

same time the lower extremities became dropsical, and several blisters made their appearance, one over the sacrum and one of considerable size on each thigh.

Nov. 24th.—The ulcers became very large, and afterwards constantly increased. His appetite was good from the first, and he was able to transact business in all respects as well as when in the enjoyment of all his bodily functions. His suffering was very slight.

I have just learned that Mr. M. died Nov. 26th, having lived 133 days after the receipt of his wound. His last days were passed in another County (Douglas), some forty-seven miles from Denver, where he was attended by Dr. Sperry, who made the *post-mortem* examination.

I very much regret the circumstances that compel me to forward this without the full particulars of the pathological condition of the cord and lungs.

This is one more case added to the list showing that the complete severance of the spinal cord close to the great cerebral mass does not necessarily cause immediate extinction of life. The treatment was about the same as is usually resorted to in such cases, having for its object the relief of pain when necessary, and to keep up the tone of the system if food should fail to meet its wants. No surgical interference was ever attempted, as I was persuaded that it would avail nothing.

REMARKS ON CATARACT.—III.

By EDWARD G. LORING, M.D., New York.

SINCE sending my last paper to the JOURNAL, in which I endeavored to prove by argument that my original statement that Graefe had meant the test of vision $\frac{1}{4}$ to apply to all his cases of cataract was correct, I have discovered that Graefe has himself settled the question in the affirmative beyond a shadow of a doubt. His statements are directly to the point at issue, and are taken from a discussion on cataract at the Ophthalmological Congress held in 1864. (*Zehender*, 1864, p. 343.) Graefe says:—

"It would certainly be very desirable to come to some agreement in regard to the amount of vision. I am at present employed in making out a statistical record of over 1600 cases of flap extraction, in which I have brought the results into four groups:

"1. Complete success; the sharpness of vision amounts at least to $\frac{1}{4}$. The patients can also read the finest print.

"2. The sharpness of vision exceeds $\frac{3}{16}$. The patients can only read the largest print, but are able, if the eccentric vision is proportionately good, under ordinary circumstances, to go about alone.

"3. The patients can count fingers at, at least, one foot.

"4. The patients remain on even a lower grade than the last, and have either a minimum qualitative, or only a quantitative perception of light, or indeed no perception of light at all.

"Perhaps this or some similar classification might be adopted by the Society for the purposes of comparison."

This stamps the fact at once not only that Graefe meant the test to apply to the whole 1600 cases, but that he wished the Society to adopt either the classification which he had used in his own cases or a similar one based on scientific principles, and not to rely on the worthless one of "fine print." Could more overwhelming proof as to Graefe's real intentions be produced? and I only regret that I should have imposed on such as were enthusiastic enough to read them what must now appear as my all too redundant remarks. As to myself, the labor of defending the statistics of one whose word in ophthalmic matters has been generally considered almost as law, has been from the beginning more a pleasure than a task.

This ends, so far as I am concerned, the discussion of this point. But a word of explanation is necessary, as the manner in which I grouped the percentages into series in my last was objected to.

I did this for the following reasons:—first, because Graefe had himself given the rates in these three forms, viz., 80, 84 and 91 per cent., and I wished to keep his figures; secondly, as I had taken the lowest, I thought I was entitled to take also the intermediate and highest to form an average. These three figures averaged give 85 per cent., while that from the pressure bandage alone gives 84 per cent. This difference of 1 per cent. I allowed to remain for my third and principal reason, which was because all my colleagues and those especially who were the immediate disciples of Graefe, and who should have been best acquainted with his works, denied, till confronted with the figures, the existence of such statistics.

Again, in my former paper I took, to be on the safe side, the highest estimate of the linear which I could find, and assumed that "what might have been" really was, and that Graefe did actually get 94 per cent. of success with vision $\frac{1}{4}$. But a later publica-

tion than the one then referred to shows that he actually got only 90.4 per cent.* If, now, we take the difference between the fractions $\frac{1}{4}$ and $\frac{1}{6}$, as in my former paper, and subtract as in that case 9 per cent., we shall then have 81.4 per cent. linear against 84 per cent. of flap. But the fact is, that in order to be within safe limits, we have leaned much too far on the other side, and taken an absurdly low percentage to represent the difference between vision $\frac{1}{4}$ and $\frac{1}{6}$.

The fairest way of getting a correct estimate is manifestly by actual count, and, as Graefe's statistics did not furnish such data, the difference between the fractions was taken as a basis of comparison. But even if we cannot obtain from Graefe's statistics the desired information, we can get a very good and by far the most correct approximation towards the proper number of cases to be subtracted from the tables of others. To do this we will take six tables (all, in fact, to which we had access) where the specific results are given. Two of these tables are taken from the Reports of the Boston Eye and Ear Infirmary, three from Dr. Knapp and one from Dr. Derby.

These six tables give, when averaged, 21 cases per hundred more with vision $\frac{1}{4}$ than $\frac{1}{6}$. If, then, we take 21 from 90.4 we have as a result 69.4 perfect success with the linear against 84 with the flap, with $V = \frac{1}{4}$ in each. But allowing Graefe from superior skill to get 25 per cent.—and this is an enormous concession—less cases with vision $\frac{1}{4}$ than other operators, we still must subtract 16 per cent. from 90.4, which leaves 74.4 linear against 84 of the flap, a difference of 10 per cent. in favor of the latter operation.

Reports of Medical Societies.

BOSTON SOCIETY OF MEDICAL SCIENCES.
EDWARD WIGGLESWORTH, JR., M.D., SECRETARY.

Dec. 5th, 1871.—The Society met at the house of Dr. Ellis, Dr. Ellis in the chair.

Determination of the Amount of Urea in Urine.—Dr. Bowditch showed an apparatus for the quantitative examination of urea in urine by the method proposed by Dr. Hüfner,† namely, the decomposition of urea by subbromite of soda.‡ The nitrogen gas

set free by this decomposition is the measure of the amount of urea contained in the urine. The apparatus in which the decomposition takes place consists of two glass bulbs connected by a glass stop-cock. The lower and smaller of the two bulbs is closed at the end opposite the stop-cock, while the larger one ends in a straight glass tube which is inserted through the bottom of a cup-shaped vessel made from a pint-bottle by breaking off the lower two-thirds.

The urine, diluted with four or five times its volume of distilled water, is placed in the lower bulb, the cubical contents of which must be previously determined by filling it with quicksilver and weighing the amount contained. The stop-cock between the two bulbs is then closed and the upper bulb filled with the reagent, which is prepared as follows. Dissolve 100 grammes of caustic soda in 250 cubic centimetres of distilled water. Wait till the mixture becomes cool, then add 25 cubic centimetres of bromine. Shake and let it stand over night.

The upper bulb and the cup above it being filled with the reagent, a measuring glass graduated to cubic centimetres, also filled with the reagent or with distilled water, is inverted over the upper end of the tube which projects through the bottom of the cup.

The stop-cock between the two bulbs is then opened, and the diluted urine being specifically lighter than the reagent rises and mixes intimately with it. Decomposition of urea takes place at once, and the nitrogen gas set free rises and collects in the measuring glass, where its amount can be determined by the ordinary methods of gas analysis. The quantity of urea contained in the known volume of urine can then be calculated by a simple chemical proportion. Greater accuracy may be attained by warming the apparatus while the reaction is going on. This produces a more complete decomposition of the urea, but the reagent is also decomposed with the liberation of oxygen. The mixed gases must, therefore, be collected over mercury and subjected to a regular analysis.

The great advantage of this method is the rapidity with which the analysis can be performed.

Canceroid of the Oesophagus.—Dr. Fitz showed a specimen of canceroid of the oesophagus, seated at the lower third, not encroaching upon the stomach. Trachea and bronchi also free from disease. The mesenteric glands, at the root of the mesentery, were converted into a mass of diseased

* Klin. Mon., 1868, p. 17.

† Jour. für prakt. Chemie [2], Bd. 3.

‡ Unterbromigsäures Natrium.