REMARKS

ON

THE DIAGNOSIS AND TREATMENT
OF DISEASES OF THE BRAIN.*Delivered at a Meeting of the Worcestershire and Herefordshire,
Bath and Bristol, and Gloucestershire Branches.*By J. HUGHLINGS JACKSON M.D., F.R.C.P., LL.D., F.R.S.,
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It is a very great honour to be asked by the President of this important Branch of our great Medical Association to deliver an address. It is a particularly pleasant thing to deliver it at the request of my friend, Dr. Currie. The only other preliminary is to ask you to believe that, poor as the address may be, I have taken very great pains in the preparation of it.

For some years I have urged, and I hope to have opportunities of urging it again and again, that for the scientific study of nervous (and of all other) diseases, we should investigate and classify on the principle of Evolution, that we should consider them as reversals of evolution, in other words, as dissolutions, a term Spencer has employed for at least twenty years as the antithesis of evolution. But I have urged equally strongly that we should not follow this plan for direct practical purposes, but that we should for these purposes have empirical arrangements, arrangements of cases by Type. (In no part of this address do I use the term scientific as implying superiority, nor do I ever use the term empirical with its conventional evil connotations.) I have particular reasons for speaking of the two different classifications, the empirical being properly merely an arrangement.

I have been supposed to put forward the principle of evolution as of value in the classification of cases for practical purposes. I have been asked to go into an asylum and show how the cases of patients in it could be classified on that principle. But what I really said was "the classification [on the principle of evolution], valuable as a means of extending our knowledge would be useless, or of little use, for direct practical purposes" (Syphilitic Affections of the Nervous System, *Journal of Mental Science*, July, 1875). I wish to urge both methods, and can do so without inconsistency. A man as a biologist classifies plants one way in his botanical garden for scientific purposes, but he arranges them as everybody else does in his common garden, on the plan most convenient for practical purposes. Moxon said long ago (Introductory Address, Guy's Hospital, 1868-9): "You must know diseases, not as the zoologist knows his species, and his genera, and his orders, by descriptions of comparative characters, but as the hunter knows his lions and tigers." I thoroughly agree with this in so far that we should try to know diseases as the hunter knows his lions and tigers, but I think that we should endeavour to know them also as the zoologist knows his species, etc.

Whilst as a physician I study this and that particular disease empirically, as it approaches or reaches a clinical type, I keep, as an evolutionist, a line of thought on the comparative study, on the homology of diseases. For example, I study miners' nystagmus, writers' cramp, and dancers' cramp, and all other "professional or occupation spasms," separately and quite empirically; and yet, without in any way overlooking their vast differences, I also think of each of them as a member of one homologous series, as being a disease (having the same pathology) of a different part of the same system, of a different motor centre on the lowest level of evolution. (*Vide infra*.) Whilst I should, for medical care of patients, for their safety and their treatment, arrange cases of insanity in any good empirical way I could find, I should also endeavour to classify them as they are different degrees of dissolution of different regions of the highest level of evolution (highest cerebral centres), and should seek their homologues in diseases of the lower levels. Such a comparative study of diseases, that on the wide basis supplied to us by evolution, is of great indirect value for practical purposes. There is but one process of evolution, just as there is but one law of gravitation. Evolution, popularly supposed to apply only to biology, applies to astronomy, biology, psychology (or rather, to anatomy and physiology as

corresponding to psychology), and to sociology; or, to use Huxley's words, to the "whole physical cosmos." It would then be very marvellous if it did not apply to the whole nervous system and all its diseases. I believe that it simplifies our empirical studies, and gives harmony to what, without it, are crude heaps of facts. Let no one fear that the study of diseases as dissolutions will take him out of the practical field. Empirical work must come first; it is necessary for daily practice, it is forced on us. The scientific, the comparative study comes later. Before we can make rational generalisations we must have made empirical observations and generalisations. On leaving this part of my subject I give a quotation from Spencer (*Data of Ethics*, p. 61): "Every science begins by accumulating observations, and presently generalises these empirically; but only when it reaches the stage at which its empirical generalisations are included in a rational generalisation does it become developed science." I submit that on the principle of evolution we have the widest basis for rational generalisations of diseases. I submit that the same principles apply to the symptomatology of a case of paralysis of an external rectus, the simplest, and to insanity, the most complex of all nervous diseases; both are flaws, but each in a very different part of an evolutionary whole. I hope that in time we shall be able to apply the doctrine of evolution for direct practical purposes. I shall now and then try to show how our empirical studies may be made less empirical by availing ourselves of this doctrine.

I now take up the subject of the study of diseases by Type, urging once more that, for practical purposes, we should know diseases "as the hunter knows his lions and tigers." An illustration is easily given, and I shall, I hope, be excused for giving one that does not come under the title of my address. When anyone speaks of a case as one of tabes "complicated" with ophthalmoplegia externa or with glycosuria, he is tacitly asserting that there is a type, tabes, one with a complication. On the clinical nomenclature of the set of diseases of which tabes is one, and on the empirical arrangement it implies, I have some criticisms to make. Our knowledge of these diseases by "cases" is becoming so minute and extensive that our present nomenclature and our empirical arrangements by type almost break down. We have types, and then these types become so complicated that they almost cease to be typical. Beginning with locomotor ataxy, we have at length tabes with an almost universal symptomatology—pupillary affections of several kinds, optic atrophy, gastric crises, bladder troubles, glycosuria, lightning pains, etc. A case of tabes may, indeed, be looked on as a series of different diseases, provisionally for the thorough investigation of each, before we consider them together as a disease owing to the same kind of morbid changes in different parts of the lowest level of evolution of one patient.

Nowadays ataxy is not considered to be a necessary symptom of tabes; when present, it is a subordinate, although a very striking one; it is a less serious trouble than tabetic amaurosis, and is far less serious to life than the bladder disorder in this disease. Frequently cases of what are called tabes are reported as being complicated with symptoms from other types, progressive muscular atrophy, lateral sclerosis, etc. There is what Gowers well calls "ataxic paraplegia." We have under the name of Friedreich's disease a series of cases which at one extreme merges into tabes, and at the other into disseminated sclerosis.¹ These remarks show that the set of diseases spoken of are studied by type; one hears such expressions as "a typical case of tabes," this expression implying that there are also atypical cases (subtypical and super-typical). They show, too, that the types are arbitrary. Some may say that they dislike arbitrary divisions; but when we cannot distinguish, we must, for purposes of practice, divide; even in purest science arbitrary definitions are necessary; the mathematician's line has no breadth.

Fully believing that we must continue to study diseases by type, I wish some one with great authority would endeavour to define our types of the "progressive" diseases involving the spinal system more narrowly. I submit a plan which will, I hope, help us to a less empirical arrangement of some of these cases. Indeed, I think we may, aided by the doctrine of evolution, make a beginning of a rational generalisation of them which will serve us in practical ways.

Were I writing a book on diseases of the nervous system, I should not have in it distinct sections on diseases of the spinal cord, medulla oblongata, and pons Varolii, but one section including diseases of all these parts, the three morphological divi-

¹ See a valuable paper by Dr. Judson Bury, *Brain*, July, 1886.

sions being considered together as making up one anatomico-physiological unity. This unity I have called the lowest level of evolution of the central nervous system; it is, I think, pretty much what Marshall Hall named the true spinal system. It is a division of the nervous system complete in itself, in the sense that it represents all parts of the body. It is to a large extent complete by itself; the organic centres on this level are considerably, if not in some cases quite, independent of the higher levels.²

A case of bulbar paralysis "complicated" with ordinary progressive muscular atrophy is, so to put it, half in and half out of a section on diseases of the spinal cord; it is well within one on diseases of the lowest level, and, considered as a disease of several motor centres of the same series of this level, is not "complicated." Whilst the anatomist speaks of segments (sensori-motor), we, as medical men, must speak of centres motory and sensory of this level, because, for example, disease may "attack" the motor elements alone of segments. Taking the spinal anterior horns and their higher homologues (nuclei of motor cranial nerves) as making up one homologous series of lowest motor centres all along the lowest level, we have an homologous series—I do not say as yet a complete one—of progressive muscular atrophies, from ophthalmoplegia externa downwards, each owing to atrophy of cells of different lowest motor centres.³ (I have already spoken of another homologous series of "lowest level diseases," that is, different "professional spasms.")

Some diseases are spoken of as diseases of the spinal cord when their symptomatology is of far wider origin. Tabes, often enough with a symptomatology from morbid pupillary affection to bladder trouble, is not a disease of the spinal cord only, but a disease of many different parts of the whole level. Taking many cases, we may say without much exaggeration that this clinical entity, tabes, has a universal symptomatology. Further, tabes, which pays no respect to the morphological divisions, in most cases "keeps to" the level and to nerves given off by its centres; although here again we have sometimes "complications" with symptoms of general paralysis (disease of the highest level).

The plan I suggest is different from a mere grouping of the three morphological divisions. Thus the deep part of the lateral column in the cord is just as much out of the level (extrinsic upwards) as are the motor nerve-fibres from the anterior horns going on to the anterior roots (extrinsic downwards); so that lateral sclerosis—I speak of it as far as it is limited to the deep part of the lateral column—whilst it is a "disease of the cord," is not a disease of the lowest level of all, but of part of a strand of fibres connecting some lowest motor centres and all middle motor centres (so-called "motor region" of the cortex). In the same way of speaking hemiplegia from lesion of the internal capsule, since it is also owing to (a different kind of) lesion of the pyramidal tract, is not strictly a cerebral disease, but is one of a continuation of the very same strand, morbidly affected in lateral sclerosis, of a plexus inter-connecting all lowest and all middle motor centres. I will speak of such fibres, although integral parts of the cord, medulla, and pons, as being—if you will pardon the awkwardness of the expression—"extrinsic fibres of the level" (not really of it, but connected with it). In the same way fibres in the cord during their long course from the great vasomotor centre and from pupillary centres, are out of the level (extrinsic downwards) as certainly as their continuations after their emergence from the cord are. The really intrinsic fibres, those properly of the level itself, are fibres inter-connecting its several centres, across and up and down.

Again, if we limit attention to one of the three morphological divisions we cannot work our symptomatology of some of the diseases of that one division. I give an example: complete transverse lesion of the lower cervical cord, say on fracture dislocation of the spine. This lesion pays no respect whatever to evolutionary distinctions; the symptoms are many and various, and careful analysis is required, even disregarding evolution. There is cutting across of intrinsic elements of the level, and of elements of it which are extrinsic upwards and downwards. Some symptoms, owing to lesion of the intrinsic elements of the level, demand for their explanation a very particular consideration of activities of healthy parts above the cord, as well as of the part of the cord

below the lesion; we have to consider the very local lesion of the cord as a flaw in one small part of the whole level. Let us speak only of the symptom constipation. We must first consider normal defaecation, or rather the normal state of the nervous arrangements on the lowest level, for that operation. No doubt the higher levels have influence on these nervous arrangements of the lowest level as in preventing or delaying defaecation, and also in assisting in the operation, partly by putting in action abdominal and other accessory muscles; this implies that in the transverse lesion spoken of there is cutting across of (inhibitory and motor) fibres, which are extrinsic upwards. I here, as my object is only to illustrate one thing, confine attention to elements of nervous arrangements concerned in defaecation which are undoubtedly intrinsic parts of the level, and to the visceral parts of the nervous arrangements for that operation. These elements, in three morphological divisions, are all within the level, and are found in three regions of it (sacral, dorsal, and bulbar). We shall neglect Stilling's (sacral nucleus) which is for the hind gut (and for other parts we are not concerned with) in this illustration. There is a motor (viscero-motor) centre in the medulla, and a controlling (viscero-inhibitory) centre in the dorsal region of the cord. From the former pass polio-enteric fibres by way of the vagus, to make the intestines contract, and from the latter leucenteric fibres to keep them dilated; both sets of fibres are fibres of the level extrinsic downwards—both in their short course before their emergence from the medulla and cord and afterwards. But we must suppose that there are intrinsic fibres, fibres in and belonging to the level itself, connecting the two centres bulbar and dorsal; by these in quiescence there is amicable antagonism between the bulbar and dorsal centres. When the operation of defaecation is started by some afferent impulse, the medulla centre, we shall suppose, inhibits the inhibitory splanchnic centre by the intrinsic fibres, and acts positively by the extrinsic fibres (in the vagus) on the intestine. Thus there is, positively, forcing, and there is, negatively, yielding. The constipation is, in the case instanced, owing to cutting across of the intrinsic fibres between the bulbar and dorsal centres; there is no longer amicable antagonism; each centre "pursues its private ends;" the inhibitory dorsal centre over-acts and over-antagonises the motor bulbar centre.

The plan suggested—that of studying diseases of the cord and of the other morphological divisions as diseases of the level—cannot be carried out fully, because our anatomico-physiological knowledge of its elements is not as yet complete. But I hope that besides looking on a case of tabes, to take that example, as a type often "complicated," we shall soon regard it as a disease of the level, and that we shall have a rational formula of cases of the class of diseases to which tabes belongs and a nomenclature corresponding—a nomenclature analogous to that in chemistry, one telling us in the name of this or that patient's disease, the several particular elements of the lowest level morbidly affected, and declaring what other symptoms in this or that case are owing to lesion of extrinsic elements of, so to say, the level. I hope we shall have names somewhat after the style of "amyotrophic lateral sclerosis;" I say somewhat, because this name is partly after a symptom, or peripheral condition at least, and partly after a central lesion. In this disease one of the lesions, atrophy of cells of anterior horns, is of intrinsic elements of the level, some of its motor centres; the sclerosis of fibres of the lateral cortex is of part of a plexus between some lowest and the middle motor centres. After this somewhat of a digression I return to studies by type.

The next illustration is by a cerebral disease. Epilepsy (I mean epilepsy proper) is nowadays considered by most medical men to be a disease of the brain. We should have two types of this morbid affection. I find, however, that many medical men have but one type—loss of consciousness with severe universal convulsion. But the great thing in the diagnosis of epilepsy is not the "quantity" of the symptomatology, but paroxysmalness. A sudden transitory loss of consciousness with very little physical manifestation, often enough suffices for the diagnosis of epilepsy. It may be said that there are "all degrees" of epileptic seizures,

² I refer here to the organic centres, as they do menial work, regulating digestion, circulation, and respiration. These centres have a large share in emotional manifestations, and are, as far as serving, quite subordinate to the highest centres.

³ As "lowest centre" is used as a proper name to signify evolutionary rank in contrast to centres on the middle and highest levels of evolution, it is correct to speak of more than one lowest centre.

⁴ The law of continuity—that things increase and decrease by insensible gradations—is, I think, in many medical matters applied without due thought. I never nowadays use the expression "all degrees" in relation to any cases of nervous disease. Slight fits of epilepsy, *les petits maux*, do not pass into severe fits, *les grands maux*, by there occurring fits of all or even of many, intermediate degrees between slightest and most severe. There are not only simple degrees, but compound degrees of increase and decrease in cases of disease of the nervous system. Increasing evolution is not a process of increase by insensible gradations; there are, as the expression "levels of evolution" implies, occasional "stoppages," which are "re-beginnings." The division into *le petit mal* and *le grand mal*, although I speak of these two degrees in the text as types,

from slightest to severest. My experience is that a patient, long subject to *les petits maux* only, has a very severe, perhaps a severest, fit some one unfortunate day, and afterwards is subject to them, and often to the slight fits too, without having fits of "all degrees." The two things, *les petits maux* and *les grands maux*, differ in compound degree; they deserve to be considered separately as types for diagnostic purposes. That epileptic fits are often very slight and transient is so very old a story that I will speak of one very important variety of *le petit mal* to which very little attention has been paid. I must premise that loss of consciousness is not essential for the diagnosis of a slight fit of epilepsy; there may be defect of consciousness only.

There are cases of very slight epileptic fits in which occurs what is commonly called an "intellectual aura." I call it "dreamy state." One variety of it is the feeling of "reminiscence" which many healthy people have. Now, if with the "dreamy state" occurring suddenly, there be a "warning" of smell or of taste or a sensation referred to the epigastric region, there is certainly epilepsy; there is always defect of consciousness, but loss of consciousness does not always follow. These seizures are often so slight, so odd, so short, so little incapacitating, and are sometimes, strange to say, positively agreeable, that the patient may disregard them until a severe fit comes to tell him what they mean. Medical men, until the diagnosis is made by a severe, and, so to say, an ordinary fit, may not recognise the epileptic nature of these quasi-trifling seizures, and may put them down to hysteria, indigestion, malaria, etc. The diagnosis of slight fits of epilepsy from accounts given by patients or their friends is often so difficult that we cannot afford to neglect quasi-trifling symptoms. In some of the cases of the sub-variety of epilepsy just remarked on, there is very seldom a warning of taste, but there are not rarely movements and noise of chewing, or tasting, or smacking the lips, or there may be spitting, implying, I submit, discharge of central gustatory elements. If, with defect of consciousness and the "dreamy state," there are such movements, I should conclude confidently for epilepsy.

I submit that such groupings of symptoms are of value as types. As yet there have been but two necropsies in the cases of patients who have been subject to such fits; both cases are very complicated. The only one published, so far as I know, is recorded by Dr. James Anderson (*Brain*, October, 1886) (taste warning and "dreamy state"); another observed by myself and Dr. Beever (warning of smell and "dreamy state"), has not yet been published; in each, tumour in the temporo-sphenoidal lobe was found. It is by cases like these two, cases of gross local organic disease, and by the investigations of Ferrier, that we shall ultimately be able to consider all "varieties" of epilepsy (using that name just now generically), not only as each is a type, but on a rational basis also, as each points to a "discharging lesion" of this or that part of the cerebral cortex. In most cases of the variety of epilepsy I have been remarking on, there is no evidence of gross local organic disease.

For many diseases each man will have to make his own types; everybody does so consciously or unconsciously, but I submit that we should make them consciously and definitely. Taking for next illustration migraine: I make three types, or rather consider cases of it as they are, (1) typical, (2) sub-typical, and (3) super-typical. (1) A typical case is one in which there is "visual projection," the well-known zigzag or "fortification" outline is most common, with, in most cases, a hemianopic area; then comes headache, most often frontal and one-sided, and lastly vomiting. (2) There are sub-typical cases, visual projections and nothing more, or headache unpreceded by visual projections, and not followed by vomiting. (3) There are super-typical cases deserving very particular consideration. In these there is in addition, so to say, to the other symptoms, numbness of one side of the body, and when the numbness is of the right side there may be also considerable aphasia. To the inexperienced these cases look far more serious than they are; they do not point to any gross local disease. Some of the symptoms of migraine, the headache among others, are, I think, after-effects of the discharge producing the paroxysm.

Here once more I stay to show that the arbitrary study of cases by type does not prevent us from making at other times and

for other purposes rational generalisations. Using for the moment the term epilepsy generically I should class migraine, epilepsy proper, and epileptiform seizures as epilepsies, on the basis that in each there is a "discharging lesion" of some part of the cerebral cortex. Their differences are sufficiently accounted for by the different "seats" of those lesions. Observe the opinion expressed is that there is a persistent, although varying, hyperphysiological (crude physiological) lesion in each of the three. But in so classifying them for scientific purposes, I no more overlook their vast differences, no more undervalue the study by type, than I confound whales, bats, and hedgehogs when I say that all these animals are mammals. One reason for the scientific classification is that I have seen cases intermediate in type between migraine epileptiform seizures and epilepsy proper ("missing links"). I have seen a case indistinguishable from what I have called super-typical migraine, except for the fact that the ailment followed a blow on the head leaving a depression on the left side of the skull; the limb-symptoms were right-sided, and there was partial aphasia (both, of course, temporary). I treat all three epilepsies (still using the word generically to cover epilepsy, epileptiform seizures, and migraine) on the same general principles, of which more further on; also treating each patient according to the peculiarities of his case; dyspepsia is often an exciting cause of a paroxysm of migraine; there is of course special treatment for the relief of the headache in cases of migraine, antipyrin for one remedy.

It may be said that classification of the three different diseases on the basis mentioned is purely hypothetical. It is not so far as epileptiform seizures are concerned, and is now not for the two cases of the variety of epilepsy proper completed by necropsy I recently mentioned. I should, however, be sorry to have it implied from this remark that I repudiate hypotheses in scientific research. For the sake of argument, let it be granted that the whole classification is altogether hypothetical. The use of hypotheses is the method of science. To suppose that we can make discoveries by the Baconian method is a delusion. No discovery, we are told by good authority, has been made on that method. An hypothesis, otherwise a supposition, is not a conclusion; it is only the starting point for methodical observation and experiment, the endeavour being not only to prove it, but to disprove it. I have not concluded that migraine, epileptiform seizures, and epilepsy proper are alike, in that in each there is a "discharging lesion" of some part, in each of a different part, of the cerebral cortex. Such an hypothesis is the beginning of hard work; its only use is to give order to that work. Huxley writes (article "Science" in the *Reign of Queen Victoria*, vol. ii): "As a matter of fact, Bacon's 'via' has proved hopelessly impracticable, while the 'anticipation of Nature' by the invention of hypothesis based on incomplete inductions, which he specially condemns, has proved itself to be a most efficient, indeed an indispensable, instrument of research." He writes too (*op. cit.*, p. 336-7), "It is a favourite popular delusion that the scientific inquirer is under a sort of moral obligation to abstain from going beyond that generalisation of observed facts which is absurdly called 'Baconian' induction. But anyone who is practically acquainted with scientific work is aware that those who refuse to go beyond fact rarely get as far as fact; and anyone who has studied the history of science knows that almost every great step therein has been made by the 'anticipation of Nature,' that is, by the invention of hypotheses, which, though verifiable, often had very little foundation to start with; and, not infrequently, in spite of a long career of usefulness, turned out to be wholly erroneous in the long run." I imagine that the saying of a great man will occur to most of us "hypotheses non fingo." It may be well to mention Newton's "Corpuscular Theory of Light," which, Huxley says, "was of much temporary use in optics, though nobody now believes in it." The plain fact is that no one dislikes hypotheses; some of those who think they do have no clear notion of what the term means, and are supposing something all day long. In what is to come I shall use the term hypothesis, and I submit correctly, for some current explanations, which are not commonly considered to be hypothetical at all. What all of us dislike is an explanation of difficult problems, the complex. We may easily err in taking the subjective confusion produced in us to be in the objective thing contemplated, which is really only very complex.

is, in my opinion, a natural division. I mean that it is one answering to the evolutionary constitution of the nervous system. *Le petit mal* passes suddenly into *le grand mal* when the discharge beginning in some part of the highest centres has become strong enough to overcome the resistance of lower (middle) centres.

* Those who make the mistake that a supposition is a conclusion forget what they were told at school, that Newton put aside his theory of gravitation for many years because it was discrepant with certain observations, was, it temporarily seemed, disproved.

To come back to the study of diseases by type. I fear I should in some ways carry this method to what may seem an extravagant extent, as in chorea. I confess I make an arbitrary division of these cases—have two types, "slight" and "severe," although there may be no scientific distinction.⁶ The slight type: a girl who has slow, infrequent movements, of comparatively limited range, of the face and limbs, especially if they are almost limited to one side, who could get about if permitted, whose appetite is normal, and who sleeps well, recovers soon in most cases by good food, tonics (quinine or arsenic), aperients, and rest—would very likely recover without medicines. The other type—"severe" chorea—is practically a very different thing: a girl with incessant, frequent,⁷ and very wide movements of all parts, who has to be kept from bruising herself by various expedients (padding hands and joints in cotton-wool, etc.), is to be looked on as requiring very different treatment. The tendency is to death. There are few cases of nervous diseases in which we can do so much good. We give food in large quantities, solid if possible. Brandy is a most excellent remedy; in very severe cases we may give twelve ounces in twenty-four hours. If ordinary means of feeding are impossible, we should put the patient under ether and feed by a tube. Our main object is to keep the patient alive. But I think brandy and in severe cases etherisation are of curative value—indirectly, at any rate. There is certainly very often rapid improvement after their administration. I never think of "specifics" in this type of chorea. Beyond aperients, the only drug I give is chloral, and that only when food has been taken. I never give opium.

It would save a great deal of trouble if we had an orderly series of types of cases in which vertigo is a prominent symptom. I make four: (1) ocular; (2) auditory; (3) epileptic; and (4) gastric. This is the order of their simplicity, and therefore the order in which they should be studied; it is not the order of their seriousness. (1) Anyone can learn very much that is of great value in the diagnosis of other, more difficult, cases of vertigo in five minutes, if he will first properly investigate a case of recent paralysis of ocular muscles. It may seem that recognition of the cause of the vertigo is easy by the strabismus. Most often so, no doubt; but in cases of paralysis of the superior oblique the strabismus is far from being obtrusive, and the real cause of vertigo in such cases may be easily overlooked, and often is. (The vertigo is not owing to double vision.) In these cases we find vertigo in its simplest form. The treatment of ocular vertigo is, of course, that of the lesion causing the local paralysis, and this, excluding tabes, is mostly caused by syphilis. I have never been able to verify the hypothesis that this paralysis is of rheumatic or gouty pathology. (2) The next simplest vertigo is auditory. Very often indeed ear disease causes—or, rather, is a factor in the causation of—paroxysms in which, with many other symptoms, there is vertigo, "swimming of external objects," etc. The diagnosis is very easy if we find ear disease in patients who have attacks of giddiness, with reeling, sickness, intense depression, and often perspiration. It is very important to recognise the causation, otherwise one might think there was serious brain disease, or mistake the case for mere stomach vertigo. Digestive derangement and other causes of lowered general health are often excitants of the paroxysm, and this guides us in an important part of our treatment. We try to cure the ear disease, but often we cannot. Quinine in large doses is praised by Charcot. The best remedies are lessened work and bracing air. (3) Epileptic vertigo is often used as a name for a slight fit of epilepsy, even when there is no vertigo at all, no apparent movement of external objects, and no feeling of the patient turning. I use it as a name for vertigo at the onset of epileptic attacks; sometimes it is for a time almost the whole of a seizure, and is afterwards the "warning." This is the most serious form of vertigo; the treatment is for epilepsy. (4) I have slender respect for the hypothesis that a stomach disorder sometimes produces a paroxysm of vertigo. If there be such a thing as stomach vertigo, it is the most difficult of the four kinds of vertigo to explain. The question is, "How does a dyspeptic attack produce a sudden attack of vertigo?" Hence I put it last on the list for particular study. When a man has a paroxysm in which objects seem to move to one side (when the vertigo is, as Gowers puts it, "definite"), I certainly do not

believe that any kind of digestive disorder is the real cause of the paroxysm; the hypothesis is then too heavy for practical purposes. No doubt most cases of so-called "stomach vertigo" are cases of aural vertigo, the attacks being sometimes brought on by dyspepsia. As to treatment, one of course treats dyspepsia in whatever association one finds it. I now start from a new line of thought—not, however, altogether leaving the subject of study of diseases by type.

I have just been urging that we should take up for particular study cases in the order of their simplicity. Were I to write a book I should follow this plan; for the ambitious aim of the work would be to help people in practice to examine for themselves in scientific ways for practical ends. We cannot learn for ourselves if we begin with complex problems; if we do, we shall run the risk of making vague, and often verbal, explanations. Cases of gross organic brain diseases (such, I mean, as tumours, masses, "lumps") are among the simplest of all cerebral diseases. Simple and serious are not, as I have recently been urging and trying to illustrate, convertible terms. Cases of gross organic disease of the brain are far simpler for diagnosis, prognosis, and even, in some ways, for treatment, than are cases of what are called the "neuroses" (chorea, epilepsy proper, neuralgia, etc.). Of the former we know a great deal and speculate too little; of the pathology of the latter we know nothing certain and speculate much. Very confident opinions—really hypotheses—are expressed as to the causation and pathology, in particular cases, of the "neuroses" when we are diffident in the diagnosis of intra-cranial tumour in very many cases. But before we speak confidently of the causation and pathology of cases of hysteria, epilepsy, chorea, neuralgia, cases without a known morbid anatomy, we should discipline ourselves by a study of cases in which necropsy is likely to confront us. A *post-mortem* examination tells us candidly what our powers of clinical investigation are worth in the case examined. I am speaking of the anatomy, physiology, and pathology of the neuroses (*vide infra*). I have small faith that any great good will come of the studies of these diseases by those who have not previously disciplined themselves by careful work on what I may be permitted to call the coarser diseases of the nervous system, and who have not before that a good knowledge of recent physiology.

It may be objected that the study of cases of gross organic intra-cranial disease is dimly fruitless, as we can do very little for the sufferers from it. I believe that we may do very much, even when the tumour is non-syphilitic. For many of the symptoms of tumour are really symptoms of an encephalitis, or certainly of some secondary change, provoked by the tumour in its character as a "foreign body;" such symptoms are headache, optic neuritis (this is strictly speaking a visible pathological condition), and "fits" of different types. If we can do nothing for the tumour, we may do something for the secondary changes it induces; certainly we may rid the patient of optic neuritis when the tumour, which some way caused it, remains; and if the patient is to die, he will live to the end of his shortened life more happily seeing than if blind. Excluding optic neuritis in cases of Bright's disease, in meningitis, and some other obviously exceptional cases, I treat cases of optic neuritis very actively by iodides and mercurials. I do not say that optic neuritis would not pass off without such treatment, but I dare not omit it. Patients have got rid of optic neuritis after removal of brain tumours (Horsley) when no drugs have been given.

There is another thing to be said of treatment in some cases of gross organic brain disease. I believe that some patients die suddenly of intense pain; I am certain that some die after sudden increase of pain, and that too in cases where *post-mortem* examination discovers no recent changes. In some cases I apply leeches to the temples, as the quickest way of relieving the sudden increases of pain. I do not hesitate to give morphine by hypodermic injection. Not only do we thus diminish misery, but we may, I think, sometimes save life. For more continuous pain (the patient's ordinary pain I may call it) I should try antipyrin. If the patient dies suddenly, as I repeat he not rarely does, his death may be erroneously ascribed to the remedies we use for allaying his pain.

Then, in some cases of cerebral tumour, the surgeon comes to our aid. To diagnose a brain tumour, and then, if we can, to localise it in certain regions, is a very important thing. Nowadays, a medical man is not thought to be wasting his time when he is anxiously trying to get to know the starting-point of an epileptiform seizure, in the case of a patient who has double optic neuritis

⁶ The status epilepticus or epileptiformis, practically an acute disease, requires a very different kind of treatment from that of epilepsy or epileptiform seizures in the ordinary course of cases. For the status, food and some stimulant; chloral is the best drug; and, if the fits are very frequent, etherisation will help.

⁷ The pairs of words "slow and quick," "infrequent and frequent," are used after the manner of their use clinically for alterations of the pulse.

—whether the initial spasmodic movement is of the thumb or great toe. Nor is he now thought to be talking nonsense when he insists that with marked double optic neuritis, there may be good sight, rather he is supposed to be repeating an old story. I shall speak only of the surgical case I know most about. Mr. Victor Horsley removed a scrofulous tumour from the right cerebral hemisphere of a patient of mine who was subject to fits, beginning in his left thumb (see Horsley on Brain Surgery, *BRITISH MEDICAL JOURNAL*, vol. ii, 1886, p. 670). The patient is still under my care, suffering now from partial paraplegia, consequent on angular spinal curvature; his left hand is still partially paralysed, but for many months he has had no fit.

There are other reasons for the careful and particular study of these comparatively simple, but yet very serious, morbid affections. Unless we are in the habit of using the ophthalmoscope by routine, when patients, unaccustomed to them, begin to have severe headaches, especially if the pain is in an unusual seat, side of head or occiput, we shall often overlook the real nature of cases of serious brain disease altogether, or we shall miss treating their earliest stages, the stages most amenable to treatment, if treatment is to do any good; we shall misinterpret them as hysterical, as being cases of indigestion, etc. It is thoroughly well known now that marked optic neuritis (the best evidence of an intra-cranial tumour) is very often indeed unattended by defect of sight. I have been told that I say too much of this, that nobody now denies the dictum. But I contend that it is not sufficiently widely known, and say that the assertion is still received by many medical men with great incredulity. If sight be good in optic neuritis, that important sign does not exist for those who do not use the ophthalmoscope, and thus from the headache and vomiting alone the erroneous diagnosis of stomach and liver derangement is easy. (There are cases of intra-cranial tumour without optic neuritis, or for months or years without it. There may be no headache, there may indeed be scarcely any symptoms at all where there is a large cerebral tumour.)

Here I give a caution. Do not take the strange, miserable feelings, "weight," "tension," etc., at the top of the head, and "dragging" at the back of the head and "down the spine," for the pain from gross local intra-cranial disease, such as tumour; the patient complains bitterly of what he calls "headache," although he may qualify the word by saying that "it is not an ordinary headache." These "pains" at the top and back of the head are very common in cases of nervous exhaustion, and do not signify local gross organic diseases within the cranium.

There is another reason for the particular study of cases of local, gross, organic disease. Patients who have intra-cranial tumour not rarely die suddenly or rapidly, when on our last visit they seem to be in good general health; they die most unexpectedly. Sometimes, as I have said, when speaking of treatment, this occurs during sudden increase of pain. Occasionally in the case of a patient who all along has only had headache, optic neuritis, and vomiting, there ensues hemiplegia or apoplexy, or both; when so, the likelihood is hæmorrhage from a vascular tumour. Sometimes, and this Fagge pointed out, death occurs by respiratory paralysis. If anyone thinks that because there is intra-cranial tumour the ailment must have a slow course, and that the patient or his doctor will be sure to have a definite warning of the approach of death, he is mistaken. I have known a patient, taken seriously ill one morning at his work, die in a few hours of respiratory paralysis, when the tumour was a syphilitic one, of one occipital lobe. A great many times have I seen a speedy ending to the life of patients who, having double optic neuritis, headache, and vomiting, seemed to be, judging only from their general condition, not even seriously ill. Yet this triad of symptoms, if we can exclude chronic Bright's disease, some cases of which are indistinguishable from cases of cerebral tumour, except by the ascertainment of the existence of that morbid kidney affection, is certain evidence of some serious intra-cranial disease, and is the best evidence of intra-cranial tumour. The assertion that a patient who has double optic neuritis and good sight, only headache now and then, and who for much of her time seems perfectly well, is to be considered as being dangerously ill, at first glance seems absurd. I said "her time," for I am now thinking of the case of a young lady who, shortly before I saw her, had been to a dance, remaining to about 4 o'clock in the morning, and who died rapidly not many days after her visit.

(To be continued.)

AN INVESTIGATION OF AN OUTBREAK OF COW-POX IN WILTSHIRE,

WITH A COMPARATIVE ACCOUNT OF SOME PREVIOUS OUTBREAKS
IN ENGLAND, GERMANY, AND FRANCE.

By PROFESSOR EDGAR CROOKSHANK, M.B.

Of King's College, London.

(Continued from page 5.)

I will now pass on to give, for purposes of comparison, an account of cow-pox as we have received it from Jenner and other writers on this subject.

JENNER'S DESCRIPTION OF COW-POX.

I cannot, perhaps, do better than quote verbatim from Jenner's essay.

"There is a disease to which the horse, from his state of domestication, is frequently subject. The farriers have termed it the grease. It is an inflammation and swelling in the heel, accompanied at its commencement with small cracks or fissures, from which issues a limpid fluid possessing properties of a very peculiar kind. This fluid seems capable of generating a disease in the human body (after it has undergone a modification I shall presently speak of) which bears so striking a resemblance to the small-pox that I think it highly probable it may be the source of that disease. In this dairy country a great number of cows are kept, and the office of milking is performed indiscriminately by men and maid servants. One of the former having been appointed to apply dressings to the heel of a horse affected with the malady I have mentioned, and not paying due attention to cleanliness, incautiously bears his part in milking the cows with some particles of the infectious matter adhering to his fingers. When this is the case it frequently happens that a disease is communicated to the cows, and from the cows to the dairymaids, which spreads through the farm, until most of the cattle and domestics feel its unpleasant consequences. This disease has obtained the name of the cow-pox. It appears on the nipples of the cows in the form of irregular pustules. At their first appearance they are commonly of a palish blue, or rather of a colour somewhat approaching to livid, and are surrounded by an inflammation. These pustules, unless a timely remedy be applied, frequently generate into phagedænic ulcers, which prove extremely troublesome. The animals become indisposed, and the secretion of milk is much lessened. Inflamed spots now begin to appear on different parts of the hands of the domestics employed in milking, and sometimes on the wrists, which run on to suppuration, first assuming the appearance of the small vesications produced by a burn. Most commonly they appear about the joints of the fingers and at their extremities; but, whatever parts are affected, if the situation will admit, these superficial suppurations put on a circular form, with their edges more elevated than their centre, and of a colour distantly approaching to blue. Absorption takes place, and tumours appear in the axilla. The system becomes affected; the pulse is quickened; shiverings, succeeded by heat, general lassitude, and pains about the loins and limbs, with vomiting, come on. The head is painful, and the patient is now and then even affected with delirium. These, sometimes varying in their degrees of violence, generally continue from one to three or four weeks, leaving ulcerated sores about the hands, which, from the sensibility of the parts, are very troublesome, and commonly heal slowly, frequently become phagedænic, like those from whence they sprung. During the progress of the disease the lips, nostrils, eyelids, and other parts of the body are sometimes affected with sores, but these evidently arise from their being heedlessly rubbed or scratched with the patient's infected fingers. No eruptions on the skin have followed the decline of the feverish symptoms in any instance that has come under my inspection, one only excepted, and in this case a very few appeared on the arms. They were very minute, of a vivid red colour, and soon died away without advancing to maturation; so that I cannot determine whether they had any connection with the preceding symptoms. Thus the disease makes its progress from the horse (as I conceive) to the nipple of the cow, and from the cow to the human subject."