Original Communications.

SOME REMARKS ON CATARACT.

By EDWARD G. LORING, M.D., New York. Read before the American Ophthalmological Society, July, 1871.

MR. PRESIDENT,—As you are well aware, it has long been recognized that astigmatism often occurs as a disturbing element in the vision of patients who have been operated upon for cataract. Thus Dr. Knapp, in Graefe's Archives for 1867, observes that "we should not neglect to test eyes which have been operated on for cataract, with cylindrical glasses, to see if vision is not thereby improved. This is found to be the case in a marked degree, where the result has been a good one, in about one-fourth of the cases. Thus I have found where $V = \frac{1}{4}$, corrected it will be $\frac{1}{2}$, and $V \frac{1}{4}$ will become $\frac{3}{4}$ and so on."

But notwithstanding this knowledge of its existence, very little has been done by the majority of operators in determining the degree of this error in refraction when making out their statistics of vision, and still less has the attempt been made to remedy the defect by prescribing suitable glasses.

This latter is no doubt due to the fact that the necessary sphero-cylindric glass is so heavy and of so awkward a shape as only to be worn with great discomfort to the patient. Thus if we wished to give a spherical glass, for example $+ \frac{1}{4}$ combined with a cylindric glass, the spherical surface inasmuch as it would have to be put all on one side would have to be ground on a radius of 2 inches. This degree of curvature would, in order to fill the eye of the spectacle frame, require the glass to be very thick in the centre, the apex of which would consequently project a good deal from the plane of the rim of the spectacle, while the slight curvature of the cylindric surface would hardly project at all. Such a glass as this is necessarily very heavy, and very clumsy and uncomfortable.

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With the hope of remedying these objections so as to allow us to give astigmatic glasses to cataract patients, I have contrived the glass which I now present to the Society, and which is made in the following manner:—

A simple cylindric glass of the required strength is first set in the spectacle frame in the usual way, the axis of the glass of course running in the required direction. A thin plano-convex glass is then ground, and, taking advantage of the fact that lenses can be cemented together by Canada balsam, this is firmly fixed by its plane surface to the back or plane surface of the cylindric glass.

As the diameter of the plano-convex is made only equal to the vertical diameter of the spectacle frame, and not to the longitudinal one, it follows that a large quantity of glass is thus dispensed with, and the weight of the glass is thereby much reduced—the two combined lenses being, in fact, when nicely made, only one-fourth of the weight of the common spherical cataract glass as found in the shops.

In the figure, *A* gives a longitudinal section of the glass, the dotted line making the line of union between the two lenses, while *B* shows the front view of the glass as it appears in the frame, the dotted line showing the circumference of the plano-convex glass.

As you will observe, the edge of the convex lens is so delicately ground and so perfectly fitted to the cylindric glass, that the point of union is barely perceptible when the glass is worn, and the peculiarity of its

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construction would escape the notice of any but a very observant eye.

The pair which I now offer as a sample has a spherical surface of $+\frac{1}{2}$ (really equal to a biconvex $+\frac{1}{4}$) and a cylindric surface $+\frac{1}{2}$ c, the patient being astigmatic to that degree in the vertical meridian. With the best corrective with spherical glasses vision equalled $\frac{1}{4}$. With this glass it rose to $\frac{1}{2}$.

The chief objection which would be raised against glasses made in this manner would, in all probability, be on account of their liability to come apart. Whether this is a valid objection remains to be proved. I would say that this pair has been in constant use for four months, and in that time they have been dropped twice, once in a crowd, from which they were only rescued after the frames had been considerably bent. They certainly show no signs of separation between the two lenses, and we know that the lenses of telescopes and opera glasses are subjected, oftentimes for years, to the extremes of temperature and hard usage without showing such a tendency; and even if the glasses should occasionally separate, it is certainly a simple matter to recement them. This slight inconvenience would be more than compensated, it seems to me, by the increased amount of vision, especially when, as in the present case, it is doubled.

In this connection I would remark that it seems to me that a want of uniformity now exists among operators in regard to testing the vision of cataract patients, which has a tendency, to say the least, to create confusion, not only as to the results of different operators, which is of comparatively small importance, but also as to the merits of the method of operating itself, which is of vast importance.

It is almost universally considered now, after the so-called peripheric linear has been in vogue for the past eight years, and after it has been consequently thoroughly tried, that it is far superior to the old flap operation, and this opinion purports to be founded not—as many medical opinions are—on unsupported convictions of its great originator and a few of his most skilful disciples, but on carefully prepared statistics, which, as they are based on mathematical principles, are of almost mathematical exactness. It is alleged that as the basis upon which the statistics for both operations were compiled was the same, namely Snellen's method, it follows that the comparison must be a just one, and as the peripheric linear yielded more favorable results this was the better operation.

From this opinion, prevalent as it is, we

demur, and believe that although the method of testing was the same, yet the standard used was different, and that consequently the conclusions drawn are not only unreliable, but even erroneous, and that, so far as statistics go, it is at least still a question whether the old flap, and not the new peripheric linear, does not give the best results.

To show that this is the case, reference must be made to the statistics themselves.

In 1863, Graefe published the results of 1500 cases of flap extraction.* Out of these he got 65 per cent. of immediate good results, with 15 per cent. additional after a secondary operation, making 80 per cent. as a grand total of perfect success. In these cases "vision of at least $\frac{1}{4}$ " is taken as the standard necessary for a good result. In patients over 75 years, however, $V = \frac{1}{6}$ is allowed. There was a total loss of 6 to 8 per cent.

In a later paper,† however, Graefe gives another series of cases in which he gets 84 per cent. of perfect results, 11 per cent. of half successes, and a total loss of 5 per cent., and he then states that in his private practice the results were even better than this, namely 91 per cent. of perfect results, 6 per cent. of half successes, and only 3 per cent. of absolute loss. 91 per cent. of patients with vision $\frac{1}{4}$ and over, and only 3 per cent. of total loss is a result which we venture to say has never been equalled by any other method.

If we now turn to the peripheric linear, we find at the outset that "Graefe," as Dr. Norris says,‡ "has not given us so extended and full an analytical report of his cases as would be desirable," but he did publish 300 cases (Archiv xii., part 1, p. 151), in which he got 90 per cent. of perfect immediate good results. But here, instead of taking vision $\frac{1}{4}$ as a standard, he took $\frac{1}{6}$, and we have no means of knowing how many cases were included between $V = \frac{1}{4}$ and $V = \frac{1}{6}$. But if, for the sake of calculation, we take the difference between $\frac{1}{4}$ and $\frac{1}{6}$ we must then subtract 9 per cent., which would leave 81 per cent. of cases with vision $\frac{1}{4}$. Graefe then goes on to state that out of the 10 per cent. which comprises total failures and imperfect successes, enough would have been benefited by secondary operations to have given a grand total of 94 per cent. of perfect results. Admitting that this "would have been" the case, we must

* Zehender Klin. Monatsblatt, p. 146.

† Archiv für Ophth., vol. xi., part 3, p. 7, 1865.

‡ American Journal of the Medical Sciences, January, 1871, p. 243.

still take, in order to make the comparison a just one, 9 per cent. away, which would leave a grand total of 84.6 per cent. of perfect results, against 84 of the second and 91 of the third series of flap operations. Thus we see that the balance swings if anything a little in favor of the flap operation. Whether this may be accounted for in the difference between strictly private and a mixed series of patients it is hard to say. But what is plain is that, even in Graefe's hands, the results of each method are so nearly equal that it is still an open question which is the better; or, to say the least, there is no sufficient cause, so far as statistics are concerned, for the now almost universal belief that the linear is so much superior to the flap.

Were this prevalent opinion, however, founded upon Graefe's results alone, little more would need to be said, but inasmuch as it has apparently been confirmed by those of others hardly less skilful than he, it may not be without interest to look at the statistics of these operators, scanty though they be, which have gone so far to produce this general opinion.

Thus Dr. Knapp, who is an ardent supporter of the new operation, has published three series of cases, of 100 each, which are exceedingly interesting and instructive, on account of their fulness of detail in regard to the amount of vision obtained.

In his first series (Archiv xiii., vol. i., p. 120), Dr. Knapp claims 62 per cent perfect results. Here he uses, however, vision $\frac{1}{2}$ as a standard of success. If we apply that used in the flap, we find, on looking at the table, that out of the hundred cases we get only 39 perfect results against Graefe's first and worst series of flaps, which gave 65 per cent. of immediate perfect successes.

Dr. Knapp remarks that among these cases there is 14 per cent. of cases in which vision ranges from $\frac{1}{4}$ to $\frac{2}{3}$, and which are capable of being improved by a secondary operation. Admitting that one half are so improved thereby as to obtain vision equal to $\frac{1}{2}$, and this is a large proportion, we then get as a final result $39 + 7 = 46$ with vision $\frac{1}{2}$ and over.

In making out his second series (Archiv. xiv., vol. i., p. 316), Dr. Knapp says that "he, as Graefe and others have done," divides his cases into three classes, which with their respective numbers in each are as follows:—

Failures,	2
Imperfect results ($V = \frac{1}{2} - \frac{1}{3}$),	12
Perfect " ($V = \frac{1}{2} - \frac{1}{3}$),	86

It will be seen from this that Dr. Knapp

claims 86 cases out of the hundred of immediate good results; but here another change takes place, and instead of taking $V \frac{1}{2}$ or even $\frac{1}{3}$ as a standard, he takes $\frac{1}{4}$, and apparently on the authority of Graefe. This may be the case, but I can nowhere find it stated in Graefe's writing that he has ever used a scale of vision lower than $\frac{1}{2}$ as a perfect result, though it is undoubtedly true, as will be seen a little later, that others have done so, and it strikes me as manifestly unjust and unscientific to maintain the superiority of one operation by statistics in which the standard of excellence used is *two and one half times less than in the other*.

If we look at the table of specific results from which the above classification was made, we see that out of the hundred cases there are only forty in which the vision is $\frac{1}{2}$ and over. That is to say, only 40 per cent. of immediate successes according to the scale for the flap operation.

We also see that there are twelve imperfect results. Now supposing we admit, with Dr. Knapp, that one half of these imperfect results can be made perfect ones, and that in all these vision can be made, not, as he says, $\frac{1}{3}$, but even $\frac{1}{2}$; and supposing we add these cases to the 40 already perfect, we get as a final result $40 + 6 = 46$ cases out of the hundred with $\frac{1}{2}$ and over.

With vision $\frac{1}{4}$, Dr. Knapp makes the final result 93 successes.

In his third series Dr. Knapp has (Knapp's Archiv, v. i. p. 130)—

Failures,	3
Imperfect ($S < \frac{1}{3}$),	15
Perfect ($S > \frac{1}{3}$),	82

By looking at the table of specific results, we find 50 cases of immediate good results. $V = \frac{1}{2}$ and over. Admitting, with Dr. Knapp, that 9 out of the 15 imperfect could be made perfect, and allowing to these cases as before $V = \frac{1}{2}$, not $\frac{1}{3}$, which is all that Dr. Knapp claims, we get as a final result $50 + 9 = 59$ cases with $V \frac{1}{2}$ and over.

Making now a comparison between the final results of Dr. Knapp's series of cases by peripheric linear and Graefe's by the flap, we have, arranged in tabular form, something like the following:—

Knapp, Linear.	Graefe, Flap.
1st series, 46	against 80
2d " 46	" 84
3d " 59	" 91

This averaged gives 35 per cent. in favor of the flap with vision $\frac{1}{2}$ taken as a standard in each case.

It may be objected that in the above calculation we have taken even a higher scale

than did Graefe himself in applying vision $\frac{1}{4}$ to all cases, inasmuch as Graefe made an exception of such as were over 75 years of age, reckoning in these cases $V \frac{1}{4}$ as a perfect result. As an offset to this, it must be remembered that we have allowed to Dr. Knapp 8 cases in the hundred with $V \frac{1}{4}$, when in the first series $V \frac{1}{4}$ and in the remaining two only $V \frac{1}{10}$ was claimed.

It might also be objected to as hardly fair to contrast any other operator, however skilful, with the great father of modern ophthalmology, and the above figures would go a good way toward making such an objection valid, especially as Graefe was working under the disadvantage of what is supposed to be an inferior operation. This impression will, however, be corrected, to a degree at least, when we call to mind that it has always been the custom, especially latterly, to look upon Graefe as a great medical philosopher rather than a skilful and delicate operator, though it would certainly strike one, judging simply from the results which he obtained, that he was, in this, as in all other clinical branches of our specialty, immensely superior to any one of this or any other generation.

Becker published (Zehender vol. v., p. 279, 1867) the statistics of 217 cases (150 operated upon by Prof. Arit) in which $V \frac{1}{10}$ was also taken as a perfect result; inasmuch, however, as no specific details are given as to vision, no satisfactory comparisons can be made from them, except that the final result obtained was only 83 per cent. with $V \frac{1}{10}$ and over.

Dr. Derby, of Boston, a warm advocate of the new operation, has given,* in an analysis of 61 cases of linear extraction, a tabular statement of the vision of 49 patients. Dr. Derby also reckons $V \frac{1}{10}$ and over as a perfect result, and sums up the cases as follows:—

Failures,	3
Partial success ($V \frac{1}{10}$ to $\frac{1}{20}$),	6
Entire " ($V \frac{2}{3}$ to $\frac{1}{10}$),	43

If, however, we reckon an "entire success" $\frac{1}{4}$ instead of $\frac{1}{10}$, we find by referring to the table that there are only 19 such cases instead of 49.

If of the 9 additional unrecorded cases we assume that 5 will have vision of $\frac{1}{4}$ and over, we get as a final result 24 cases of perfect result out of 61 or 39 per cent.

There are, indeed, other statistics by different operators, all of which, however, are compiled with so little exactness or founded upon such various and indefinite scales of measurement as to be of little value for the

purposes of comparison. Still, those which have been cited here are enough to show that the want of uniformity in the standard is a serious embarrassment in our making a just comparison not only between the results obtained by various operators, but even of the true merits of the two methods, and this is the object which we had in view.

And in this connection I would say that I can easily understand how the younger and rising school of ophthalmologists, after having gained their experience in the old method, with all the numerous mistakes and failures incident to beginners in this difficult and exacting technique, should now obtain, after they have become proficient, better results with the linear than they did at the outset with the flap. For it is certainly easier for a practised hand to change slightly the method of operating than for a novice to learn how to operate.

What we should like to see done would be for those who are skilled in both methods, and who have plenty of material, to try them side by side under the same influences and with the same tests. It is only in this way that the two operations can be justly compared. But if these conditions are too hard to fulfil, we might at any rate expect that the advocates of each method might at least conform to the very simple condition of using the same standard of measurement, be this what it may.

It may be said that the one originally taken by Graefe, namely $\frac{1}{4}$, is too high, and that it was probably for this reason that he himself changed it in the linear method. Admitting that this is true, and that $V \frac{1}{4}$ is nearer the mark, the question at once arises, what are we to do for the sake of study and comparison with the immense number of cases carefully recorded by Graefe and tabulated under the scale of $V = \frac{1}{4}$; cases which, from their great number and from the vast amount of instruction which they embody, form one of the most brilliant pages in the whole annals of modern ophthalmology? And if, to suit the progressive spirit of the age in its craving for "perfect results," we go successively from $\frac{1}{4}$ to $\frac{1}{8}$ and to $\frac{1}{10}$, where, may we ask, shall the end be?

I would say further, Mr. President, that these remarks are not in any way meant as a criticism on either operation. Had they been so, reference would have been made to many things which have not even been mentioned, and certainly to the change instituted by Graefe himself from a wound which was entirely in the sclera to one which is two-thirds or more in the cornea; and from the slight are latterly sanctioned

* Boston Med. and Surg. Journal, June 8, 1871.

by Graefe to a gradual increase in the curvature of the section, till, in the hands of many operators both in Europe and this country, the only appreciable difference between the new operation and the flap with a concurrent iridectomy is the difference between the old and the new knife.

The discussion of these points, as well as the final decision as to the merits of the respective operations, I feel had much better be left to those of my colleagues whose experience is richer and whose judgment is riper than my own.

THE RELATIONS BETWEEN HÆMOPTYSIS AND PULMONARY TUBERCULOSIS.

A CLINICAL LECTURE BY PROF. SKODA.

Translated from the *Annales et Bulletin de la Société de Med. de Gand*, for January, 1871.

PROF. NIEMEYER has recently assigned to hæmoptysis an importance entirely unlike that which it formerly was held to possess. He believes that tuberculosis is caused by the hæmoptysis itself, maintaining that the blood arrested in the bronchial tubes and in the air-cells after a hæmorrhage gives rise to a chronic inflammation, and that on this depend the febrile state and the other symptoms of phthisis. If the blood thus retained in the minute bronchi and in the air-cells really possessed such an influence and could excite such an inflammatory state, we ought to expect that the same result would follow hæmorrhages which attend cardiac disease. Now no such condition occurs in the course of that affection. Where an hæmoptysis takes place in patients whom we consider to be tuberculous and who die during the hæmorrhage or soon after, we do not generally find any arrest of accumulated blood in the bronchi and air-cells; while if death occurs after a hæmorrhage in diseased heart, there is found a collection of blood in the lung. The hæmorrhagic infarctus very rarely presents itself after the hæmoptysis of tuberculosis, and is an exceptional occurrence in cardiac disease. But it is this very thing which would determine the conditions of a chronic inflammation! I have never seen such a result. Doubtless, if accumulated blood does remain, a moderate reaction occurs, in the course of which only the normal changes of the blood take place; that is, it coagulates, becomes encysted and forms the infarctus alluded to, but never progresses to suppuration. Such a hæmorrhagic infarctus may last months and years, growing smaller and smaller, and finally disappearing altogether. The blood-globules undergo a metamorphosis

by which the black pigment is the result, or else disappear by fatty degeneration. The fluid elements, which become separated from the rest, are reabsorbed; the dark coloring-matter is left, and if the hæmorrhagic infarctus continues any length of time it remains as black patches in the substance of the lungs. According to this view, then, the observations relative to the effusion of blood in the lungs in the course of disease of the heart accord so little with the theory of Professor Niemeyer, that one is forced to confess that this hypothesis is untenable.

According to the investigations which have been made in the living subject and upon the cadaver, it is very probable that the hæmoptysis which occurs in pulmonary tuberculosis before and during its development, has its seat in the mucous membrane of the bronchi, and not in the air-cells. If the blood came from the latter, it would certainly be very difficult to explain the rare occurrence of the hæmorrhagic infarctus; but since it comes from the bronchial mucous lining, it is easy to see that none remains as a plug, but that it is expelled by coughing. I can state positively that in cases in which death occurs in the course of an hæmoptysis, it is the rare exception to find blood in the bronchial tubes, but that it is found rather in the larynx and the trachea; because, by the cough and the contraction of the bronchi, it is at once drawn forward and expelled.

So, too, I cannot accept the theory that the hæmoptysis may give rise to serious after-effects. Such a result can be only in cases in which the hæmorrhage occurs in a lung tissue already diseased, especially in cavities from which the blood cannot be evacuated; and it is possible that the morbid properties peculiar to the cavities themselves contribute thus to develop a more active irritation. It is, moreover, to be noted that the blood is not specially irritant to the tissues; for example, a hæmorrhage into the subcutaneous tissues after a blow does not produce any marked irritation, as we very well know, but it is generally quickly reabsorbed; so there is no reason for supposing that the blood is so irritant in a tuberculous patient as to favor the farther development of the symptoms of the disease. Nevertheless, I attribute a great importance to hæmoptysis, but only as a symptom indicating that the disease is present, or that it is in process of development.

Another question here presents itself. When directly consequent upon an acute pneumonia, there remains some of the in-