesting consequence of this fact is the following: Donders supposed that senile hypermetropia might be explained by increase of the cortex index of the lens in advanced age, while he believed the nucleus index to increase much less, so that the lens became more homogeneous. Our experiments show that this theory can not be sustained, that, on the contrary, the difference between cortex and nucleus index is greater in age than in youth. I believe that the convexity of the lens surface diminishes in the old, and that this is the cause of senile hypermetropia. Further investigations are necessary for clearing up these questions.

Another interesting question is whether the range of accommodation is always the same in both eyes or if we are able to correct anisometropia by accommodating with one eye more than with the other. As you know, a lot of experiments have been published to show that such an unequal accommodation is possible. Evidently it would suit us very well if we could do so, and in prescribing glasses for anisometropia it is very important to know whether we can accommodate unequally or not. I have made many investigations on the subject, partly with special apparatus which allows a most exact determination. But never, either in artificial or in natural anisometropia, was I able to find the slightest difference in the range of accommodation of the two eyes. Even in a completely blind eye accommodation is the same as in the normal eye (of the same person), as Zimmermann has shown.

The following question has, perhaps, a still more practical importance: Is it possible, by contracting only

certain parts of the ciliary muscle, to produce an astigmatism of the lens so as to eliminate by this means an opposite astigmatism of the cornea? You know that a great many oculists believe in such a possibility and suppose that this "astigmatic accommodation" may be the cause of different diseases of the eye, as blepharitis, glaucoma, cataract and others. The fact that such views are held necessitates a most careful investigation of the question. This I have made by several different methods and have never found a single fact which would make such an astigmatic accommodation probable. None of the astigmatic patients whom I have studied showed the least sign of partial contraction of their ciliary muscle.

Most observers who have studied the question suppose that astigmatic patients always accommodate in such a manner as to bring the object into the anterior or the posterior focal line, and this should be the case even when the object appears here less distinctly than it would if the eye accommodated on another point of the interfocal space. Now, I have tried to find out at which point of this space the object is seen most distinctly. I took photographs of different print types with an astigmatic apparatus. I found that the photographs were least indistinct about the middle between the anterior and posterior focal lines.

Although the conditions in the human astigmatic eye are more complicated than in the apparatus, we may, nevertheless, say that most probably the astigmatic eye, as a rule, sees best when it accommodates in such a way that not a so-called focal line, but a spot which is approximately in the middle between both focal lines, is brought on to the retina. In any case I do not know of any observation that would make the possibility of astigmatic accommodation for correcting corneal astigmatism probable. So when we prescribe glasses we no longer need to regard this hypothesis.

It is needless to say that I speak here only of voluntary partial contraction of the muscle; whether there exists an involuntary partial spasm of the muscle is not as yet known. If you instil a drop of atropin or eserine so as to bring it only to the inferior part of the cornea the drug by diffusion comes more quickly to the inferior half of the muscle than to the superior half, and this may produce for a few minutes an astigmatism of the lens, but such a fact has no clinical interest at all for our question.

The new facts in the accommodation which we have pointed out here bear on the question of relative accommodation and the association between accommodation and convergence. These are rather complicated, and it would be rather tedious for you were I to discuss here the whole subject. Let me give you a short outline of the plan which I have followed in these investigations.

Since the days of Donders it has generally been admitted that the so-called curves of relative accommodation are in all their points determined by physiologic relations between the range of convergence and the range of ciliary muscle contraction. But this is not possible, since these curves are produced in their different parts by completely different factors. I was able to show by measurement that both the relative proximums and the relative remotums lie in the manifest accommodation space on two straight lines, which are parallel to the so-called convergence lines, and that they are limited by a horizontal line, which we may call the "near point abscissa."

It had been supposed that accommodation to the

punctum proximum necessitates always a maximum contraction of the ciliary muscle. If this were true it would be necessary that the relations between convergence and accommodation should change continually with advancing age. And we should up to the most advanced age be compelled to associate an always increasing innervation of the ciliary muscle with the same convergence innervation. But at the age of from 60 to 70 we should be obliged either to contract our ciliary muscle *ad maximum* with the slightest convergence and without any practical effect, or we must suppose that, late in life, we should again have to learn to converge without any or with very small ciliary muscle contractions. Both suppositions are most improbable.

According to these views, which were partly those of Donders, in the years of incipient presbyopia we ought to behave like hypermetropic patients, we ought to have accommodative asthenopia and convergent strabismus. According to my views, the relations between accommodation and convergence remain the same during life; the diagrams I find for relative accommodation are quite different from those which have been generally adopted, but the diagrams of Donders may be easily explained by the conditions under which he made his measurement.

I will now leave these rather complicated questions and try to review the most interesting facts with regard to comparative physiology of accommodation. In the mammalian eye we find everywhere the same mechanism: slackening of the zonula by contraction of the muscle. As to the range of accommodation, we have seen that only in monkeys it is similar to that of man; all other mammals which we investigated had only a very small range of accommodation even in youth, and we find this not only in our domesticated animals, but equally in wolves, for instance.

It is a most interesting fact that Rabl has recently come to corresponding results by means of anatomic investigation. The disposition of the cortical lens fibers in man and monkeys is a rather irregular one, just as it is to be expected in a substance, which must be able to change its shape very quickly and easily. In all other mammalian lenses the radial lamellæ of the lens show a much more regular disposition of fibers, corresponding to the relatively slight accommodative changes in such lenses. This is a most remarkable agreement between the results of physiologic observation and of anatomic conditions.

In birds the accommodation is produced equally by contraction of the ciliary muscle and slackening of the zonula. The range of accommodation is here similar to that in man. Regarding the necessity of accommodation in different animals: As to birds, it is evident that a high range of accommodation is necessary for them, for the grains which form their food are very small and must be picked up with the beak. Grossmann, in his studies on cats' accommodation, said that cats must have a high range of accommodation, too, but I think this is not necessary; when the cat catches a mouse it does not require much accommodation in order to see its prey, nor is it necessary that it should be seen quite distinctly. Moreover, the cat catches the mouse with its claws; therefore, I do not think that our results as to the small amount of accommodation in cats are in contradiction with the biologic conditions.

Theoretically, accommodation seems possible in the following ways:

1. Change in the convexity of the cornea. It is still unknown whether, practically, such an accommodation

takes place or not; perhaps it exists in birds' eyes; at least we find here the most interesting fact, that by ciliary muscle contraction the inner lamellæ of the cornea are displaced toward the anterior lamellæ.

2. Change of position of the retina. Formerly, even for the human eye, this mechanism was admitted, and of late years some oculists have asserted that the so-called accommodation in aphakic eyes was to be explained by a slight displacement of the retina backwards. But this aphakic accommodation is, no doubt, only an apparent one; by careful observation we can show that the aphakic eye lacks any accommodative change. Whether in animals an accommodation is possible by change of the axis is not yet quite certain; it may perhaps be possible in cephalopods' eyes; here we find the sclerotic extremely thin and changing its position on the slightest pressure.

3. A third possibility of accommodation is the displacement of the lens forward or backward without or with change of its convexity. This interesting mechanism is to be found in fishes' eyes (Beer). The eyes of fishes show, when at rest, myopic refraction of from 5 to 6 diopters; they have a negative accommodation, that is, for the distance, and this is performed by approaching the lens to the retina; the lens itself does not change its shape.

This movement of the lens is produced by a band-like muscle, "campanula Halleri," which stretches from the inferior part of the lens to the retina, the upper part of the lens being fixed to the sclerotic. By contraction of the muscle the eye can become approximately emmetropic.

A most interesting peculiarity is this: The movement of the lens is not directed straight backward, but backward and a little to one side, so that the image of an object is displaced sideways from the original position on the retina.

The fish's eye is thus seen to be wonderfully adapted to its task; as a rule the fish has to see near objects and is only exceptionally called on to see to far distances; it never becomes presbyopic, and by the lateral movement of the lens the fish is enabled to follow an enemy's movement without moving the eye.

In cephalopods' eyes a similar mechanism takes place, yet here we have no campanula, but a ciliary body very similar to that of mammals; its contraction draws the lens backward, so that here also we have normally a negative accommodation at a distance.

The mechanism of lens displacement, without change of convexity, is also to be found among reptiles; some of them seem to have a very slight range of accommodation, others, as for instance, the turtle, a larger one. Here a most remarkable positive accommodation takes place by advancement of the lens, which is in part produced by an increase of pressure in the vitreous, so that the lens is pushed forward through the pupil; it is very easy to observe this wonderful phenomenon in enucleated turtle eyes.

The interesting details on the comparative physiology of accommodation are inexhaustible. I wanted to show the different ways by which Nature has realized the problem of adapting the eye to different distances. As to human accommodation, I hope I have shown that many important questions, which at the first glance seem to be merely practical, are in intimate connection with physiologic science and can be elucidated by physiologic experiment. Any progress in our practical work should be preceded by scientific investigation.

EXPERIMENTAL PATHOLOGY AND SURGICAL TREATMENT OF ERYSIPELAS.*

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Since the discovery of the staphylococcus as a cause of suppuration and the streptococcus as the cause of erysipelas these organisms were found in many other conditions, and hence have long since lost their position as the cause of suppuration and of erysipelas only.

The streptococcus is found in septicemia and as a complication of diphtheria and other affections; while the staphylococcus is found in furuncles, carbuncles and all abscesses of a subacute nature.

When a wound is infected in these days of antiseptic precautions, pus abounds at first with staphylococci alone. In pre-antiseptic days, when pus was considered an almost desirable condition, when wounds were uncleaned, an erythema corresponding to erysipelas soon developed, accompanied by the clinical manifestations characteristic of this disease. It was evident that what seemed to be an ordinary infection could, when neglected, transform itself gradually into a more marked and virulent form, to be followed by general septicemia. At present these complications are not allowed to manifest themselves owing to prompt antiseptic cleansing.

The phenomenon, however, was apparent that if wounds were not cleansed the infection, which seemed moderate at first, would become more and more aggravated, apparently erythematous erysipelas or violent phlegmonous erysipelas, resulting in gangrenous cellulitis or hospital gangrene. Especially was this the case if the infection took place in the superficial lymphatics where phagocytes are not so plentiful as in the deeper or cellular lymphatics, and where the opsonins could not so readily help in the process of destruction.

Clinical experience has proven this point. If, however, the infection be locked up in the subcutaneous tissues, then an abscess, which may be strictly circumscribed at first, becomes more and more virulent in appearance. These clinical phenomena led me to experiment with the staphylococcus and streptococcus under various conditions to see whether or not it was possible to alter the virulence of these apparently dissimilar organisms so as, in a measure, to explain the clinical phenomena.

The following experiments were carried out by me in the bacteriologic laboratory of the Medico-Chirurgical College. The streptococcus was isolated from a virulent case of septicemia, which proved fatal in eight days' illness. It gave the typical growth on gelatin and agar.

Tubes containing 10 c.c. of bouillon were inoculated with virulent streptococcus, one tube being kept neutral. To a series of eight were added a 1 per cent. solution of bicarbonate of soda in minims of 2, 4, 6, 8, 16, 32, 64 and 100 respectively; likewise to the same number of minims was added a solution of hydrochloric acid, 1 to 1,000. This whole series of acid, neutral and alkaline media were then placed in an incubator at 44 degrees C. The result of this experiment showed a plentiful growth of streptococci in the neutral tube. The streptococci also grew in the series of acid tubes seemingly well, but losing the chain-like arrangement and partaking more and more of the staphylococci or grape-like arrangement, until they seemed typically staphylococci in the tube containing 100 minims.

When a gelatin culture was made from the tube containing

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