Case C.—M. L., a woman, 36 years old, was admitted Aug. 26, 1900, lecturer. Purulent bronchitis with edema of lungs. Diaphragmatic hernia.

Kidneys encroach greatly on peritoneal cavity. Weight of right kidney, 1.160 grams; left, 780 grams. Right kidney, 27x 10.5x8 cm. Surface markedly irregular and distorted, due to multiple cysts, which have apparently, except as mentioned below, entirely replaced normal kidney substance. Cysts vary in size from 0.8 to 4 cm. Some are present for the most part, so that the cyst contents are visible in many as a straw-colored fluid. Other rounded masses scattered at irregular intervals over the surface and surrounded by cysts of a dark grayish-brown color and of considerable firmness, suggesting the remains of kidney substance. Left kidney 20.5x10.5x7.5 cm. On posterior surface a firm grayish mass, evidently kidney substance, measures 4x2.5 cm. No abdominal tumor or symptoms on account of kidneys were noted during life.

Urine, 1012. Albumin, very slight trace. Sediment very slight, consisting of pavement epithelium. Cells from neck of bladder, coccidin crystals, bacteria, a few normal blood globules and leukocytes.

Case D.—F. B., a man, 70 years old, was admitted to the hospital April 5, 1901. Death from double pneumonia. Cause, 220 grams; left, 220 grams. From anterior surface of right kidney projects a thin-walled cyst, 4 cm. in diameter. At lower pole of same kidney a similar cyst, 6 cm. in diameter. Left kidney presents a similar cyst and seven smaller cysts. Contain clear straw-colored fluid. Specific gravity, 1010. Slight fatty degeneration. Glomeruli stand out as bright red points. Microscopically the glomeruli are large and injected. Slight focal increase of connective tissue with atrophy of adjacent tubules. In places are dilated tubules (1), in which granular material. Certain tubules contain granular casts.

No symptoms due to cystic kidney.

Case E.—M. K., a woman was admitted to the hospital May 8, 1901. Ischiorectal abscess. Bronchopneumonia. Edema of lungs. Congenital cystic degeneration of right kidney, with compensatory hypertrophy of left. Age 60 years. Right kidney, weight 40 grams, measures 9x2.5x2.5 cm. Made up almost entirely of cysts from 1 to 2 cm. in diameter and filled with clear fluid. These are separated by only a thin wall of kidney substance. At the upper pole, kidney tissue reaches its maximum thickness, being there 1 cm. Hilum of kidney dilated, but ureter patent, showing no evidence of obstruction. Right renal cortex about one-third size of left. Left kidney, weight 235 grams, 14x6.5x4.5 cm. Smooth. Capsule strips easily. Pale on section. Glomeruli and markings of pyramids indistinct. Cortex 8 mm. thick. Many fat droplets in tubular epithelium.

No symptoms recorded in relation to kidney.

Urine, much pus. (Ischiorectal abscess.) The case is evidently one of unilateral cystic kidney, with compensatory hypertrophy of the other.

Several other cases of cystic kidney in the autopsy records are not included, as a careful study showed that the cysts were secondary to interstitial nephritis, and the above cases are the only ones of undoubted congenital cystic kidney taken from a very large number of autopsy records. It is interesting to note that in none of them was a diagnosis of the condition made during life.

Conclusions.

From the above discussion we may draw the following conclusions:
1. If there be sufficient healthy tissue between the cysts to perform the normal renal functions the disease may never attract attention, and the patient may live a normal life and die of disease independent of the kidneys.
2. Under the same conditions as to renal tissue, while the renal excretion is normal, the cysts may cause pain and discomfort from their size and weight without dan-

gerous symptoms for years. Mobility of the kidney, with its accompanying troubles, may result from this enlargement.

3. The growth and enlargement of the cysts may so strangle and compress the normal kidney tissue between them as to throw it out of commission, so that stasis and uremia may result.
4. The normal tissue between the cysts may become affected by the various forms of nephritis, just as healthy kidney may, and this nephritis may be fatal in itself or by reason of the cardiac and arterial changes which accompany it.

5. These same cardiac and renal changes may and do accompany the compression of the intervening tissue by the cysts, and are not an infrequent cause of death in these cases.

6. The surgical aspects of the disease relate to the pain and discomfort due to the size of the tumors or mobility resulting from them, and in operating for these conditions it is all important to remember the pathology of the disease and more especially the fact that it is bilateral in 98 per cent. of the cases. A cystic kidney, while causing pain and disability by its enlargement, may still be performing so large a part in urinary excretion that its removal would be dangerous to life from renal insufficiency.

7. It, therefore, becomes important if possible to take some means to diminish the size of the offending organ and anchor it in place without interfering with its secretory functions. Such a procedure, which will be vastly safer than nephrectomy, consists of the free incision and puncture of the larger cysts, thus diminishing notably the size of the kidney, and suturing of the organ in position. If for any reason a nephrectomy seems desirable it should not be performed unless an exploratory incision or ureteral catheterization has demonstrated that the other kidney is healthy.

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OPHTHALMIC PRACTICE—THE PRESENT TIME.*

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An extremely interesting paper by Dr. George M. Gould on "A Study of Failures in Ophthalmic Practice" has called my attention to the subject of the work of the ophthalmologist of the present, and we may well ask the questions: What is it? What shall it be? Dr. Gould's candid acknowledgment of his failures and the reasons therefor will be readily accepted by most ophthalmologists, who will agree with the premises so well set forth in his paper. But I do not think, however, that the final word has been said as to the causes of our failures, nor do I believe that the present method of examination can not be improved. If we do have failures, such conditions must always exist, more or less, with all medical men, be they specialists or general practitioners. No one is infallible, even the most expert may be misled or fail by reason of conditions which the physician can not control. This subject can not be considered from any one standpoint; the work of all the investigators now working along these special lines in our profession must be included.

We may object to the statement: "By failures to

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cure I mean the failure to relieve those symptoms due to eyestrain,” as we well know, and as Dr. Gould has shown, that we must not consider all “the ills that flesh is heir to” as due to eyestrain. I do consider, however, that in the present state of our knowledge of eyestrain, and the present advanced methods of examination of ocular conditions, our failures will compare very favorably with all other work in the medical profession. If so, then we may justly ask: What is or should be the essential practice of ophthalmology at the present time?

Before we take up the study of the advancement of ophthalmology during the past two decades, let us study some of the reasons for our failures as set forth in Dr. Gould’s paper. I note, first, the “want of a good refraction school.” In attempting to answer this I must start with the assertion that I must differ very positively with the statement that “ophthalmology is most inaccurate and unscientific,” because I believe fully in Helmholtz’s statement: “Ophthalmology is by far the most accurate and scientific of all the specialties of medicine,” and, furthermore, because we do agree in our examinations of the refraction and motility of the eye if these are properly and scientifically made. We can never agree on this subject if we do not study carefully the investigations of others who are equally as scientific as ourselves, but instead set up a little pedestal of our own and, standing on it, make the assertion that ours is the correct, while that of others is all wrong. The investigations of Stevens, Risley, Savage and others surely deserve some consideration even if we can not accept all their teachings. Some of us will be proficient with one method of examination, others will succeed in some other way, but the refraction of the human eye must be a fixed condition, which by proper methods of examination must show the same results to all observers who are sufficiently expert in the use of the objective tests that should be found in every well-equipped oculist’s office.

If this be true, we must have good refraction schools in this country, and I would venture the assertion that the refraction schools of this country are more fully advanced and more complete in their teachings than in any other country in the world. The reader of this article may ask: Where is this teaching that is so perfect? The answer is, in no one place nor is it given by any one teacher, but all combined furnish the result. We might say that Dr. Gould’s teaching was all that could be desired, but yet I fear that if his methods only—as I understand them—were practiced we should have many failures. What, then, shall the student of ophthalmology do, and where shall he go? I would answer that in this way: Let him listen to the teachings of all the eminent men in this country who are constantly placing their views on ophthalmology before their students and the medical profession; not in a narrow way, by following the teachings of any one man, but by the true and honest way, of carefully studying the methods of all and then combining them in the way that seems best to himself.

Now let us consider what seems to me is Dr. Gould’s teaching. “The day of the worship of the scissors or of the ophthalmometer is long in the past.” If this implies anything, it is that we must put aside the use of these two instruments as useless and obsolete. Certainly such an assertion would seem an obsession to many well-advanced oculists, and I venture to say that no complete examination of the eye should be made without the use of the ophthalmometer, notably so in the prognosis of myopia. I shall speak of the scissors later, but the doctor can not deny that some cases have failed with glasses and have been relieved with the aid of the scissors.

“Incorrect diagnosis of ametropia” is said to be another reason for our failures to relieve eyestrain. This can not be denied, but is it not a personal fault? Is it not one that must exist in the physician himself and for which he must take the consequences? This almost goes without saying, yet it is not the fault of our teachings; if the examiner of refraction will not devote the necessary time to following out a scientific examination of the refractive condition he must fail in many cases, and if he does give the time to his patients he should be sufficiently expert to notice and to correct every small error either astigmatic or muscular.

Is it an error to recognize and to correct the condition known as heterophoria? Dr. Gould evidently admits that this condition may be present and speaks of certain conditions of heterophoria as esophoria, etc. He states that “esophoria of high degree is not usually curable and is rare.” Just here we may find one of the causes of failure, for the proper operation for esophoria does and will cure many cases of this condition after the most careful examination and correction of the relief has failed to give any relief. I have seen these cases and have reported a number of them in which a proper operation has resulted in obtaining complete relief of symptoms that have existed for years, and this relief has been permanent, as my records will show. Hence, we have no right to accuse the physician who carefully operates for heterophoria of doing so from a “financial point of view”; the same accusation has been made regarding the operation for the removal of the appendix during the quiescent period, though that assertion has not altered the opinion of some of our best surgeons. Now we must not consider every physician who operates for heterophoria as a tenotomist—I decidedly object to the term, as I think that it is extremely unjustifiable even though it is one that seems to be freely used at the present time. Tenotomy has its tenants day, since the first operation was performed for the relief of heterophoria associated with myopia. To-day this procedure is giving way to the more advanced theory that heterophoria is due to a primary essential weakness of one or more of the rotators of the eyes in relation to its antagonist, and the operation that will strengthen these weak muscles is gradually taking the place of the tenotomy of the stronger muscle. If, then, esophoria may exist, is it not a sufficient corollary that exophoria and hyperphoria may also exist and cause an imbalance and eyestrain that can not be relieved by glasses, even when we have eliminated the “patient’s personal equation” and have given the glasses or prisms after a most careful and scientific examination?

As I understand the teachings of those who object to the operative correction of muscular insufficiency, we must examine the eyes under atropin, order glasses, if they do not relieve, repeat the same process and so on. It seems to me like "pulling Pelion on Ossa" to reach the patients and how many failures do we have by trying to follow that method.

We may reasonably ask: Is heterophoria refractive in origin and innervational in nature? To my mind and from my experience I must answer that question in the negative. What evidence do we have that it is refractive? Some patients have been relieved of their reflex symptoms by the use of glasses. Only this and nothing more, and yet how many patients do not
find the expected relief after the most careful correction of their refraction under the use of a mydriatic. There must be a reason somewhere for these failures. Perhaps all this may be infantile in argument, but, as the assertion in an affirmative sense is made over and over again, then the proper correction of that fault should bring about certain relief. Why does it fail in some cases? Because in the patients who fail to find relief there are other causes that are at fault besides the refraction, and chief of these is the imbalance of the ocular muscles. Perhaps this condition per se may not be great enough to cause all these many "ills that flesh is heir to," but associate this imbalance with a refractional error, and then we have the true cause of many reflex symptoms and the true reason why some patients who have had their imbalance corrected still have to wear the correction for their refractional condition. I do not think any one has yet claimed that the correction of an imbalance by an operation will at the same time correct an existing refractive error. A priori we might as well ask why does Dr. Gould advise his patients to wear prisms with their glasses if the imbalance is only refractive, or why shall we advise prism exercise? These are interesting questions that I trust may be answered at some future time. As to heterophoria being "innervational in nature" I am utterly unable to answer that question, as I have yet failed to find any reasonable explanation of that phenomenon in any writings of those who persistently advance the theory. To me it seems to be only an assertion completely wanting in proof. I shall be more than pleased when I can feel that "exophoria or subnormal adduction" is always curable by prism exercise and proper glasses. I have failed to be so successful with my cases if the imbalance is of any considerable degree. In those patients seemingly helped I fully agree with Winters that muscular tissue built up in this way is not nor can it be permanent. A child's muscular tissue may be developed by constant exercise, but unless that development is supplemented by a perfect metabolism from suitable food the tissues so built up will soon return to their former flaccid condition. It is so with the muscular balance of the eyes. We may improve the power of adduction by the constant exercise of the interni by the use of prisms—but this can not go on forever—and when we stop the exercise then we find the old imbalance returning with exophoria present. Esophoria—or subnormal abduction, if I may use that term—will not yield to the same exercise, and why? There is no answer to that question, nor will the procedure of prism exercise influence a vertical imbalance, yet the remedy for one should be the remedy for all.

Now if we earnestly seek for the true cause of these cases of imbalance, either with or without a refractive error, we must find it in the muscular structure of the eyes. Not in the refraction, a contributing cause, nor in the innervation, as the nerve impulse or the brain centers simply cause the muscles to act according to their functions and to the extent of their muscular development. (I do not think the prize fighter has any more innervation than myself, but I do know that he has more muscular power.) If, then, it is the muscle that is at fault, it must be in an essential weakness of an adductor or an abductor, in all cases of a lateral imbalance.

If these premises are correct, we may find some reason for the many diatribes against the tenotomists or the so-called muscle cutters, as it is a poor substitute for an operation that would strengthen a weak muscle at the expense of a strong one, but that does not prove that "operation cures nothing." If tenotomy is not indicated and exercise will not give a permanent cure, we must have some remedy for these cases, "our failures," and I think that is fully indicated from the very nature of the primary cause. In other words, if the essential cause of all cases of heterophoria is weakness of a muscular function, then the indications for the correction of that imbalance must be an operation that will enable that muscle to act normally. To this end we must turn to surgical means and an operation that will produce this effect can be found in the principle that if we shorten a muscle in its long axis then with the same degree of innervation we must have a greater action of motion on the peripheral part to which the muscle is attached. This stands without question, and reason should indicate to us that if we shorten the externus we must enable that muscle to increase the rotation of the eyeball outward and in this way correct the esophoria. Is this always successful? I can not say that it is always so, unless we could eliminate the personal equation, but I do state, without any reasonable contradiction, that we can restore the muscular balance by such an operation in a vast number of cases, so far as the objective and subjective tests will show. That we must have failures goes almost without saying, but I feel that the percentage of our failures with this operation, when it is fully indicated, will not be greater than the failures to relieve all cases of eyestrain by glasses when the refraction alone is at fault. For the purpose of study in reference to the statistics of refraction and motility, I have taken the last one hundred cases from my books at the date of this writing. Those patients were all personally examined as to the refraction and the muscular balance. In three-fourths, or 76, I found no special evidence of any muscular imbalance; among the remaining 24 there are three cases of squint which may be omitted. The remaining 21 patients all presented some evidence of heterophoria of more or less degree, as shown by the Maddox rod test, the prisms and the tropometer. Nearly all had some refractive error that was carefully corrected and glasses ordered. It is fair to assume that, as they did not return, the relief from the symptoms afforded by the glasses had been satisfactory. This reduces the number of cases of heterophoria that need attention to only 2 or 3 per cent. These patients, after the glasses had been fairly tried without relief of their symptoms, were operated on, according to the imbalance indicated by the examination, by the shortening of the muscle with the catgut suture. The final result proved the necessity for the operation.

A new element has been introduced under the subject of eyestrain that should require some attention at the present time; this is called subnormal accommodation. It was mentioned in Dr. Gould's paper, to which I have previously referred, but, unfortunately, though beautifully alluded to, yet we find no explanation of the necessary tests by which an ordinary observer or even a scientific one can make a suitable diagnosis of this condition. To say that it is an "elusive mischief" may express the matter fully, and the rough rule that plus lenses over the distance ones will sharpen the images of ordinary print when held at eighteen inches is hardly a scientific explanation, and one that is simply and wholly subjective. Plus lenses will always sharpen the image of printed matter in the young as well as in the old, but Nature will not admit of such a correction in the young when the ciliary muscle is active and the lens elastic.
Theoretically, we should all use plus lenses for the near, so that the divergent rays may enter the dioptric system parallel and so place the focusing muscle at rest. But just as "Nature abhors a vacuum," so will she resent any artificial interference with her natural function of seeing. To see at the near point the ciliary muscle must contract—you may call it habit if you wish—even with the convex glasses before the eyes. Now if our patients can read the finest type—Jaeger No. 1—either without glasses or with the full correction of the static refractive condition at six inches, or less, showing full reserve power for the usual reading distance, then I doubt very much if we can have any subnormal accommodation. Furthermore, if patients may accept the plus lenses the continued relaxation of the ciliary muscle will not be tolerated for any considerable time in young people. I always test my patients for the near point, and if I find it sufficiently near I do not think they will need or use convex glasses. It is true that exceptions will prove the rule, and that we do have exceptions I am willing to admit, but I think that the young persons who will use the convex lenses for any continuous work will not have a near point of six inches; they will have a large amount of uncorrected hypermetropia or some general physical condition that needs the services of the family physician more than that of the oculist. This leads us to the point which all oculists should realize: that we, as physicians, must work together; in other words, that the presence of subnormal accommodation is not really the indication for glasses, but shows, rather, that the general system is at fault and that under the care of the family physician, that best of all specialists, the patient will recover without the use of glasses.

Relief from the strain of the accommodation by outdoor exercise for the young, instead of passing the time reading books and papers, attention to hygiene and suitable foods to build up the bodily strength will soon restore a natural function that does not need nor should it ever have artificial aids to seeing. It is a difficult subject to decide impartially; we do not like to send our patients to others. We must consider that the practice of medicine is not a very lucrative one and financial reasons must intrude themselves—as one physician said to me when attempting an operation he should have referred to an oculist, "I wanted the money"—but if we had that true esprit du corps between the physician and the specialist, so that the physician could feel "my own shall come back to me," then he would more gladly seek the aid and counsel of one who may be more conversant with that special branch of medicine which the case demands. The same rule holds good on the part of the specialist. He also is a physician, but after he has completed his examination along his special lines and finding that the patient no longer needs his special services, should he not refer the case to the family physician? This is a golden rule that in the end would be productive of much benefit in a financial sense to physicians and to patients. I know that the specialist must treat certain of his patients in whom there are certain pathologic conditions that need his watchful eye and that these patients can not be referred back to the family physician if he considered their best interests. Diseases that involve the iris and the cornea should always be under the exclusive care of a specialist, but many others will do just as well under the care of the family physician after the diagnosis of the ocular condition has been made.

The teachings of ophthalmology of the present time present many changes from that of two decades ago. In my early work on refraction we were wholly dependent on the subjective tests of the visual power. We did not know if the patient's selection of the glass was the correct one, and if we were uncertain we were told to use the "crucial test," or the use of atropin for two or three days, when, the accommodation being paralyzed, the patient must accept the correct glass. It is true we could decide the gross errors, as hypermetropia and myopia, but low degrees of astigmatism were not easily recognized. Such was the armamentarium of the oculist in those days, and when I read a paper before the New York State Medical Society at that time on "Our Refraction Cases" the vast majority of them were classed as simple hypermetropia, while imbalance of the ocular muscles was not thought of except in fixed squint and paralysis.

To-day, how different are the teachings of ophthalmology! Now the crucial test is not so often necessary, and in my own teachings the objective methods of investigation of the refractive media takes the first place. I am not a prophet, certainly not in my own country, yet I venture to say and I hope I am correct that the day will come when the physician is so advanced in his refraction work that the diagnosis may be made and the correct glasses given without consulting the patient.

Perhaps that day is far distant, but during the past decade the advances in objective methods have been very great. Javal's ophthalmometer is an exceedingly useful instrument in the diagnosis of astigmatism and has demonstrated that it is in the anterior surface of the cornea in the vast majority of cases. The same instrument enables us to measure the radius of the cornea, by which we have an indication of the refraction, and in myopia the differential diagnosis between benign and pernicious conditions; in other words, between axial and refractive myopia. Furthermore, the retinoscope has come to us in the past two decades, and with this instrument we have an excellent method for the diagnosis of the refractive condition of the entire dioptric apparatus and one that is particularly useful as an objective method with children and illiterate persons. These are scientific instruments now used elaborated in a scientific manner, and with proper study and constant use will fulfill all that can be required of them. Their use can not be mastered in a day nor in a month, but I know that when we have fully appreciated the principles on which the examination depends the application of these tests, even without the use of a mydriatic, will reveal the static refraction of the human eye in almost every case. I do not state that these instruments are infallible, but I do think that if associated with these instruments we employ a careful and systematic method of the subjective test the crucial test will be seldom used or found necessary.

A scientific examination should not end with the correct estimation of the refraction. We can not say that the fault, or, I might say, the cause, of all the phenomena of eyestrain exists in the refraction. Errors must be corrected carefully and scientifically, but after that has been done we still meet with failures in some of our cases, not many, perhaps 4 or 5 per cent. With proper glasses these patients are better for a time and then the old symptoms return. Why? If they have been carefully examined, then changing the glasses will do no good and the fair and honest oculist must extend his examination to the motility of the eyes.
We see by the action of the refractive media on the rays of light from the pages of our books, etc., but to make that vision perfect we must have binocular vision; in other words, the eyes must work together so that the image of the object may fall on corresponding regions of the retina. This is one of the most beautiful functions of Nature that can and must be adjusted by the action of the straight muscles of the eyes, assisted by the oblique, to keep the vertical meridians parallel. Now if there is an imbalance of any of the ocular muscles, not sufficient to cause squint, but that does require a constant effort of innervation of one or more muscles to keep up this constant fixation of the visual lines when the vision is in the distance or in the near, it seems to me that this constant effort of fixation, this work that calls forth the constant innervation of a muscle or muscles about the eyes, must cause some symptoms of eyestrain. Nature has certainly a fixed standard of vision; that is acknowledged by all oculists; and I can see no reason why we should not have a fixed standard of rotation that will equalize the necessary innervation of the last muscle. This is denied by most observers, even to the extent of saying that no case of eyestrain can be due to a want of balance between the opposing muscles, and others that a comparison between the action of one muscle and that of the opposing one has no foundation in fact and is useless as a test. But observers and investigators have opposite views, and why? Because the careful and repeated application of strictly scientific tests in their cases proved the correctness of their views.

If, then, we admit the existence of an imbalance of the ocular muscles—and no oculist can deny it if he will read what Seguin² taught some twelve years ago—it goes without saying that the teachings of ophthalmology must include this subject. When we consider how important these movements of fusion and of fixation are to the individual, how beautifully they are adjusted to the wants and the pleasures of the human race to-day, how essential they are to the preservation of binocular vision, then it becomes imperative that we should understand the normal function of fusion and of fixation. We can not see when the eyes are moving; they must come to a state of rest, so that the images may be formed on corresponding parts of each retina, and if the muscular apparatus that controls this function is at all in imbalance there must be certain symptoms of eyestrain with, perhaps, certain phenomena of reflex conditions. What, then, is the normal function of fusion or duction and of fixation or version? Do not the daily wants of the individual show it? The eyes are at rest when the visual lines are parallel and the look is in infinity, and when the look is directed to the near point the visual lines are convergent. These are the requirements of the visual act in our daily life, and Nature has provided for it, as we find that abduction must always be greater than abduction, as about three to one. The function is voluntary and is controlled by the lower brain centers. This is well shown by the prism test, and I believe that a comparison between these rotations under the stimulation for fusion is essential and necessary.

Furthermore, the function of orientation or the topographical sense—to see around us without moving the eyes—is the function of our field of vision; but to see clearly in any one particular region the eyes must be directed to that point. This is the function of binocular vision and depends on the conjugate movements of the eyes, controlled by the higher brain centers. Here we find the field of version or fixation, with its limits established at the points to which the eyes can be turned from that of the first position, with the head immovably fixed. This, again, is a clearly demonstrated field, in which the rotations of the eyes have certain limitations that can be scientifically measured by an excellent objective instrument, the tropometer of Stevens. It is found in the normal field to be about 55° inward and about 50° outward from the position of rest with the look in infinity and about 33° up and 50° down. If we consider these as normal or standard fields, will not an imbalance show a decided change in these limitations?

Is it not as necessary to teach the method of making these investigations in the study of eyestrain as that of the refraction of the dioptric media? It goes without argument that this must be so. This is my method of teaching ophthalmology, and I venture the assertion that an eye which has normal vision and normal fusion and fixation will not have any symptoms of eyestrain, either reflex or in the eyes themselves. I will not dwell on the failures of these tests of imbalance except so far as to say that the more I study them the more I feel convinced that all imbalance is primarily and essentially due to an abnormal anatomic weakness of some one or more of the straight muscles of the orbit. If this contention is correct, then I do not advise any muscle cutting, but I do advise the operation known as shortening, by taking a tuck in the muscles and with the use of the catgut suture. I will conclude with the words of Dr. Gould, whom I have quoted so freely: “But when all has been confessed, discounted and allowed for, the truth remains that in no department of medicine is practice so satisfying, in none is cure so much the rule, as in refraction work.” But I would add to that assertion: If to our refraction work we also consider the possibility of an imbalance of the ocular muscles our failures may be less and our satisfaction may be greater.

**DISCUSSION**

Dr. A. E. Prince, Springfield, Ill., called attention to the closing paragraph of Dr. Valk’s paper, which, if correct, he said, will inspire him with new zeal in the treatment of a class of cases which have hitherto been a source of discouragement to him. This resolves itself, he said, into two tangible propositions: 1, all asthenopia is curable; 2, no asthenopes are advisable. Dr. Prince finds so many cases of asthenopia, accompanied by small errors of refraction and little or no muscular imbalance, and asthenopia after careful correction had been made, that he has formed the habit of designating on his record cards this class of cases by the term, atonic asthenopia. He nearly always finds that these conditions are caused by an unwise use of the eyes. A person interested in a piece of fancy work or a novel and continues in spite of Nature’s warning until forced to desist by symptoms of incapacity or discomfort. The clerk or shop girl is obliged to work the eyes a given number of hours, often at fine work, under unfavorable conditions, and breaks down. She goes to an oculist, who corrects the error of refraction she may have, she resumes work and breaks down again and is obliged to quit. The scholar in the lower grades lays the foundation of trouble by holding the book too close, thus often doubling the accommodation strain. The oculist finds a small error, which he corrects with glasses, and neglects to discover the cause; the child persists in this vicious habit of holding the book too close and permanent asthenopia is the result. The student with refraction to do three hours’ work in two taxes his visual powers by long hours of uninterrupted eyestrain, under faulty conditions, and develops photophobia and congestion, wears colored glasses, and persists, ultimately to find himself with permanent asthenopia. These cases, Dr. Prince calls atonic and he advises the patients

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to work at normal range and to interrupt every five minutes
with ten seconds rest. They often go along and do a creditable
amount of work, but are obliged to exercise care in the
use of the eyes. Over-taxis results in a setback, which
compels renewed rest and prudence. He asked Dr. Valk and
the members of the section if they met these cases, and if
they discovered refractive or muscular errors, the correction of
which does not remove the symptoms of asthenopia. Regarding
the second proposition, he said that he thinks Dr. Valk
expresses the modern tendency of thought relative to
tenotomies. Dr. Prince takes the middle ground. He first
favors advancement, in all cases, either heterophoria or stra-
bismus, and later performs tenotomy when indicated. This
procedure is rational and the results are better.

Dr. ISAAC WILKINSON, Washington, D.C., stated that three
years ago he began a very extensive study of his series of
asthenopic cases. He treated the patients first under homo-
 tropin, examined them before and after, its use giving the,
correction, eliminating muscular defects, and when they re-
turned with still some symptoms he used atropin. Out of a
series of fifty cases he has found what he terms astigmatic
accommodative cases. This arises usually from the use of atropin after from 3 to 6 days. He has had these
cases use atropin as long as ten days before the full amount
of astigmatism was shown. These cases with full correction
have been relieved entirely. Dr. Wilkinson believes that in
our haste to get through with our work we often pass over
our asthenopic patients without a sufficient number of exami-
nation. He believes that the continued use of atropin will
not bring out the accommodation, if there is latent astig-
matism with hypermetropia; we must use atropin. In Wash-
ington, where so many patients are clerks who go to school at
night, there is an unusually large number of people suffering
from eyestrain according to the population, and the loss of
ability to use their eyes means more to them than to ordinary
individuals.

Dr. G. M. GOULD, Philadelphia, declared that we have come
to a blind alley in this matter and that each one differs from
the other. He asked if there are any two or ten prescriptions
which agree for glasses independently given which agree in
the diagnosis of one patient's error of refraction. He does
not think so. He has hardly ever seen prescriptions which
agree nearly, much less in refraction, and when these cases
come to some sort of an agreement, he said, or make the
specialty ridiculous. They must get down to a basis of truth
and do away with this ever recurring disagreement with each
other. The only way to do this is to have an endowed and
recognized optical refraction school. By disagreements and
neglect, he said, ophthalmologists are producing quack optical
refraction schools.

Dr. LEARLUS CONNOR, Detroit, said that he daily sees the
work of fellow ophthalmologists with which he absolutely
agrees, and that he has done so ever since he began the
practice of ophthalmology. Therefore, he said, the practice
of ophthalmology in Detroit and in that part of the country
is not a bedlam. Further, he said that an increasing feeling of
agreement comes with years of experience and with our
appreciation of what the other fellow has done, but that it
will never be brought about from a priori ideas; it must be
based on observation, proper deductions from such observa-
tions and practice based on that. It is simply carrying out
the laws and principles taught by Morgagni more than a hun-
dred years ago, on which all scientific medicine has been based.
A special school to teach refraction from a priori reasoning is
not needed, he said, but every fact must be accurately
recorded and the efforts of one man placed in possession of
others, and as they have the same eyes, the same reasoning
powers, and the same desire to benefit patients, so will there
be a unity. Dr. Connor thinks that there is a substantial
unity in ophthalmology, as much so as in any other part
of medicine or any other profession. He does not think that
ophthalmologists need be afraid that they are not progressing,
or progressing in the right way, or that they do not have the
respect of intelligent men in and out of the profession every-
where.

Dr. G. C. SAVAGE, Nashville, said that notwithstanding what
Dr. Connor has said, he thinks that there is difference in re-
results and that this depends on the difference in methods.
When all can have the same methods all will have the same
results.

Dr. FRANCIS VALE, New York, said that he hopes the day
will come when physicians will be so advanced in refraction
examination that by objective methods they can fit glasses
without asking their patients any questions whatever.

CONJUGATE LATERAL DEVIATION.*

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It seems a well-established fact that all the moves of the
eyeball which are innervated from the cerebral cortex are associating or conjugate. It also seems
well established that lateral conjugate movements of the
eyeball can be effected through stimulation of the
center for the sixth nerve, or on the floor of the fourth
ventricle. Of the associate or conjugate movements of the
eyeballs, the lateral ones are the most extensive;
and this is consistent with the fact that the human
being can move about on a plane surface to an extent
which is limited only by physical obstruction or the
deficiency of his motor apparatus.

Dangers which threaten the human being come, as a
rule, in the nature of things from some direction in the
plane on which he stands. They rarely come from
above, and more rarely still from below. The associ-
ated movements of the eyeballs downward are much
limited, and those upward still more so. All movements
of the eyeball that are conjugate are assisted and sup-
plemented by the movement of the head, and it is evi-
dent that the movements of the head to the right and
left are greater than in other directions. These reflec-
tions bear directly on the importance of the conju-
gate movements in general, and particularly those to
the right and left.

Prevoit first pointed out the significance and impor-
tance clinically of conjugate deviations of the eyeballs
as a symptom of apoplexy; but this is a fugacious
symptom and passes in a few hours, because in the first
place it is a distant symptom and, in the second, the
sound side of the brain probably learns to perform
the duty of the stricken side. As will be shown in a case
later to be cited, localization from conjugate deviation
is uncertain, both on account of its transitory character
and because the position of the cortical center for asso-
ciate movements is not yet established.

Landouzy, Wernicke, and Henschon, from clinical and
postmortem study, have located the center in the inferior
parietal lobe. Munk arrived at the same conclusion
from experimental study. Ferrier, on the other hand,
observed conjugate deviation from electrical stimula-
tion of the angular gyrus, but, as this experimenter at
that time thought the angular gyrus the center of vision,
he concluded that the reaction obtained was in the
nature of reflex from the visual center to the oculo-
motor center. Ferrier, together with Horsley and
Beever, have placed the center in the second frontal
convolution.

Schaeffer and Munk more recently found that farad-
ization of one visual center caused conjugate movements
of the eyes to the opposite side, and Munk showed that
this occurred even though the association tracts between
the visual and what he regarded as the oculomotor cen-

* Read in the Section on Ophthalmology of the American
Medical Association, at the Fifty-seventh Annual Session, June, 1906.