be distinguished from inflammation. In any part of their course a syphilitic neuritis may occur, and this is probably the most common cause of an isolated affection of a single nerve or of a single branch. Thus every diagnosis involves the careful consideration of the symptoms and their combinations, as well as of their mode of onset and associations. The subject is too large for me to do more than thus indicate the lines on which it must be approached. It needs, indeed, to receive a thorough investigation in the light of the pathological facts that are scattered through medical literature, and which need to be greatly increased in number before our diagnosis can be exact.

The greatest difficulties arise from the fact that ocular palsies are caused by some of the effects of syphilis that are not specific in their nature. An isolated palsy of one third nerve, in a young man known to have suffered from the constitutional disease, was naturally supposed to be truly specific. But treatment, although prompt and thorough, had no influence on it. One day he suddenly became unconscious, and died in a few hours. We can scarcely doubt that an aneurysm, due to syphilis, caused the palsy and his death. But the most frequent non-specific causes of these symptoms are the consecutive degenerations. These may set in before the period of true specific lesions has passed, and the ocular nerves constitute one of the regions in which the symptoms of the two kinds most often coincide. More frequent than actual coincidence is the doubt produced by it. Is this palsy due to a true specific lesion, or is it part of nerve degeneration that is a non-specific sequel to syphilis? The question is of great importance in regard both to prognosis and to treatment.

In cases of locomotor ataxy that occur comparatively early, this difficulty is especially frequent. The distribution of the symptoms may furnish an answer, as we shall see in a moment. Sometimes the suddenness of the onset raises a strong presumption that the palsy is tabetic and not specific. The same conclusion is indicated when symptoms quickly lessen without treatment and soon afterwards return. The nature of many of these tabetic palsies is doubtful, but some are certainly due to nuclear degeneration. These, as we saw in the first lecture, are often sequelæ of syphilis. Their distinction from a true specific lesion is another problem of much difficulty, a difficulty further increased by the fact that the nuclear affection seems sometimes to follow a peripheral palsy so directly that the symptoms of one pass into those of the other.

The most important characteristics of the nuclear palsy are the association of muscles according to function, its bilateral distribution, and its persistence in spite of treatment. In most true specific lesions, prompt treatment has an influence which is absent in the degenerative maladies. I need not trouble you with instances of these features. They are important distinctions, both in the isolated degeneration and in that associated with tabes. I may add to these distinctions the therapeutic test, which is often of great use in the diagnosis, but is of negative significance only when the proper treatment has been employed early.

I have said that the nuclear degeneration sometimes seems to follow a specific palsy—chiefly when, from the lateness of treatment, this palsy has lasted long and recovered imperfectly. A curious speculation is suggested by this—a speculation which I may indulge myself by starting, although I may not pursue it. Is the semblance of causation a real indication? Conduction through motor fibres is arrested; this arrest may—perhaps must—modify the action of the cells from which the fibres conduct, for the fibres do not carry force like tubes, but by an action similar to that of the cells, but far lower in degree. The disturbance of the centre must entail changes in nutrition, as every functional state does. We see the effects of prolonged arrest of conduction in the altered action of the facial muscles after disease of the nerve-trunk. It is conceivable that, given a predisposition to degeneration, the changes in nutrition excited by the arrested action may acquire an independent energy, and assume an independent course. The conclusion is, moreover, not without indirect support from some of the facts of physiology.

A tendency is often to be observed in the present day to under-rate diagnosis, or at least the elaborate diagnosis of which the diseases of the nervous system furnish so many examples. In the face of the urgent needs of suffering humanity, with its mute or uttered cry for the relief we cannot always give, our precise distinctions and elaborate processes may seem like an ingenious

device for interesting us while the patient suffers. That such an impression is wholly wrong I need not say to those who hear me now; but the tendency is real; it is reflected beyond our own ranks; and I may give one warning—a warning to myself as well as to others—that we should be always on our guard lest we do anything, by word or manner, that may excite or foster the feeling to which I refer. Diagnosis must come before treatment, but this should make us careful lest we produce an impression that we regard the order of the two in time that also of their importance—an impression easily produced when, as often, the treatment is plain and its methods familiar, while the diagnosis is complex and its processes strange.

Surely the diseases we are now considering supply a strong reproof to those who consider that, "to say the least, diagnosis is somewhat overdone." A diagnosis that can be described as "rough and ready" may serve to make our treatment effective in the majority of cases, but there are many in which only a most careful and even elaborate diagnosis will enable us to do all that can be done, and there are not a few in which the utmost degree of care and elaboration is needed to save us from being wrong in our treatment—often wholly wrong, and sometimes disastrously wrong. I could give many instances of this, striking and sad, but it is needless to do so, and it may be unwise. My own conviction is that diagnosis in itself cannot be overdone. To permit diagnosis to be studied and employed at the expense of treatment is a proceeding for which no words are too strong. To cultivate, at the expense of its application, that which has no other end, should in medicine be a sin beyond the reach of pardon; but, as a guide to treatment and prognosis, we cannot well know too minutely the seat of the maladies we are treating, and assuredly we cannot know too certainly their nature. Our art may be all-powerful to save, but without guidance it is useless—a hand to use without an eye to see. When the disease is beyond our present means of combating it, the same guidance is still needed to save the patient from that which, without power of good, may not be without capacity for harm; and I believe that there is no point in diagnosis, however elaborate it may be or superfluous it may seem, which is not or will not be, in some case or at some time, of definite and even essential service in enabling life to be saved or health restored.

THE

PAST AND FUTURE OF PATHOLOGY.

Being a Portion of the Presidential Address delivered at the Annual Meeting of the Pathological Society of London on January 29th, 1889.

By W. HOWSHIP DICKINSON, M.D., F.R.C.P.,
President of the Society.

AFTER some preliminary remarks in the course of which he thanked the Society for the honour done him, Dr. Dickinson proceeded as follows:—

Pathology is of great antiquity, but is apparently far from being exhausted. The science of the nineteenth century has touched it with new vitality, and, with the help of the microscope and chemistry, it is now developing and bearing fruit in a manner which has no parallel in any former part of its long history.

The ancients were acquainted with some of the larger facts of pathology, and turned them to practical use. They learned at a very remote date to connect pus in the pleura with its external signs, but, on the whole, their observation was superficial, more upon the living than the dead. I cannot find that before the seventeenth century there was any knowledge of conditions to us so obvious as cerebral hæmorrhage and hepatisation of the lung, though the symptoms in connection with them were matters of ancient experience.

So great indeed have been the acquirements of modern times, that we of this day can scarcely imagine the state of medicine even at a date so recent as to be within the life and possibly within the memory of some who are still with us. Bright published his observations on the kidney in 1827. How confused must have been the ideas of dropsy and coma before that time, and how much of enlightenment was due to his observations in morbid anatomy and his association of them with certain obvious symptoms.

This Society was founded in 1846. Let me briefly glance at the advances which its chosen science has made since that date. In 1847 Virchow made known some of the processes of embolism, which were more fully worked out by the late Dr. Kirkes, and the results published in 1852. There appear to have been isolated observations of the same kind before these. I owe to Dr. Payne the knowledge that, in 1684, one William Gould, an Oxford doctor, recorded the fact that clots, "particles of a polypus," he calls them, were carried from the left ventricle to the carotid artery, and there impacted, stopping the intercourse of spirits in the brain. But our present knowledge is due to Virchow and Kirkes. The light they have been the means of directing upon many disorders, particularly of the brain, is such that we can scarcely realise the confusion and obscurity in which they must have been involved before these simple pathological observations provided the clue so much needed. Among great pathological discoveries, this in importance, as in date, follows next upon that of Bright.

Close upon this comes the lardaceous or amyloid disease, which had been slowly emerging from primordial darkness since, in 1842, it was noticed by Rokitsansky as a simultaneous infiltration of several organs by an albuminous blastema, which imparted the aspect of lard or bacon, and accompanied cachectic states. In 1853 Virchow discovered the so-called amyloid reaction and constructed the amyloid theory. With the ready means of detection thus provided, and the interest of a novel theory, the disorder at once became the subject of attention and of further elucidation, as the annals of the Society sufficiently show, and, as the direct result of pathological inquiry, assumed a position in clinical medicine to which great importance must be attached, whether we have regard to the number and consequence of the organs it embarrasses or to the practical benefits which ensue from its recognition.

Next in time comes the discovery of Addison, who, in 1855, did for the suprarenal bodies what his colleague had done for the kidneys, and astonished the world by showing that these hitherto disregarded organs were subject to morbid changes which were connected with symptoms as striking as any that Bright had associated with renal disease.

Of more frequent application, and therefore of more practical importance, is the discovery of the nature of suppuration, which was delayed until 1867. The migration of leucocytes had indeed been noticed by William Addison at an earlier date, but it is owing to the later and apparently independent observations of Cohnheim that the process became an accepted part of medical science, a part which seems to us now so essential that we can but wonder that until so recent a date pathologists and surgeons were content to believe what now appears impossible, that this flux, often so profuse, was the product of the tissue, not of the blood. It is interesting to recall a forecast made seventy-four years before Cohnheim's observations, which comes as near to the true theory of suppuration as was possible without microscopic research. "Pus," said John Hunter, "is formed from some change, decomposition, or separation of the blood which that undergoes in its passage out of the vessels."

I may next touch upon the nervous system. When this Society was founded the minute pathology of this paramount portion of the human frame was almost absolutely unknown. An important structural disease of the cord was described as essential paralysis, I suppose because paralysis was thought to be its essential and solitary character, one to which no organic change was annexed; nor had any of the disorders now known to depend on changes in the anterior horns been thus traced to their organic origin. Locomotor ataxy was then beginning to be recognised by its symptoms, but scarcely yet by its lesions. The symptoms of pseudo-hypertrophic paralysis, and of the various forms of sclerosis and of neuritis, had not yet been separated from those of other disorders, and no human eye had discovered their attendant morbid anatomy, the conditions which, with regard to sclerosis and neuritis, compose their definition and distinction. At the time of our foundation no one could make sections of the brain or cord which displayed any but the coarsest alterations. The art of making translucent sections is quite a recent one; we owe it chiefly to Lockhart Clarke, whose methods initiated a new epoch in the pathology of the nervous system.

By such means some diseases, as has been seen, have been traced to definite pathological origins, while others have been associated with lesions not hitherto known to belong to them, though it is probable that these lesions are not the ultimate results of inquiries, which must still be regarded as incomplete;

but they are not without use, for they show the track and mode of morbid actions which have not yet been traced to their source.

Among the diseases of the pathology of which we have learned something, though not everything, may be mentioned tetanus, hydrophobia, chorea, the various forms of insanity, and I trust I may not be thought egotistic or obstinate if I add diabetes to this number. Some of these diseases (hydrophobia certainly, and tetanus probably) have beginnings in the humours, whether they be called infectious or zymotic; but none the less must they be material, proper for pathological research, and capable of identification.

The possibility that minute animal or vegetable organisms might be the essential causes of contagious diseases is a hypothesis which has long been present in the minds of physicians and naturalists. Sir Henry Holland, fifty years ago, speculated upon insect life as a cause of disease. He tells us that the exanthemata and other contagious disorders had been, in the imagination of an earlier theorist, attributed to different species of acari and quotes Henle who had recently propounded the theory that "the material of all contagious disease was not merely organic, but matter possessing all the conditions of parasitic life."

But it has been reserved for later times and our own contemporaries to place this theory upon a basis of observation. Vibriones have long been known as associated with decomposition, but any definite association of bacteria with disease is younger than this Society. The bacillus of anthrax was made known in 1855; that of relapsing fever in 1873; that of tubercle in 1882. I need not mention others; their name is legion; they threaten to become almost as numerous as diseases. The detection and definition of, as yet, unknown kinds, and their association each with its ailment, or, if not so, with the condition, whatever it be, in which it presents itself, is work for the future, to which must be added further questions: Is the bacillus the cause of the disease, or is it only associated with the cause? Is it essential or accidental; or sometimes one and sometimes the other? What chemical changes are associated with the life of the bacillus? What are its relations to the blood and tissues? These questions, and many others, suggest themselves; the field of view widens, but as yet we can see no limit to it. Practical results have already been gained, among others, the system of antiseptic surgery.

But, while we pry into the doings of the organisms by which we are infested, let us not forget that there are such things as chemical diseases—by which I mean such as depend in the first place neither on organisms nor organs; not so much on the machine as on the material which is put into it or taken out of it. More or less oxygen, nitrogen, or carbon; more or less acid or alkali; the due balance of potash and soda—all these questions land us in pathology, and that of the most important kind. Be it remembered that we are a pathological Society, not one of mere morbid anatomy. Among chemical diseases, or what we suppose to be such, I need only instance rheumatism, gout, and scurvy. About these we have learned something, and seem to be on the verge of learning more; but the part that is wanting is generally the final one, which should give coherence and use to all the rest. Nature treats us like the illogical lady who granted the major and minor but denied the conclusion. For example, take scurvy.

Nothing seems better ascertained than that foods are antiscorbutic according to the potash they contain; thus scurvy should depend on a want of potash, and be prevented or cured by this alkali. But as yet we have not been able to make the sum prove. Potash will not cure scurvy, though pot-herbs will. We seem to be close to something which we have not grasped; could we go but one step further, and find the precise pathological deficiency constituting the disease, we might find ourselves in possession of a chemical antidote.

Let what we have done encourage us to attempt more. Let us think twice before we admit that any disease is functional—unconnected, that is, with material change. Let it be the function of this Society to unmask functional disease. An alteration in the action of an organ may depend upon a change in the organ itself, in the material supplied to it, or in the nerves which regulate it; but there must be a change somewhere. Changes in the organ or its supply are obviously within the scope of research, pathological or chemical. Alterations in innervation are less obviously so, but not necessarily and for ever outside it. If we can find nothing amiss in the nerve, we must look at the nerve-centre, and failing this at the agencies, be they what they may, by which that centre is influenced. Here we may be clinically successful, but perhaps may as yet find nothing

within the range of morbid anatomy. If these agencies, many of which belong to the outside world, are only temporarily wrong, equally transient may be the result. In delirium, for example, fleeting and difficult to seize may be the conditions, whether in brain or blood, which pertain to it; but when the result is enduring or permanent, we should be able to recognise something in the permanent structures to keep it up. A transient mental disturbance which we must call functional, because, whatever change there may be with it is not discernible, may leave disease which is persistent. A fright may cause chorea, insanity, or diabetes. But where the disease is thus lasting, it must be kept up by something as lasting as itself, which cannot be elsewhere than in the organism. It is improbable that any such perdurable mischief will remain for ever out of sight. I myself refuse to consider any disease, be it short or long, as functional in the sense of being without material change; but I am willing to call so many disturbances which pass away with their cause. But when diseases outlast their external cause or come without one and persist even unto death, then the word "functional" is but a disguise for ignorance of the grosser kind.

OBSTETRIC MEMORANDA.

A CASE OF MONSTROSITY.

THE following case was attended by my assistant, Mr. Atkinson:—

Mrs. D. went to bed quite well on the night of November 18th, but awoke shortly after with labour pains (this being her ninth labour). She lived about four miles from my house; the husband came for me about 1 A.M. on November 19th. Being engaged with another case, my assistant went, and, on his arrival, found the head (right) occupying the cavity of the pelvis, and the mem-

branes protruding; the pains were severe, and he ruptured the membranes, expecting the child to be born almost directly, as the head seemed small and the pelvis large (nearly all her children, including the first, have been born before my arrival). Notwithstanding the severe pains, the head did not advance, and he sus-

pected something was wrong. He then made a further examination. Passing his hand beyond the presenting head, he found another one at the brim. Believing it was a case of twins, he applied the forceps, and delivered the head with very little difficulty; but then came the real difficulty. The head was born, but no other part would follow, in spite of his efforts to deliver by drawing on the head. Making a further examination, he could now feel an arm, and also found the two heads were springing from one body; he then thought of applying the forceps to the second head, but a strong pain brought it down to the perineum, and it was delivered naturally. The perineum was, of course, enormously stretched, but it escaped without rupture; the placenta followed in about ten minutes, and there was little hæmorrhage; it was very large. The first head suffered a good deal from pressure, and was very much discoloured, and did not cry for some time after it was born: the second cried at once. The mother has never had a bad symptom.

Description of Child.—It is a large male child, with two heads, quite perfect; ordinary length of necks, one body, three arms, two legs, talipes varus of both feet, and a caudal appendage with a bulbous extremity; the third arm springs from between the shoulders; the third hand has five fingers and a thumb; the thumb has two nails side by side, the first and second fingers no nails, the third a nail on dorsal and also on palmar surfaces; the fourth and fifth single nails. The following are the measurements: The heads are exactly of the same size; circumference, 12 $\frac{3}{4}$ inches, from fontanelle to chin 5 inches; the right neck is 1 $\frac{3}{4}$ inch, the left 2 $\frac{1}{2}$ inches, in length; circumference of right neck, 6 $\frac{1}{4}$ inches, left 6 inches. Chest, 15 $\frac{1}{2}$ inches. Abdomen, 12 $\frac{1}{2}$ inches. Right arm, 7 $\frac{1}{2}$ inches; left, 8 inches. Legs: right, 9 $\frac{1}{2}$ inches long; left, 8 $\frac{1}{2}$; circumference of right thigh and knee, 4 inches; left, 4 inches thigh, and 3 inches at knee. The right and left hands, and also the feet, bore the usual complement of toes and fingers. There are two separated spinal columns down to the lumbar region, where they merge into one, below which it is a perfectly-formed single male

child. Each spinal column has ribs attached to it, which pass round to the sternum in front (there being only one sternum); there are also short ribs which take their origin from the inside of the vertebral columns, and attached to something behind, not easy to determine what, between the two columns, and deeply placed, probably another sternum, but ill formed. There are two