ous recorded instances of foreign bodies, such as bullets, etc., being lodged in the brain with subsequent recovery. As an auxiliary to the power of the brain to accommodate itself to the motion of a ship at sea, I must refer to the instinctive act of inspiration, of which I have already spoken, as a great adjunct to relieve the brain from an undue supply of blood.

It must be remembered that recovery from sea-sickness during a voyage, in most cases, takes place after one or two days; by which time the sufferers, now convalescent, have exchanged the short choppy waves of the English channel for the totally different seas of the Atlantic Ocean or the North Sea. Having myself several times crossed the Bay of Biscay, and having been once three months on board a sailing vessel, I am quite aware of the entirely different kind of sea to be met with outside the Channel; and I can conjecture that any relief which took place in the broad swelling waves of the Atlantic would not have been experienced if I had been still pitching about between England and France.

As to my suggestion, I must remark that it was intended to prevent sea-sickness, and not as a remedy to relieve it after it has been once set up. It is offered as a means whereby the action on the sensorium shall never be induced. After that has once taken place, the effect cannot be expected to subside immediately from the mere avoidance of further exciting cause.

I am afraid that some misunderstanding may have arisen from the use of the word "towards" instead of "in the direction of" the bows of the vessel. I could not have meant to indicate the forward part of the vessel as desirable for the recumbent position; because, the centre of oscillation of a ship being the point about which all its parts may be supposed to oscillate, whether in pitching or rolling, it will be in the midships that the least motion will exist; and it is obvious that there, or as near as may be, the berths or sofas should be placed, especially those for the ladies, who, from delicacy of organisation, are the most easily affected.

I must just allude to a suggestion that "precaution as to diet" is of great importance in preventing sea-sickness. Of course, before going on board, any excess or change of usual habits would be obviously inexpedient; but no rules for a particular diet before going on board can possibly be suited to all habits and constitutions: that which would be suitable for strong men would be very ill-adapted for delicate constitutions. There is evidence that the contents of the stomach have very little to do with the sickness, which is secondary only to a disturbance of the sensorium. Vomiting and retching equally take place after the stomach has wholly emptied itself; and this is a distinctive difference between vomiting which arises as a consequence of cerebral disturbance and that from disordered stomach.

ON THE CAUSATION AND SIGNIFICANCE OF THE CHOKED DISC IN INTRACRANIAL DISEASES.

By T. CLIFFORD ALLBUTT, M.A., M.D.Cantab., F.L.S.,
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FEW investigations of late years have excited more interest than those which have been made into the connection of certain changes in the eye with diseases of the central nervous system, and into the additional means of diagnosis which such changes may afford. Much has now been done in England, France, and Germany, to explain these secondary changes and to define them; the result being that they are proved to have an importance which cannot be overlooked, and in many cases to possess a value perhaps greater than that of any other symptom taken alone. No observer of nervous diseases can now dispense with the use of the ophthalmoscope; and the reports of cases in which the instrument was not frequently used must henceforth be regarded as defective in an important particular. Take, for example, a case in which a person having once been infected with syphilis, complains of headache: an ophthalmic examination may not infrequently reveal optic neuritis—a discovery which puts us on our guard against intracranial mischief of a dangerous kind, and likely, if neglected, to result in much graver lesions. Or there may be no history of syphilis; but the patient still complains of headache, occasionally accompanied with vomiting; and for these symptoms, which he calls bilious, he consults a physician. An ophthalmic investigation in such a case not unfrequently results in the discovery of a state of the optic disc known as congestion-papilla, *stauungs-papilla*, or choked disc; and such a discovery would make the diagnosis of encephalic tumour almost a certain inference. The number and importance of cases of this kind are daily increasing so fast, that what was formerly regarded as a chance occurrence is now looked upon as the rule, and the absence of any marked change within the eye in encephalic tumour or meningitis is reported rather in the way of exception. Moreover, it is now known that amaurosis in intracranial or spinal disease is no longer to be regarded as a simple palsy of the optic nerve or nerves; but that it is always, or nearly always, due to a change in the structure of the nerve itself—a change which may partake of the nature of dropsical infiltration, of irritative proliferation or of simple atrophy. It is at present very important to us to know whether there is any constant distinction between these kinds of change, and whether we are at liberty to draw any definite inferences from them respectively as to the nature of the mischief within the head or spine.

In the present paper, I purpose to deal more especially with the distinction which exists in a greater or less degree between optic neuritis proper, or, as it as sometimes called, descending neuritis, on the one hand, and the congestion-papilla, or choked disc, on the other. For anything like an adequate comparison between these two states, I have not space at present; but I may venture to refer the reader to the full description of them, and to the comparison between them, in the volume upon the whole of this subject which I have recently published (*On the Use of the Ophthalmoscope in Diseases of the Nervous System, Kidneys*, etc. Macmillan: 1871). Shortly, however, I may say that at one time we see the optic disc presenting especially a state of venous turgor, with swelling and serous infiltration. The disc is prominent, its outlines are dimmed, and the retinal veins are distended, dark, and tortuous. This state we call the choked, swollen, or congested disc. At another time, we see rather a proliferative than a dropsical process at work. The retinal vessels are now less distended, and they are in great part concealed by new elements, which also extend over and conceal a wide belt of surrounding retina. To this state the term optic neuritis, neuro-retinitis, or descending neuritis, is applied. On *post mortem* examination, the mischief in the first case is found to be limited to the optic discs, or nearly so, while in the second case it extends along the length of the nerves. Von Graefe was among the first to distinguish these two forms of change, and to attribute their difference to a difference in their causation. Briefly, he attributed the former state to increased pressure within the cranium; and the latter he attributed to the extension of a proliferating process at the base of the brain along the connective elements of the optic nerve up to the eye. Hence the term, descending neuritis.

For ten years before the publication of the book which I have mentioned, I was engaged in collecting a great body of experience bearing upon these points, and, on the whole, I was confirmed in the substantial truth of Von Graefe's distinction. The only real difficulty I found is one which others have also experienced; and this is, that in a certain number of cases, and at certain stages of many more, the visible characters of the disc cannot be so distinctly contrasted as they undoubtedly can in the so-called typical cases. Too often we are met by mixed forms which are difficult to interpret, and from which we cannot, therefore, safely infer that the intracranial cause is really increased pressure on the one hand, or meningitis on the other. Nor can we say in such cases that both causes exist, or have existed, together; for it seems certain that a case of the choked disc, however typical at one time, may at another have put on an appearance more like neuro-retinitis—not because any neuritis has "descended" from the encephalon, but because the local papillary congestion has led to local irritation with local proliferation of connective elements. When we are able to watch cases throughout, these mixed forms present less difficulty, for an early examination often reveals a typical choking in the same eye which, at a later date, presents the appearance rather of neuritis. Many cases, however, present themselves to us at one stage only in their course, and for these we still lack the means for any certain distinction. This difficulty apart, Von Graefe's original views have been rather strengthened than weakened by subsequent investigations, including my own which are very numerous. In the work to which I have referred, therefore, I accepted them as the groundwork of my own teaching. Very recently, a hypothesis has been set forth by Benedikt and others, to the effect that the consecutive changes in the optic disc are not due to increased pressure within the head, nor to propagated inflammation from the basal membranes, but to vasomotor irritation or paresis.

This hypothesis seemed to me to be defective in explaining the several orders of facts which we have, and also to fail in throwing any light upon those difficulties which are yet unsolved. For a full survey of the reasons against this vaso-motor hypothesis, and of those which rather support Von Graefe's hypothesis in the face of it, I must agai venture to refer the reader to the volume on *The Ophthalmoscope*, page 119 *et seq.* Leaving neuro-retinitis aside, as the title of this paper requires, I would still urge that the evidence in favour of the production of the *stauungs-papilla* by excessive intracranial pressure is too strong

to be lightly set aside : but as to the mode in which this pressure makes itself felt there is more to say, and it is to this part of the question that I now more especially desire to call attention. Von Graefe's explanation of the mechanism by which the intracranial pressure sets up *stauungs-papilla* was this, that the pressure resolves itself towards the base of the brain, and that it there exercises more or less compression upon the cavernous sinus, and hinders the ebb of the venous blood coming from the eye. Hence the optic nerve becomes congested, and this congestion tells especially upon the intraocular portion of the nerve, as the nerve where it passes through the sclerotic is grasped by an unyielding ring, which ring acts the part of a multiplier.* Thus the disc is engorged, throttled or choked, and it presents a swollen, hyperæmic appearance to the ophthalmoscope. Now, this explanation presents many difficulties even to those who most feel the strength of the evidence in favour of intracranial pressure as the cause of the choked disc ; and it has been felt that the pressure-hypothesis, if it be to withstand criticism, must, in the detail of its action, be explained in some other way : in fact, another explanation has recently been proposed, and the proposer (Professor Manz of Freiburg) has given us his views in full, supported by much evidence, both experimental and clinical, in an able paper published in the last number of the *Deutsches Archiv f. Klinische Medicin* (Ueber Sehnerven-Erkrankung bei Gehirnleiden ; Hydrops Vaginæ N. Optici ; *loc. cit.*, Bd. ix, Hft. 3, s. 539). Not only had the explanation of congestion of the disc by means of a supposed pressure upon the cavernous sinus failed in many pathological cases ; but Sesemann (*Arch. für Anat. und Physiol.*, 1869, 2 Hft., p. 154) has shown, by injection in animals, that compression of the cavernous sinus, when present, does not necessarily cause choking of the disc, as other ways of relief exist. On the other hand, all recent authors admit that choking of the disc, apart from neuro-retinitis proper, does frequently occur, and we have to learn how this is set up.

Now, about this time certain anatomical observations were being made by Schwalbe, Schmidt, and others, to which I alluded in a note on page 57, but which were at the time quite recent, and scarcely verified. These observations are, however, now accepted, and they are held to demonstrate that there exists a lymph-cavity between the outer and the inner sheaths of the optic nerve, which lymph-cavity is continuous with the arachnoid cavity. This lymph-channel is now called the subvaginal cavity ; and Schwalbe's experiments prove that liquids under pressure readily find their way into it from the arachnoid cavity. Thus a pressure of accumulated fluid is liable to be set up around the optic nerve-entrance, where there is a limit to its further progress.† Thus another explanation of pressure upon the disc becomes possible ; and many consider that this possible cause is to be verified by physiological and pathological investigation.

An extreme instance of such a mode of action is to be seen in the record of a case which came under the observation of Manz himself, and which is to be found in the appendix to my work on *The Ophthalmoscope* (p. 319). In this case, Manz found an enormous dropsical swelling of the optic sheath in a case of tubercular meningitis ; and, although he could not but regard it as being of the same nature as the fluid in the arachnoid and in the ventricles, yet, like those accumulations, he regarded it as due to local exudation ("*Autochthoner Hydrops ;*" Hydrops Vaginæ Nervi Optici, Zehender Monatshefte, 1865, p. 281). Ammon likewise was disposed to refer such an exudation to hyperæmia of the internal sheath (*Beitr. zur pathol. Anat. d. intraocularen Schnerv-endes, etc., Arch. f. Ophth.* Bd. vi, Abth. i). Stellwag, again, describes a case of the kind, and attributes the amblyopia to a serous infiltration of the nerve itself. Manz and others, in these cases, had often noticed how wide the outer sheath appeared, and how it seemed wrinkled ; but at first they supposed this to be due to the shrinking of the nerve itself. But he soon found that injections passed quite easily into the "subvaginal space", and so thenceforward he elevated the orbital plates at an early stage of his necropsies, before cutting the nerves themselves or lacerating the encephalic tissues. In this way he was led to discover that the optic sheath was often dropsical with contained fluid ; or in other instances he tied the nerves at the foramina and obtained the same result ; showing that examinations made in the usual way fail to demonstrate dropsy of the nerve-sheath even when this had existed, as the fluid readily escapes on the least laceration. In many cases, again, where the dropsy actually found was but small in quantity, yet the distension of the sheath signified that its quantity had previously been greater. It was noticed, too, that the sheaths themselves were never hyperæmic, but always pale, and often shiny or semi-transparent. These changes always occurred bilaterally. In one case of meningeal hæmorrhage, the sheaths were full of blood, which had evidently made its way into them from the arachnoid.

The further question, whether such exudations penetrate into the optic sheaths from the arachnoid cavity only, or also from the subarachnoid space, is elucidated by some further anatomical investigations of Axel Key,* who finds that the supravaginal space of the eye which communicates anteriorly with the perichoroidal space between the choroid and sclerotic, empties itself backwards also into the arachnoid space. So, also, as we have seen, does the subvaginal space ; while Axel Key says that the spaces between the optic nerve and the inner sheath are one with the subarachnoid spaces of the brain and spinal cord. Near the papilla, however, these spaces surrounding the nerves often unite ; so that we expect to find, and do find, that under certain degrees of pressure free penetration of fluids takes place from both the arachnoid cavity and from the subarachnoid space on the one hand, and, on the other hand, into the space between the two optic sheaths, or into that which lies above this, or again into that which lies below it.† To this we must add, that the subarachnoid space opens a way into the brain-mass itself by the pia mater and the perivascular canals of His. Manz considers that these communications between the arachnoid cavity, subarachnoid space, the inner sheath of the optic nerve, and the subvaginal space, are also borne out by the facts of pathology. All this is full of interest, therefore, for those who are working at the connection of cerebral and ophthalmic disorders. This much seems quite clear, that fluid pressure within the encephalon is quickly transferred along the optic sheaths to the optic entrance ; that fluid thence percolates among the connective elements of the disc (for which it is scarcely necessary to suppose, with Schmidt, that a special system of fine canals exists) ; that in this way the disc becomes swollen, and the venous distension which then follows is thus shown to be not the cause of the swelling of the disc, but the consequence—to be not a primary, but a secondary, event. This is quite in accordance with my own experience; for I have seen in tubercular meningitis that venous fulness may precede any swelling of the disc, or may occur without any such consequence, in which cases it may be due to early and invisible tubercle of the choroid ; or, on the other hand, the disc may become suffused, and even swollen, without venous dilatation preceding it. Slight degrees of suffusion, without much venous fulness, are indeed far from uncommon, especially in tubercular meningitis. The equable pressure of fluids within the cranium accounts for the equal advance of the choked disc in both eyes, in which respect this sign differs from descending neuritis, which rarely moves by equal degrees in the two eyes.

As regards the intracranial disorders in which the choked disc is found, Manz has always found evidences, past or present, of increased intracranial pressure ; but there seems to be a definite relation between the amount of intracranial fluid and the nerve-dropsy. He sees evidence of past pressure in flattened convolutions or in distended ventricles. In some cases, Manz thinks, an "autochthonous dropsy" of the nerve-sheath may be set up by extension thither of the meningitic process. On the whole, however, he regards this as unimportant, and attributes choking of the disc almost exclusively to the transference of fluid under pressure. He even goes further than this, and believes that he has several times seen the swollen disc recede with relief of other symptoms of pressure ; and, on the other hand, has seen the disc become prominent as these symptoms, such as sopor, etc., reappeared or set in for the first time. Further observations on this most interesting point are required.

One point only remains—to inquire whether neuro-retinitis is wholly different in its causation and mode of occurrence from the *stauungs-papilla*, or whether there is any relation between the two. Professor Manz is evidently disposed to believe that some relation exists between the two processes, though he is too cautious to commit himself to any definite statement. He refers, however, to the tendency he has noticed in the epithelial cells of the lymph-spaces to irritative proliferation during the period of dropsical distension. This proliferation, which seems to resemble that noticed by Ranvier (*Compt. Rend.*, 1871) in dropsy of cellular tissues elsewhere, may perhaps be a good explanation of the mixed forms, but can scarcely account for the marked forms of primary neuro-retinitis which after death are found to occupy much or all of the nerve-length, and which, as I have shown in the treatise to which I have made reference, coexist so often with such irritative conditions

* In a remarkably able and generous article on my work on the Ophthalmoscope, in the April number of the *Medico-Chirurgical Review*, among many other very valuable criticisms, the author questions the importance of the part played by the sclerotic ring, and suggests that equalised pressure, producing its greatest effect in the direction of least resistance, suffices to explain the prominence of the disc.

† An excellent account, by Schwalbe, of the three systems of lymph-channels and cavities in the eye, is contained in the last number of Stricker's *Handbuch v. d. Geweben.*

* Axel Key's observations are taken by Manz from a quotation of them in the *Centralblatt*, 1871, No. 33. They originally appeared in the *Nord. Med. Archiv.*

† In cases of the choked disc, the optic nerve, if hardened, will generally show that the inner sheath is more or less detached from the nerve itself.

of the base of the brain as syphilitic meningitis. Nor do I remember ever seeing a well-marked swollen disc pass into a typical form of neuro-retinitis. At present, then, while accepting the valuable and perhaps conclusive evidence of Professor Manz concerning the genesis of the choked disc, I think, on the other hand, that the causation of "descending neuritis" has as yet to be fully demonstrated.

PRACTICAL OBSERVATIONS IN OPHTHALMIC AND AURAL SURGERY.

By ERNEST HART, Esq.,
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III.—The Uses of Atropia.

In this short series of papers, I have already spoken of several common causes of error and misfortune in the treatment of ophthalmic diseases and injuries, based upon the examination of a decenniad of cases; and have especially referred to consequences of superficial examination of cases which resist manipulation, and of a slowness to appreciate the characters of, and enter upon the appropriate treatment of, glaucoma. I believe it will be useful, further, to group together, for the sake of emphasis, a numerous class of cases in which I have seen mischief arise from slowness to employ atropia locally. Looking through a long list of cases, I find that I have employed atropia-drops in the course of the treatment of 1,523 out of 2,710 consecutive cases of disease and injury of the eye (a list not including, of course, affections of the ophthalmic appendages); and I pass in review a long array of cases in which I had to regret that it had not been used before the patients came into my hands. Two of the most recent consultations which I have entered, were cases of injury to the eye, respectively at Paddockhurst and at Brighton, during the last month. In both cases, the treatment had been unexceptionable, except that atropia had not been used; and, though the globes have been saved, the fixed and closed irides will make the possibilities of vision depend upon iridectomy under not very favourable circumstances. In the very last consultation on a case of iritis, of which I have notes, the treatment had equally been conducted without dilating the pupil. Mercury, leeches, and lotions were in use; a darkened room and an antiphlogistic diet were all in use when I was called; but the vision was extremely misty, the pupil contracted, and the patient in proportionate danger of "false pupil" from the effusion of lymph, and of loss of sight.

Now I do not think that I shall be using an exaggerated form of expression, or going beyond the strict and well-balanced weight of words which is necessary to give due force to the fact to be conveyed, if I say that we could, in the treatment of ophthalmic disease, better afford at this day, so far as our knowledge of disease and means of mastering it extend, to dispense with all other drugs, lotions, and applications put together, than with this one *topical* medicament. Let us consider what atropine does for the inflamed eye. It allays local sensitiveness, and removes local spasm; it gives to the eye and to its internal muscular apparatus—iris and ciliary muscle—physiological rest, the greatest of all curative means. Nor does it do this only, but, in dilating the pupillary aperture, it drives far from us the bugbear which long haunted the ophthalmic surgeon, and which still pursues those who are not sedulously active in the use of atropia—closure of the contracted pupil by an adherent plug of lymph, and gluing of the uveal surface of the iris to the lens. It would rob the consulting-surgeon of a great many profitable but trying operations, if the atropine eye-drops were ready in every surgery, not only on all emergencies, but for the exigencies of daily practice. It is as safe a rule in ophthalmic practice to use an atropine drop when in doubt, as in whist to play a trump. I can hardly think of more than one absolute contraindication, and that is the existing oval dilatation of the iris in glaucoma. There are, of course, also mechanical contraindications, as in peripheral wounds of the cornea with hernia of the iris, where to dilate the iris is to increase the peripheral protrusion; but even here, the moment the corneal gap is healed, atropia becomes of the first necessity. But in all cases of iritis, in contusions and injuries of the eye, in corneitis, purulent ophthalmia, scrofulous ophthalmia, and deep-seated mixed inflammations of the eye, the local instillation of a solution of atropia is the most precious of therapeutic means. The most useful strength is, I think, expressed in the formula: Neutral sulphate of atropia, two grains; glycerine, five drops; distilled water, an ounce. The frequency of the use of the drop must vary with the facility and rapidity with which it acts. Where the iris has become much inflamed before the local treatment is adopted, it is sometimes very indocile, slow to respond, and hard to dilate. Then the measure of frequency must be the amount of resistance, and perseverance must be the rule of treatment. In the treatment of keratitis and minor cases of deeper inflammations, one application a day, or at most two, will suffice; and presently, once in two or three days. It will be enough then to keep the pupil dilated, and the ciliary apparatus at rest and free from tormenting spasm. The present result of the most careful observation of the origin and cause of ophthalmic disease, pursued with the advantages of the improved methods of optical diagnosis now at our command, is to simplify our treatment, and to ostracise a majority of superfluous agents of medication. With a little cotton-wool, alum, and glycerine, hot and cold water and atropine, and a pocket-case of instruments, we can treat with a previously unattainable success nearly the whole range of ophthalmic diseases. I am speaking of local and surgical treatment. Ointments, poultices, caustics, irritants, scarifications, venesections, blisterings, and setons, may be looked upon almost as things of the past. The medical, dietetic, and hygienic treatment are favourably modified, and simplified to an almost equal extent, by the intelligent study of diathetic indications; but the whole shopful of topical applications may be left aside by the surgeon who will thoughtfully apply the vast resources of the few simple agents I have named; and of them all, atropine is greatest. I will not undertake to say that mercury is useless in the treatment of some forms of (syphilitic) iritis; but I will affirm that I have repeatedly seen iritis occur and run a very severe course in patients previously and at the time already under the influence of mercury; and that, in a long series of cases which I treated by atropia and careful dieting only, and without mercury, during five years at St. Mary's Hospital, the results were so excellent that I could not affirm that they would have been improved by the most guarded and judicious use of mercury. A number of these cases were seen by my colleague Mr. Gascoyen, who took part in the experiment.

It is possible, though not easy, to abuse atropine. It must not be used, as I have said, in glaucoma or in peripheral wounds of the cornea. A case or two of troublesome constitutional symptoms, through absorption of the excess by the lacrymal mucous membrane, have been recorded. This is never likely to occur with ordinary care, and I have never seen it occur; but it may be well to bear it in mind in treating infants. The most convenient and unfailing application may be made by the use of the atropised gelatine discs which I introduced a few years since, and which are now largely used in this country and abroad. They are always ready, do not spoil by time, and are clean and precisely dosed, each disc containing as much as a drop of the solution I have mentioned.

I do not think I have spoken too strongly of the advantages in practice of a free use of the local instillation of atropine. In cases of doubt, it will often throw much light upon an obscure condition; and those not much given to study ophthalmic cases will be presently surprised to find how often it will, in a dubious case, first display, and then rectify after a few days, recent and unsuspected adhesions of the iris. In the average run of ophthalmic cases, whether for the treatment of a large proportion of the inflammatory disorders of the eye, by securing anæsthesia and physiological rest, or in the diagnosis of optical defects, it is of ever-recurring use. Few things would tend to curtail the extent and to increase the success of consulting practice, more than a judicious prodigality by the general practitioner in the local use of atropine.

ON TAPPING IN PLEURISY AND EMPYEMA.

By BERKELEY HILL, M.B., F.R.C.S.,
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The discussion between Drs. Fuller, Playfair, and Douglas Powell, on the importance of tapping effusions in the pleura, whether serous or purulent, has been most instructive, and leads one to hope that, ere long, this mode of treatment will become habitual in the early stages, instead of being reserved for a last resource to remove hectic fever and exhaustion, or to ward off approaching suffocation. Having taken great interest in treatment by tapping, and having, through the kindness of my medical colleagues at University College Hospital, had several opportunities of treating extremely severe cases, more especially where air was unavoidably admitted to the pleura, I may perhaps be allowed to make one or two remarks on the subject. The third volume of the Clinical Society's *Transactions* contains a narrative of some cases under my care, and a brief *exposé* of the arguments for and against withdrawal of the effusion by tapping. In this I endeavoured to show that, in serous pleurisies, after pyrexia has subsided, early evacuation, without waiting for the effect of blisters, iodine, etc., is advantageous: 1. Because fresh formation of fluid does not always follow