at least equally certain that, although we may by such measures relieve the discomforts that attend an ordinary catarrh, or assuage the pain of a rheumatic joint, or of an inflamed serous membrane, we can in no sense arrest the progress of the general malady by effecting these local benefits. And surely it is beyond all doubt that we cannot thus cure, or even aid in the cure of, deep-seated inflammatory processes; those, for example, which take place in the substance of the lungs, or of the heart, or of the brain, or of the liver.

Again, it is unquestionable that we may, by remedies given internally, assuage many of the symptoms under which patients who are suffering from inflammation labour, and under some circumstances cure the patient or his disease. We may thus by opium, or other narcotics, relieve pain, soothe irritability, and give sleep; and no one will deny the value of these effects in the treatment of acute rheumatism, of inflammation of serous membranes, of carbuncle, of ear-ache, and of numberless other like affections; and we may by the cold bath, or by digitalis, or by aconite, reduce temperature, and there is good reason to believe that such reduction of temperature is occasionally of the highest importance in reference to the preservation of life; or we may even in certain inflammations, as psoriasis, which have something of a specific character about them, effect a cure, at all events a temporary cure, by medicines.

But it is worth while to ask those who believe in the general curability of internal inflammations by the administration of drugs, how many external inflammations are obviously benefited by them. To adduce, in the first place, some of the inflammations which I have spoken of over and over again: Who has seen a boil or a carbuncle, herpes zoster, or erythema nodosum cut short in its progress by means of drugs? Who trusts to internal remedies for the cure of a chilblain or of a whitlow, of a housemaid's-knee, or a group of inflamed lymphatic glands? and who, I think I may venture to ask, has seen erysipelas subside under the obvious influence of some special drug? Yet if so many inflammations which lie under our eyes are manifestly uninfluenced in their progress by internal remedies, and if others of them, which we have been apt to think we can cure by such means, turn out on closer scrutiny to be doubtfully influenced by them, that surely supplies a strong argument against the general curability of internal inflammations.

I fear I shall hardly have conveyed my real meaning if I fail to explain what it is that I understand by the word "cure" in reference to inflammation. Inflammation consists in a certain group of phenomena taking their origin from some special exciting cause. This cause may be exposure to weather, or some irritant acting locally or through the system; the resulting phenomena include congestion and proliferation of cells, which may go on to suppuration, ulceration, and gangrene, and restoration to health by the cessation of these processes, and the absorption or removal in other ways of the products of inflammation. Further, inflammatory diseases have, unlike the exanthemata, a remarkable tendency to recur on the slightest provocation; and undoubtedly their course and severity are largely influenced by the condition of health, or by constitutional peculiarities of the individual attacked.

Now I assume that, when a patient contracts pneumonia, or rheumatism, or erysipelas from exposure to atmospheric influences, the disease may be kept up or increased, and thus soon rendered fatal, if such exposure be continued or repeated; and I assume, on the other hand, that by the adoption of suitable hygienic measures such maintenance or growth of the disease by successive increments may be prevented. Again, I assume that, if two persons who have contracted either of the above diseases be at once put under the same hygienic and remedial treatment, the disease will probably take a different course in each, that difference being dependent either upon the degree in which the primary cause operated in each case, or on differences of antecedent health. Again, I assume that, if the disease be severe, the patient's life may, on account of this severity, be in danger or lost; but that death is due, in many cases, less to the direct influence of the inflammatory process, than to certain secondary complications or accidents which may be arrested, or obviated, or cured. Thus, long-continued want of sleep, or deprivation of food, or excessive temperature, may each of them prove the immediate cause of death; or in erysipelas, œdema of the larynx; or in rheumatism, compression of the heart by excessive accumulation of pericardial fluid. And further, I assume that, when an abscess forms, or gangrene takes place, these may be directly benefited or cured, if near the surface, by local measures. Now, in all these different points there can be no doubt that we have it in our power to influence very materially the course not only of the three diseases which have just been named, but that of many other inflammatory affections. If we do not cure the patient, we have it at least in our power to keep him from exposure, to maintain or to improve (within certain limits) his general health, and to relieve

symptoms and complications which distress or threaten. And thus it may be admitted that we can, by the judicious exercise of our art, give relief, hasten convalescence, and save life, and thus, in a loose sense, cure the disease. But what I understand by the cure of disease is, the stamping of it out—the bringing it to an end before its time. Inflammation has a tendency to take a certain course, and then to die out spontaneously; and unless in any case this normal duration be anticipated by our treatment, we fail, as it seems to me, to cure the disease; and it is in this sense that I doubt whether we have any curative influence over many inflammatory affections.

OBSERVATIONS

ON

SEA-SICKNESS, AND ON SOME OF THE MEANS OF PREVENTING IT.

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II.

SOME remarks which I published lately through the medium of the JOURNAL were not intended to be an exhaustive statement of my views on the subject of sea-sickness. I propose, therefore, to consider a few points which I intentionally postponed for a future occasion; and I may at the same time briefly allude to certain objections, which, though courteously offered, have, as I believe, been made without a sufficient apprehension of my view of the subject, and of the scientific basis on which it is founded.

The chief points which I omitted to consider are, the immunity of certain individuals from sea-sickness, and the relief which is usually experienced after a certain time spent at sea. But, before entering on these special topics, I wish to enlarge a little further on the connexion of sickness and vomiting with brain-disturbance. It is well known to pathologists that, of all the causes which act sympathetically to induce vomiting, affections of the head are the most common: nothing can affect the sensorial functions without in some way disturbing the stomach. The experience of all practical men testifies constantly to this; but if reference to published authority be desired, I may quote undeniable testimony in a valuable paper in the *Transactions* of the Royal Medical and Chirurgical Society, by Sir Benjamin Brodie (vol. xiv, p. 339). In treating of concussion of the brain, he says that "sickness and vomiting, for the most part, are early symptoms". Now there can be no doubt that the effect on the brain produced by the blood during the descent of the ship is of the character of a slight concussion. It is in the less severe forms of concussion that sickness and vomiting most certainly occur. In the graver forms of head-affection, such as apoplexy, it is not so often found, because it requires a certain degree of excitability of the brain to induce the act of vomiting, and that excitability has been destroyed by extreme pressure.

I pass now to the immunity of certain individuals from attacks of sea-sickness. This at first sight seems an anomaly, and presents a difficulty in accepting the theory which I propose, as well as every other theory which may be brought forward. I can only suggest, as a solution of the difficulty, that there are constitutions so formed as to be very slightly subject to sensorial impressions. The same inequality subsists between different individuals in their capacity of being emotionally affected: some are unmoved by the most distressing subjects; some have their feelings easily excited. It must be borne in mind that the argument as to exceptional immunity is equally applicable to all methods of explaining the existence and causes of sea-sickness.

In regard to the recovery from sea-sickness, there is much to be said without abandoning the theory of the original cause. A wonderful and instinctive power of accommodation to circumstances is possessed by the human frame. In the course of time, the sensorium is able to adapt itself to unusual circumstances; and on this point again we may refer to the same paper by Sir Benjamin Brodie, who, to his statement, "that sickness and vomiting are early symptoms", adds "that they seldom continue after the patient has recovered from the first shock of the accident". I may add to this, that, when organic change has taken place in the brain, as in the presence of a clot of blood, from the rupture of a vessel, there is, on reaction taking place, a cessation from sickness and vomiting.

That the brain has an extraordinary power of adaptation to circumstances, is evidenced by recovery from hemiplegia, as well as by numer-

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.

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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 is 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 again 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