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Amblyopia of Squint.

BY

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THE AMBLYOPIA OF SQUINT.[1]

BY O. F. WADSWORTH, M.D.

THE theory that the amblyopia so prevalent in strabismus is the result of a suppression of the image formed in the squinting eye, in order to avoid the confusion excited by double vision, was for a long time universally accepted and was upheld by the authority of the leaders in ophthalmology. This amblyopia was regarded as a form by itself and given the name of *amblyopia ex anopsia*.

Schweigger, in his handbook, published in 1871, denied that the amblyopia is so produced, and asserted that it is on the contrary a congenital defect; and Alfred Graefe, in 1875,[2] retracted the opinions he had formerly expressed, and gave in his adhesion to Schweigger's views.

By the early part of 1876, I had, from observations made during the previous few years, convinced myself that *amblyopia ex anopsia*, so called, did not exist, and believed at first that I was alone in this conviction. Examination of the literature of the subject, preparatory to a paper, showed me that Schweigger and Graefe had preceded me, and had presented the matter so much more fully and so much better than I could hope to do with my limited material that I immediately abandoned the idea of writing.

A little later, in 1877, Leber[3] strongly upheld *amblyopia ex anopsia* against the assaults of Schweigger and Graefe, without, however, presenting anything new.

[1] Read before the Boston Society for Medical Improvement, December, 1886.

[2] Graefe and Saemisch, VI.

[3] Graefe and Saemisch, V.

Again, in 1881, Schweigger published a masterly monograph on strabismus, based on the observation and statistics of a large number of cases, in the course of which he argued more at length against the suppression-amblyopia theory.

If I now, at this late date, bring the subject to your attention, it is because what I regard as an erroneous doctrine still is widely prevalent, and is taught by most of the recent text-books. At the last meeting of the American Ophthalmological Society Dr. Theobald read a paper in its support.

Before going farther, it should be said that, in many cases, opacities of cornea, lens or vitreous, or changes in the fundus, sufficient to account for the amblyopia present, are found in the squinting eye. With such cases as these the discussion need not concern itself.

The importance of the question is not merely theoretical. If *amblyopia ex anopsia* is a fact the practical treatment of squint should be greatly influenced by it. If a squint persisting for a few months, as some of the advocates of the doctrine assert, is sufficient to bring about a pronounced and permanent defect of vision in the squinting eye, it should be our duty in all cases to operate for its removal as early as possible. In all cases, at least, in which no visible lesions, ample of themselves to prevent a useful amount of vision, are present. If, on the other hand, persistence of the squint does not cause amblyopia, we may postpone operation with advantage till such time as it is possible to determine any errors of refraction that may exist, and, by correcting these errors by appropriate glasses, assist the effect of the operation. That errors of refraction do exist in a large majority of cases every one admits.

The chief hindrance to a decision of the question

which shall be generally accepted is the fact that at the early age at which strabismus usually begins (two to four years) it is in most cases impossible to obtain any satisfactory evidence as to the amount of vision; and, on the supposition that a few months of squint are enough to cause a decided loss of sight, by the time the child is of an age to admit the application of convincing tests it is too late to settle the dispute as to whether a defect then discovered is congenital or acquired. Again, if when the tests can be made no defect be found, as not seldom happens, the advocates of *amblyopia ex anopsia* claim that this is accounted for by the circumstance that the squint has not been monolateral, that sometimes the habitually deviating eye has been used, and there has been therefore no continuous suppression.

On the other hand, it is to be said that no satisfactory evidence of deterioration of vision in a good eye after squint has appeared has been presented. Theobald [4] says of this, "that there are scores of cases recorded in the note-books of ophthalmic surgeons in which there has been observed a progressive decline in the vision of squinting eyes, I do not for a moment doubt." That he has record of none such himself is evident. But he cites Roosa as reporting one case of this character. Roosa [5] gives this case, in which the records of vision were made at an interval of four years by two different assistants, as "the only case that I have personally seen, where any color is given to the theory of amblyopia, or deterioration of vision from disuse of the eye." But he also states that the results of the analysis of all his cases give no support to the *amblyopia ex anopsia* theory.

Even one case carefully observed deserves respect-

[4] Trans. of Am. Ophth. Soc. IV, 2, p. 284.
[5] The Results of the Operation for Convergent Squint. Transactions of the Med. Soc. of the State of New York, 1886.

ful consideration. But in the case here quoted there is not, it seems to me, sufficient guarantee against possible mistake. And the mistake may have occurred in either of two directions; the first observer may have overestimated the vision of the squinting eye, or the second observer underestimated it. Any one who has had much experience in testing vision must have recognized the liability to error in the examination of young children, and this liability is decidedly increased when we have to do with a squinting eye.

Let me illustrate. A child of some nine years of age with convergent strabismus was examined by my assistant. The letters of XX were read at 20′ first by the non-squinting, then by the squinting eye, thus apparently showing V. $= \frac{20}{20}$ in each. Then, at my suggestion, all the letters of XX were read backward, without mistake, by the squinting eye. Yet on covering the test card so that only one letter could be seen at a time, letters of XL were the smallest that this eye could recognize. Its vision was, in fact, only $\frac{20}{40}$. The letters had been unconsciously committed to memory when they were read by the good eye. Here, if the examination had been pushed less far we should have overestimated the vision, and a few years later re-examination might have shown progressive deterioration of vision in a squinting eye.

Another example demonstrates how vision may be underestimated at a subsequent examination. A girl of six years, with marked convergence of the left eye since the age of two and one-half. Under the influence of duboisia, V. $= \frac{14}{10}$ in each eye. The test was made with every precaution, the non-deviating eye being effectively bandaged, and a card taken for the testing of the squinting eye which the child had never before seen. Tenotomy of left internus under ether, with decrease, but not correction of the convergence.

Eight months later the best vision that I could get was R. $\frac{1}{7}\frac{4}{5}$, L. $\frac{1}{2}\frac{4}{0}$. There was apparently decided diminution of vision in *both* eyes, greater in the squinting eye. The child was of a very nervous temperament, and after the tenotomy always became excited whenever a visit to me was suggested. I entertain no doubt that the deficiency of vision found at the later test was due to the mental disturbance and excitement which then existed.

Even in adults it is by no means always easy to determine accurately the amount of vision. A lady of forty-two years had had a strong convergent squint of the right eye since the age of nine. She had been an invalid for a long time. On repeated examination on different days, both without and with atropine, the best vision obtained was R. $\frac{1}{7}\frac{4}{5}$ —, L. $\frac{1}{2}\frac{4}{0}$ —. Seven months later, her general health having meanwhile improved, but the squint remaining the same, there was found R. V. $= \frac{1}{4}\frac{4}{0}$, L. V. $= \frac{1}{1}\frac{4}{2}$.

The two main arguments on which the supporters of acquired amblyopia rely are, (1) the peculiar character of the defect of vision, (2) the asserted fact that a visual defect of like character is rarely if ever found without squint.

Let me state their thesis. The peculiarity of the amblyopia consists in this: with no visible change in the eye, a perfectly normal appearance of the fundus, and without any narrowing of the boundary of the visual field, there is impaired function of the region of the macula and all those parts of the retina which receive images of objects situated in the common visual field.

In order to avoid the bewilderment caused by the double images which are induced by the faulty direction of the squinting eye, the impressions received through it are persistently suppressed, and the attention

is concentrated on the impressions received through the other. The process is the same as when we apply one eye to the microscope or ophthalmoscope and, keeping the other open neglect the images formed on its retina. In strabismus, however, the suppression of the images is not merely temporary, but continuous, and causes a blunting of the function of the retina or of the corresponding visual centres in the brain. Only the peripheral portion of the nasal half of the retina, not being concerned with the common visual field, is exempt from this influence and unaffected.

At the early age at which squint generally begins a few months of suppression may be enough to produce a high degree of amblyopia, yet the longer the suppression has persisted the greater is the deterioration of sight. Theobald [6] assents to this last as a rule, but assumes that structural changes take place in the visual centres,[7] and that when the suppression "has produced such a degree and such a kind of amblyopia as shall do away with the discomforts of double vision, its purpose is accomplished, and it probably ceases to be operative."

In alternating strabismus the suppression is not constant, and therefore amblyopia does not result.

This is, I believe, a fair statement of the theory. How is it borne out by the facts? If it be true, then the amount of amblyopia found in squinting eyes should be approximately the same, or, if the element of time is to be considered, its degree should show some correspondence with the length of time the deviation has existed. No one has ventured to offer an estimate of the least amount of amblyopia that is capable of satisfying the suppression theory.

Schweigger [8] has given statistics of the refraction

[6] Loc. cit., p. 281.
[7] Loc. cit., p. 283.
[8] Klinische Untersuchungen ueber das Schielen. Berlin, 1881.

and vision of several hundred cases of strabismus. He found vision less than $\frac{1}{7}$ in about 30 % of the squinting eyes, but makes no subdivision between normal vision and V. = $\frac{1}{7}$.

Hospital cases, treated as out-patients as they generally are, rarely are recorded or examined with sufficient accuracy of detail to furnish available statistics.

I have taken the cases of strabismus occurring in the records of my private practice. All those in which changes in the fundus, or opacities of the media, might account for the defective sight, and all in which the youth of the patient or other cause prevented the determination of the vision and satisfactory examination of the retina have been thrown out as useless for the purpose in hand. With this exception only, four cases in which the vision of each eye was recorded, but there was no note as to the fundus, have been included, but in each of these the vision of the two eyes was nearly alike and fairly good, $\frac{1}{12}$ to $\frac{1}{30}$.

There remained 57 cases of convergent strabismus.

Five of these had divergence when they came to me ; 4 following operation for convergence ; 1, æt. 23, M. $\frac{1}{18}$ and V. = $\frac{1}{3}$ in one eye, H. $\frac{1}{36}$ and V. = $\frac{1}{10}$ in the other, had changed spontaneously from convergence. Three were nearly or quite straight, after tenotomy. Five were alternate, 4 periodic, 47 monolateral.

In 15 cases (26 %) (4 alternate, 1 periodic) V. was precisely the same in each eye ; in 7 of these (2 alternate, 1 periodic) V. = $\frac{1}{10}$ to $\frac{1}{12}$.

In 9 cases (16 %) (1 alternate) the difference between the vision in the two eyes was not greater than that between $\frac{1}{20}$ and $\frac{1}{30}$.

The poorest V. in these 24 cases was in a case of M. with slight nystagmus, R.— $\frac{1}{8}$, V. = $\frac{6}{30}$, L.— $\frac{1}{7}$ ○ — $\frac{1}{18}$ cyl., V. = $\frac{6}{30}$.

Of the remaining 33 cases, V. in the squinting eye was: $\frac{14}{20}$ to $\frac{14}{40}$ 7 (12 %): $\frac{14}{30}$ to $\frac{14}{100}$ 8 (14 %): $\frac{14}{110}$ to $\frac{14}{200}$ 7 (12 %) (3 periodic): $< \frac{14}{200}$ to counting fingers at 15″ 11 (20 %).

The poorest V. in the non-squinting eye was $\frac{14}{40}$.

The same selection as before gave 11 cases of divergent strabismus: 1 alternate, 2 periodic, 8 monolateral.

In 5 (45 %) (1 alternate, 2 periodic) V. was alike in the two eyes, $\frac{14}{10}$ to $\frac{14}{15}$.

Of the other 6 cases, V. in the squinting eye was: $\frac{14}{15}$ to $\frac{14}{20}$ 2 (18 %): $\frac{14}{50}$ to $\frac{14}{100}$ 2 (18 %): $\frac{14}{200}$ 1 (9 %): fingers 5′ 1 (9 %): both the last two cases having As. $\frac{1}{8}$ with ophthalmoscope.

Lowest V. in non-squinting eye $\frac{14}{30} - \frac{14}{40}$, with M. $\frac{1}{8}\frac{1}{5}$.

Uniting all the cases they give 30 % with V. $< \frac{1}{7}$ ($\frac{14}{100}$), practically the same percentage as found in Schweigger's much more extensive statistics.

These cases are not numerous, but I believe they are fairly reliable. The only error probable is that in a few the full amount of vision may not have been obtained; certainly it was not over-estimated.

The statistics here given seem to offer little encouragement to the theory of *amblyopia ex anopsia*. Half the patients had in the squinting eye V. at least = $\frac{14}{20}$ to $\frac{14}{30}$, leaving out of the calculation two with vision somewhat less than this, but alike or nearly alike in both eyes. It is certainly common enough to find vision as low as $\frac{14}{30}$ in eyes without squint and without visible change; more than half the cases then must be considered to have escaped the evils of suppression.

It is said, however, that the instances in which vision is not greatly decreased may be easily explained

on the supposition of an occasional alternation, that this is sufficient to preserve the function intact. I can hardly believe it has been expected that this supposition should account for some half of the cases. If the explanation be admitted, how shall we account for the fact that in three of my patients the eyes were sometimes straight, both by the statement of parents and by my own observation, and yet vision was only $\frac{14}{110}$ to $\frac{14}{200}$ in one eye. All the three had like refraction in the two eyes (two hypermetropic, one emmetropic), all measured with the ophthalmoscope as well as with glasses, two under homatropine. The emmetrope, a boy of seven years, had, three years earlier, before he knew his letters, evidently imperfect sight of one eye.

It is asserted also that a difference in the refraction, by causing the images formed on one retina to be indistinct, facilitates suppression. This may be fully granted without at all helping the theory. The question is not whether there is suppression, suppression of some sort there must be else we should have double vision, but whether the suppression causes amblyopia.

On the other hand, Theobald brings forward differences of refraction as a hindrance to amblyopia. He says,[9] "the non-development of amblyopia in exceptional instances is easily explained without the necessity of abandoning the suppression theory, a marked difference in the refraction of the eyes being competent to produce this result, because the indistinct retinal image which the squinting eye receives under such circumstances causes less confusion and is, therefore, less apt to be suppressed."

Both views can hardly be true. With regard to the latter one, four of my cases with monocular amblyopia (V. = $\frac{14}{200}$ to counting fingers at 2′) had, three

[9] Loc. cit., p. 281.

of them As. $\frac{1}{12}$ to $\frac{1}{8}$, the fourth a high degree of myopia and As. in the squinting eye, measured with the ophthalmoscope. The marked difference in the refraction did not protect here. Nor did it protect in a fifth case with M. $\frac{1}{16}$ and V. = $\frac{14}{15}$ in one eye, H. $\frac{1}{36}$ and V. = $\frac{14}{70}$ in the other.

Another supposition of Theobald, already quoted, that the suppression probably ceases to be operative when it has produced a certain degree and kind of amblyopia, accords poorly with the great variability in the amount of the visual defect which the statistics have shown.

To answer the second argument, that monocular amblyopia without visible change is rarely if ever found in non-squinting eyes, I refer again to my records. The indexes of my case books do not make the selection of these cases so easy as those of strabismus, yet I have readily found 15 cases, in which the eyes were perfectly normal in appearance, which had never squinted, but had monocular amblyopia ranging from V. = $\frac{14}{40}$ to < $\frac{1}{200}$, five of them with V. < $\frac{1}{7}$. The vision of the better eye was in only one of the fifteen cases so low as $\frac{14}{30}$, in one other so low as $\frac{14}{20}$; while of the five cases with V. < $\frac{1}{7}$, in but one had the better eye so little vision as $\frac{14}{15}$.

Moreover, I have excluded from this series cases with excessive degrees of H. or M., or with more than the lowest degrees of astigmatism, to avoid the possible objection that the amblyopia was due to the same faulty development which caused the refractive error. Had such cases been admitted the list could have been easily extended, and it is to be noted that several such cases are found among the amblyopic squinting eyes.

Further, squint is a personal blemish, patent to all

observers, and naturally sends the patient to the physician; congenital monocular amblyopia is not apparent, is often neglected, often discovered only accidentally and late in life.

A man of fifty-one years came to me because he had, two or three weeks before, accidentally found his right eye imperfect. Both eyes were quite normal in appearance, externally and internally. R. V. = $\frac{14}{11}$; L. V. = $\frac{4}{12}$; H. $\frac{1}{48}$ in both. On inquiry, I learned that as a boy he had used the left eye in shooting, because, as he said, "I could never close the left eye well and see well with the right." Yet only lately had he realized that the right eye had imperfect sight.

The second argument, then, is evidently based on faulty or limited observation. Nor need it weaken the force of this statement if monocular amblyopia is found in larger proportion among those who squint than among those who do not. Donders,[10] at the same time that he pointed out the influence of hypermetropia in convergent squint, also showed that imperfection of vision in one eye rendered the occurrence of squint more easy.

One other argument needs to be noticed: It has been frequently claimed that a marked improvement in the sight of the squinting eye often occurs immediately, or in a short time after it has been straightened by tenotomy, and this asserted fact has been adduced as evidence that the amblyopia could not have been congenital.

Perhaps the extravagance to which this claim has been pushed never reached a greater height than when, two or three years ago, an oculist reported a case in which, as he asserted, the squinting eye had not even perception of light, but after it had been straightened

[10] "On the Anomalies of Refraction and Accommodation of the Eye," p. 295, 296. London, 1864.

by operation, acquired by exercise a considerable amount of vision.

Leber [11] states: "In the lesser degrees of amblyopia from disuse, where central vision has not yet been wholly lost, great improvement or complete restoration may be effected by practice"; and on the following page: "Very remarkable are the cases where tenotomy brings about an immediate improvement of a very high degree of amblyopia, which, therefore, certainly must have been caused and maintained by the squint — cases which still need an explanation." He gives as an example a case reported by Knapp [12] in 1863, in which, directly after operation, vision was said to have risen from $\frac{1}{16}$ to $\frac{1}{2}$. Then, after some further discussion of these cases, he goes on to say: "Sometimes, also, the strabismics find the right use of their eyes even before the tenotomy; I have seen cases in which, during my testing, the at first greatly reduced vision, within a few moments improved almost as remarkably as in the above-mentioned case after the squint operation." The hollowness of the evidence that straightening the eye causes an increase in its vision could hardly, I think, be better illustrated.

One would think that such experience as this would have awakened attention to the liability of mistake, and the necessity of careful and repeated testing of the squinting eye before deciding on its actual amount of vision, or assuming an improvement from the operation. Leber seems, indeed, to have had some inkling that the unaccustomed strain on the muscles needed to bring the squinting eye into correct fixation may have interfered with the proper concentration of the attention on the visual act. But it evidently did not occur to him that the theory of amblyopia from suppression

[11] Loc. cit., p. 1014.
[12] Monatsbl. für Augenheilk, I, 474–478.

demands a continuous, not an intermittent amblyopia; not an amblyopia which is present only so long as the attention of the squinter is absorbed by something other than the letters which he is called on to read.

This experience of Leber offers a key to the whole matter, not simply that an unaccustomed strain on the muscles accompanies the attempt to see — that is only one of the factors involved — but the secret of the apparent improvement observed after tenotomy is found in the lack of sufficiently careful previous examination. When vision has been exhaustively tested before tenotomy, no real improvement is found after it.

The facts, that a not inconsiderable proportion of strabismics have vision alike or nearly alike in the two eyes even when, according to all the evidence attainable, the squint is strictly monolateral: that in a considerable number of other cases the amblyopia of the squinting eye is not of high degree, while in some of these vision of the non-squinting eye is also below the normal standard; that only about 30 % of squinting eyes, among them many with high degrees of astigmatism or other refractive error, have vision less than $\frac{1}{7}$; and finally, that there are many persons with congenital monocular amblyopia of equally high degree who do not and never have squinted, make it a reasonable requirement that, before the theory of suppression-amblyopia be accepted, some positive evidence of its existence, something more than a mere hypothesis, should be brought forward to support it.

There is, however, a condition of functional disability often found in squinting eyes which is dependent on the squint, and is in many cases capable of remedy. But it is not by any means peculiar to squint; it is not necessarily accompanied by a reduced acuteness of vision; correction of the deviation is not required for

its removal; and when it is removed, acuteness of vision is no greater than it was before.

It consists in an incapacity for continuous use of the amount of vision which the eye possesses, and is due to long-continued want of exercise of the function. It may be overcome by separate practice of the eye. It is found also without squint when one eye has such an error of refraction that at all times very indistinct images are formed upon its retina. Such an eye may take part in binocular vision to a certain extent, and aid in the estimation of distance; but if its error of refraction be corrected and it be called on to read it will give out speedily, yet, with practice, it may learn to read fluently and continuously. Clearly, this condition is widely different from that which has been described as amblyopia from suppression.