Constitutional conditions combined with ametropia, the cause of asthenopia / by D.B. St. John Roosa.

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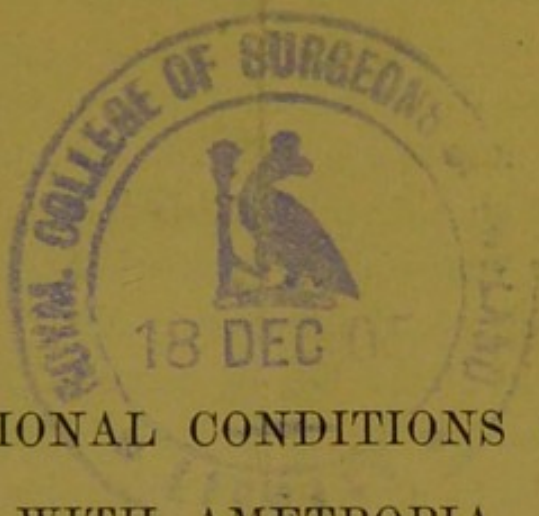

CONSTITUTIONAL CONDITIONS COMBINED WITH AMETROPIA THE CAUSE OF ASTHENOPIA.

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We are at least making some advance in our knowledge of the true place of abnormal ocular conditions in the causation of disease when the statements so widely made in this country only a few years ago, that they were the real causes of most of the neuroses, are pretty generally abandoned. The most that is now contended for by the authorities who attach a high importance to the want of equilibrium of the ocular muscles is that they produce a set or series of symptoms limited to the eyeballs, the lacrymal passages, the orbits, and the head, and comprehended under the term asthenopia. As is well known to the readers of the *New York Medical Journal*, it is my belief that in the refractive condition of the eyes will be found the source of any insufficiency, or of any want of equilibrium of the muscles of the eyeball, and that we must logically seek for relief by modifying the influence of the cause—that is to

say, by correcting the error of refraction. It goes without saying that in this statement I exclude paresis dependent upon disease at the origin or along the course of the muscles—for example, syphilitic or rheumatic paralyses.

Nor am I at all singular in this belief. Many ophthalmologists, after years of trial of prisms, both as means of gymnastic exercise to the muscles and as correcting glasses, and of tenotomies, have finally come to the belief that all which can be locally done for asthenopia not dependent upon actual disease is to correct the refractive error, especially the astigmatism which is so commonly associated with hypermetropia. I make this statement because Professor Woodward,* in his article upon Muscular Asthenopia, does me too much honor in saying that prior to my publications "the generally accepted opinion of ophthalmologists was that the causation of asthenopia is twofold." The view that ametropia, and not alone hypermetropia, is at the bottom of asthenopia, that the muscular conditions so much overestimated and to which such a pompous nomenclature has been given, are secondary to refractive conditions, is not a singular or a new one. Javal holds it, and has for years. It is certainly logically deduced from the writings of Alfred Graefe. Of four speakers on this subject at the late meeting of the New York State Medical Society, Callan, Mittendorf, and myself hold substantially the same views. Dr. Lewis H. Dixon, of Boston, a member of the American Ophthalmological Society, in writing upon my paper upon The Relation of Errors of Refraction, etc.,† says in a letter to me, dated December, 1890:

"I have been greatly interested in it (the paper on Muscular Asthenopia), for it so exactly expresses my own opinion on the subject. I have long been an intense believer

* *New York Medical Journal*, Feb. 7, 1891.

† *Ibid.*, April 19, 1890.

in the benefits resulting from the correction of very small errors of refraction, particularly of astigmatism. I have seldom met with cases of asthenopia otherwise explained where careful search did not reveal an appreciable error, the correction of which almost always relieved the trouble, and insufficiencies of various kinds disappeared.

"Aided by homatropine, retinoscopy, and Javal's ophthalmometer, I have often found slight refractive errors, making the eyes unequal, where less persistent search has failed, and the results of correction have surprised the patients and me. Very sincerely yours,

"L. S. Dixon."

After hearing my remarks upon this same subject in the Ophthalmological Section of the late Berlin Congress, Dr. Berry, of Edinburgh, said that he coincided almost completely with the opinions which Dr. Woodward believes will "prove harmful to many deserving patients," "in whose interest" he states that his reply to my papers is written. But, although I am not alone in my views, and although I believe the trend in opinion in all ophthalmological circles is toward the consideration of insufficiencies as effects of errors of refraction and not as causes of asthenopia, it must be admitted that only very lately muscular asthenopia has been generally supposed to exist and to deserve a separate classification. But there must be a time when opinions undergo revision, especially in a department of medicine like ophthalmology, which really became a science, as far as errors of refraction and accommodation are concerned, only about thirty-five or forty years ago. It is not probable that the last word will be said in these affections for many a year to come. I can not consider it any argument in favor of any opinion on asthenopia that it has been for some time accepted. As I have already stated in my original paper, it was Donders who broke in upon the general

practice of referring all asthenopia to the muscles, and demonstrated that generally, at least, it was to be referred to errors of refraction and accommodation. He hinted very plainly his belief that in fixed conditions, or comparatively fixed conditions—such as errors of refraction—would be found the most frequent sources of asthenopia. I have simply gone a step further, and with Javal, whose authority is generally recognized as no mean one, and with Bull, of Paris, one of Javal's colleagues, I have carried Donders's views to their logical conclusions. I believe that muscular insufficiencies and want of muscular equilibrium are very common, but they are dependent upon the shape of the eyeball or upon its refraction, just as strabismus depends upon myopia, or hypermetropia, or upon hyperopic or myopic astigmatism.

Professor Panas, of Paris, told me last summer, what all close observers of strabismus will, I think, be ready to confirm—that astigmatism was usually the exciting cause of strabismus. Not only is this true, but those who attempt to relieve squint and secure binocular vision may save themselves much needless trouble in the use of prisms in those cases that can be corrected without a tenotomy if they will correct the astigmatism and then allow the muscular insufficiencies to correct themselves. If errors of refraction cause such marked insufficiencies as strabismus, why do they not logically cause the lesser degrees of insufficiencies? There is no axiom more accepted in logic, as well as in mathematics, than that the greater includes the less.

Dr. Woodward's *a priori* argument seems to me to beg the whole question. He takes for an example a patient whose eyes are emmetropic, but whose ocular muscles are not in a state of normal equilibrium. We differ at the start. I can not accept any conclusions drawn from the premise that the human race contains any considerable

percentage of emmetropes. Emmetropia has been proved to be the rarest of conditions. I myself had something to do in originating the investigations that indicate this in my paper published in 1878,* entitled An Examination under Atropine of the Refractive State of Eyes with Normal Vision $\frac{20}{20}$, and which has never been affected with Asthenopia or Inflammation. This paper and later investigations by other ophthalmologists embracing many cases and more thorough examinations, especially as to astigmatism, have brought most ophthalmologists to believe what I have stated above—that emmetropia is an exceedingly rare condition. Writers upon this subject should at least attempt to invalidate the statistics of those who assert that emmetropia is an ideal rather than a real condition before they base so many arguments upon what we think have been proved to be incorrect premises.

Noyes, a writer for whom we all have great respect, in a paper upon Muscular Asthenopia, just published,† finds forty-seven per cent. of emmetropia. I do not know of any other recent authority who shares his views. Four, instead of forty, per cent. would be the most that I would admit. Of fifty persons recently examined for me by Dr. A. B. Deynars, one of my assistant surgeons at the Manhattan Eye and Ear Hospital, thirty-three had astigmatism, saying nothing of hypermetropia and myopia, and they were all carefully chosen as persons who never suffered from asthenopia.

It is, I think, begging the whole question at issue to urge as an argument that the determination of hypermetropia and myopia is thoroughly understood by any competent ophthalmologist. The question in this discussion is in part, What constitutes ametropia and how is it detected?

* *Transactions of the American Ophthalmological Society*, 1878.

† *Ibid.*, 1890.

When an ophthalmologist assumes that emmetropia is a frequent condition of the eyeball, that a low degree of astigmatism is no invalidation of this statement, and that muscular insufficiencies are independent conditions not resulting from errors of refraction, to my mind he never can form correct conclusions as to the causes of asthenopia except by sheer accident.

The speedy and accurate determination of the degree of corneal astigmatism and its axis in a given case was the last thing to be done to enable us to thoroughly study the local causes of asthenopia. This has been accomplished by the perfection of the ophthalmometer, an instrument invented in a crude form by Helmholtz, but adapted to general use by Javal. All the previous methods of determining the existence, degree, and axis of astigmatism, after twenty-five years of experience, I consider much inferior to this.

Donders said that hypermetropia was at the bottom of asthenopia. He was right in so far as local causes are at the bottom of this affection; but hypermetropic astigmatism must be included to make this statement complete. If this is so, how important to exactly determine when it exists! The ophthalmoscope and retinoscope in the hands of the most skillful observers, such observers as the late Dr. Edward G. Loring, as he himself said, often gave unsatisfactory results.*

They are subjective methods. The test by atropine or other mydriatics remains the only certain one. This is troublesome and tedious to a degree, but it is certain. Yet no one who has been released from the necessity of its employment will fail to be grateful. The letter of Dr. Lewis in the Journal of February 21st clearly sets forth the merits of the ophthalmometer, as did the paper of Dr. Speakman read before the Academy of Medicine in the

* *Text-book of Ophthalmoscopy*, p. 16.

autumn of 1890; but I wish now to speak of the instrument again with relation to this discussion. A release from the use of atropine and so forth is afforded by the use of Javal's ophthalmometer. In the hands of those who have carefully practiced its use, it simplifies the problem of the determination of astigmatism very much. The cornea can be adequately illuminated by it, even in a large city, by means of ordinary daylight during the working hours of almost all the days of the year. With such offices as are easily obtained in small towns, none but the most stormy days prevent its use. By the aid of electric light we are able to use it in any weather. To use the ophthalmometer with exactness, however, one must spend a little time in practicing with it It is not necessary to paralyze the accommodation, except in the very rare cases of spasm, and then for therapeutic purposes, if we can once exactly determine the degree of astigmatism. This is the key to the problem in the diagnosis of the causes of asthenopia; with that solved, we can soon determine by the ophthalmoscope or the test letters, without the use of any mydriatics even once, whether to advise a convex or concave cylinder, and whether or not an additional spherical will be needed.

The limits of any ordinary article will not allow of a further discussion of this point; but the members of my staff at the Manhattan Eye and Ear Hospital are giving, at every one of my clinics, demonstrations of the truth of what I am now stating.

I do not now use atropine for the determination of the degree or kind of an error of refraction once, when I formerly used it thirty times. But let me not be for one instant misunderstood. The causes of asthenopia in many cases are more far-reaching than can be found solely by the investigation of errors of refraction. My position has been

for years that, in many cases, asthenopia is only one of many neurotic conditions in the same individual. Let it be granted that ametropia is the most frequent condition of the human eyeball, and this or some statement like it must be true. If all the world is ametropic, and only a fraction asthenopic, ametropia alone can not always be the cause of asthenopia. Those asthenopes with trifling degrees of astigmatism or hypermetropia who are cured by glasses, those asthenopes who are relieved by prisms and tenotomies, are benefited by suggestion; but that tenotomy becomes a dangerous method of even this form of treatment, many of us can testify from having seen its results in diplopia and an aggravation of the neurotic condition.

The treatment by glasses, whether cylindric or prismatic, is a safe one at least. But tenotomies, whether *graduated* or measured, for anything but actual deformity, are a delusion and a snare. In fact, I do not believe at all in such a thing as a graduated tenotomy.

The certain something in the general condition which makes the grasshopper a burden, and which causes asthenopia to result from the most trifling errors of refraction, especially from astigmatism, is sometimes bad hygiene in children, or sexual perversions in youth, or any kind of nervous exhaustion in youths or adults. The wiser a physician the oculist is, the better he will do for asthenopia coexistent with ordinary errors of refraction. A mechanic may soon learn to correct myopia, manifest hypermetropia, and astigmatism, but a philosopher, with a broad medical education, will always be required to treat what may be called American asthenopia. The whole human physiology does not depend upon the eye and its functions. The eye is the light of the body, but the want of co-ordination of its muscles, and its so-called errors of refraction, do no harm at all to the health of the ordinary human being.

It only remains for me now to end my reply to Dr. Woodward by an analysis of the six cases which he believes substantiate his views. It is not probable that they will seem to do this to me, for it is many a year since I began the study of asthenopia. I have used prisms and tenotomies, and I have examined many cases in which they have been used. For the last few years I have replaced prisms by cylinders and have abandoned tenotomy for all latent affections of muscles, reserving it for strabismus, and I am now better satisfied with my results than ever before. It is not likely that six or sixty cases reported by another authority will change my views, unless I see in his cases that I have overlooked important points which I find to have been observed in the cases of the believers in muscular asthenopia. I do not find such points in the cases of the previous series published by Dr. Woodward; on the contrary, I find his methods defective and, to my mind, inaccurate. I changed my views after a careful study of thousands of cases, some of them watched for several years. My studies, as published in various papers, show, I think, a logical pathway to the road I am now upon. But, in justice to my opponent, I must pay some attention to the cases adduced by him as unanswerable arguments.

If we take the six cases that are presented as types of what may be done by graduated tenotomy, we see about this:

Case I.—The first patient is a physician whose age is not stated. He complains of smarting and burning, and a jerking of his head to the left. He is myopic; there is no mention of whether he has any astigmatism or not. A prism of 1°, base downward, before the right eye, stops the spasmodic jerking of his head. Four days later the jerking is less marked. He had jerked it three times during the examination. Twelve days later the jerking of the head has practically ceased, but a gradu-

ated tenotomy is performed in the right superior rectus. Six days after, the head has not jerked since the last visit. Nine days after this, it has jerked half a dozen times since the last visit, and then the right inferior rectus is divided. Twenty-three days after, there is no jerking of the head, and finally, *about nine months after the first interview*, the patient himself says his head jerks very little now. But if he gets excited or tired it increases in its movements. Very seldom does he bend over, as in auscultation, but that it will jerk two or three times. He is relieved of the burning of his eyes, and he can use his eyes in comfort while reading.

Certainly this is not a very marked relief of the general nervous system, considering all that has been done. It is, in my opinion, a case of general nervous disease uncured, with which the eyes have very little to do.

Case II is that of a lady, whose age also is omitted, who has always suffered from headache and pain in the back of the head, especially after using her eyes. She has double vision and dizziness. She has half a diopter of astigmatism *against the rule* on the right side, and none on the left. The next day she appeared to have astigmatism in each eye, in both instances *against the rule.* Three days after, still astigmatism, but varying in degree. She objects to the use of atropine; therefore, glasses plus one half a diopter, axis 180°, are ordered for each eye; relief was partial until six months later, when, after using them constantly for six months, she gets perfect relief. The next year, not being cured, this patient is put under atropine, and then she is found to have hyperopic astigmatism with the axis exactly opposite what was first recorded. Now the astigmatism *is according to the rule* (axis 90°). Certainly the use of Javal's ophthalmometer would have avoided this mistake. It is very difficult to conceive how she got much benefit from the glasses, when, according to the atropine test, which the author, with almost all others, believes to be crucial, she had been wearing the wrong ones for six months. Finally the astigmatism is properly corrected, but the headache continues; and then graduated tenotomy is performed, and some four days

afterward she reports that she has not had a severe headache since the operation. Twice she has had mild headaches, however.

Case III is that of a lady, age again not stated, who is anæmic, has headaches, who has astigmatism against the rule in each eye—that is, myopic astigmatism, axis 90°. This is to me an extraordinary condition of things, because her vision in the right eye is $\frac{20}{20}$, and in the left eye $\frac{20}{30}$ without glasses. It is very rare indeed that there is myopic astigmatism with the axis 90° with such vision. We may be forgiven for questioning the correctness of the estimation of the kind of astigmatism in such a case as this. Spasm and hyperopic astigmatism, but not myopic astigmatism, present these symptons. No relief came from the glasses prescribed, and, seventeen days after, the left internal rectus is divided; twenty-two days after, the right internal rectus. She takes a great deal of iron, and eleven days after she does not suffer from headache, but, if she uses her eyes excessively at night, she is apt to have headache, and she does not wear her glasses, which is very wise, if our supposition as to her actual refraction is correct.

Case IV.—A miss, age unstated, with twitching of the lids, nearly constant headache, and anæmic. She has vision of $\frac{20}{20}$, but she accepts concave cylinders of a quarter of a dioptre in one eye, and a convex in the other of the same power. Convex cylinders are ordered without benefit, or with slight. Bland's iron pills are given in large quantities, but, nine months after, graduated tenotomy of the right internal rectus is performed. A prism of 2°, base out, is prescribed for each eye; medicine is stopped. Two months after this the left internal rectus is divided. Finally, nine months after this, according to her own account, she is again in trouble with her eyes. She says: "In some way or other I strained them, and since then have not attended school, but it is only since I strained them, probably through carelessness, that they troubled me." This certainly is not a very promising result for a patient who has submitted to two tenotomies and been under treatment for eleven months.

Case V.—A gentleman, age unstated, headache after read-

ing, occasional diplopia, vision in each eye $\frac{20}{20}$, a quarter of a diopter of astigmatism in each eye. A prism is ordered 2°, base inward. In about two weeks the right external rectus is divided, and prisms are used for exercising the internal recti. In about two months this patient has given up his glasses, has no headaches, and his health is better. This is undoubtedly a good case, but one would not like to cause a great deal to depend upon it. An operation was proposed for him, but it was rejected. He also seems to have been an astute patient, for he recovered without it. To my mind, the diplopia in this case probably depended upon hyperopic astigmatism. Finally the patient will find an oculist who will correct his astigmatism, and he will leave off the awkward prisms and be more comfortable.

Case VI.—The last case is that of a lady, age unstated, with asthenopia and headaches. General health excellent; vision normal; rejects all glasses. No error of refraction detected with the ophthalmoscope. A prism is ordered of 1°, base down, for the right eye, and for the left a half degree, base up. Nine months after she is able to use her eyes for any length of time without any uncomfortable feelings.

This is the only case of the series in which no tenotomy has been performed; the strength of the prisms will be noted—one degree in one, and half a degree in the other. As I said in my original article, I still can not understand how prisms so weak as this will have any more than a suggestive result in treatment. If they have, cylindric glasses will do better.

I seriously ask the student of asthenopia to look over the six cases in the light of these criticisms, and answer if he can find in them any ground for the justification of the value of graduated tenotomies or prisms. I can not.

To recapitulate my argument:

1. I believe that the general nervous condition, especially the nutrition of the nervous system, will have very much to do in determining the causes of asthenopia, even

in the cases with considerable errors of refraction. I point to the asthenopia after typhoid fever, which finally disappears without any special treatment, as an index to what is meant by this conclusion.

2. In what is comparatively a fixed condition—that is to say, a decided deviation from the ordinary standard in the eyeball—is, to my mind, a most probable local source of asthenopia. Muscular insufficiencies result from these deviations. This is illustrated in a marked way in strabismus.

3. The ophthalmologist must not ignore the fact that the standard of emmetropia laid down by the writers from 1850 to 1875 is incorrect. Since then it has been pretty clearly shown, and is capable of wide demonstration, that ametropia exists in at least 90 per cent. of the human race.

4. It must not be forgotten that neurotic patients will submit to any treatment, even to ocular tenotomies, month after month, and year after year, in the vain hope of finally achieving what is impossible for some individuals—that is, the use of the eyes as long as they choose under any conditions without any discomfort, or until they derive complete immunity from ailment and pains, which heredity, evil habits, or environment render impossible. Patients can not be made over. An admission of this, and less ambitious hopes for the cure of neuroses, will prevent the profession from making statements that only serve to bring our scientific name into disrepute.