The following sentences form the concluding paragraphs in the paper published in the *Proceedings of the Royal Society*.

Fig. 1. Case of J. F. Organisms between the root-sheath and the shaft of the hair near the root. (Drawn by Mr. Noble Smith.) Magnified about 470 diameters.

Fig. 2. Organisms from the group shown in Fig. 1, more highly magnified. (Camera drawing.) Magnified 560 diameters.

"It may be well to divide the statements made in this paper into two heads: those which relate to ascertained facts, and those which relate to a theory of the causation of alopecia areata, which I believe is sustained by these facts.

"1. The facts are, that minute bodies of definite and fixed shape and size are found in and on the hairs in alopecia areata. These bodies are distinct from the granular elements present in hairs, and are neither oily particles nor crystals. They are of the size and shape, and have the refractive qualities, of bacteria. When present in small numbers on the shaft, the hair is entire; whilst within some hairs much affected by the disease they were found in great numbers.

"2. The theory is, that these bodies are bacteria, and that the disappearance of the hair is due to a breaking up of the hair-shaft by the multiplication in it of the organisms.

"As I believe it is desirable to give to definite objects like those which I have described a name which will mark their association with the theory I have founded on them, and as I am myself satisfied as to their nature, I suggest the term *Bacterium decalvans* as a convenient designation."

* Subsequent recent investigations have confirmed the author's views regarding the existence of this bacterium. He has now observed it in all the phases through which a bacterial organism may be traced, and will shortly be able to publish an account of methods by which it can be more readily observed.—August, 1882.

THE DEATH-RATE OF BILSTON last year was the most satisfactory on record. The average death-rate of the place thirty years ago was from 31 to 32 per 1000; last year, thanks to sanitary improvement and the efforts of the medical officer of health, it was reduced to 16.1 per 1000, which must be regarded as very satisfactory for a Black County town. Only 31 deaths, moreover, out of the 381 recorded were due to infectious disease, a result which may, or may not, be due to Bilston being "one of the best drained towns in the country."

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF PATHOLOGY.

At the Annual Meeting of the British Medical Association,
in Worcester, August 1882.

By J. HUGHLINGS JACKSON, M.D., F.R.S.,

Physician to the National Hospital for the Epileptic and Paralysed, and to the London Hospital; President of the Section.

PATHOLOGY is the basis of every rational system of therapeutics. That, possibly, the greater part of our most effectual treatment is at present empirical I would not deny, but that therapeutics will become rational is what we all hope. To those who are quietly and laboriously working at what may, superficially looked on, seem to be obscure pathological problems, steady advances in diagnosis and rational therapeutics will be chiefly owing. It is evident that, for rational therapeutics, we must know what there is to be treated. No man in our profession deserves greater credit than the pathologist—including, of course, the clinical pathologist, and never forgetting the medical officer of health. The medical officer of health is the Pathologist of the Social Organism, and deserves the highest social recognition. Speaking more narrowly, tacitly assuming the obvious qualifications, the best practitioner, perhaps not the most confident one, is he who has carefully made most *post mortem* examinations. I do not mean him who stops here, who stops in a stage which is rather to be called one of morbid anatomy than of pathology. But a man must begin here—must begin here to learn for himself, at any rate. The best knowledge for practice is what a man gets for himself. The only way of being thoroughly practical is to face the facts, to get verification or disproof of our opinions. A *post mortem* examination never flatters us. If, for an example, we diagnose tumour of the cerebellum during the life of a patient, we may, *post mortem*, find one in the anterior cerebral lobe. Such a rap on the knuckles is good for us. It makes us less confident, and teaches us to be more careful. A *post mortem* examination may tell us that we have been treating a patient with useless, perhaps with injurious, drugs.

Every medical man has some system of pathology. But there are systems and systems. Besides scientific pathology there is a crude pathology, and there is, unfortunately, a metaphysical pathology. We have long heard that old maids' husbands are always well behaved, and on the same principle the pathology of those who do not make *post mortem* examinations is often confident and definite. But the really important thing is to be accurate and precise, otherwise we confidently organise error in definite formulæ. A man's pathology may be too definite in the wrong way, because he has not worked for himself at pathology. It may be only superficially and verbally definite and really crude. He who has made many *post mortem* examinations is not so metaphysical in his explanations of the pathology of some of those cases of disease of which there is no known morbid anatomy. In looking carefully for himself into the coarsely concrete, a realistic habit of mind is produced in a person, and thus he avoids verbal explanations of those most difficult cases which are without known morbid anatomy—of those, for example, commonly called the Neuroses. To my young hearers I say, always endeavour to obtain *post mortem* examinations. Unless you are in the habit of facing the facts, your diagnoses will be untrustworthy, and much of your therapeutics indefinite. If a man do not learn pathology when he is young, he is not likely to learn it properly later. Unless he have seen much for himself, he will not practically realise the researches of others, nor be able to judge them. I would urge on young medical men the formation of clubs for making *post mortem* examinations. Let me mention a slender personal experience. Many years ago, at York, a club of this sort was formed. It had no other organisation than the agreement that each member should obtain an examination whenever possible, and ask the other members to attend. The history was stated by the person who, so to speak, owned the case, and then each of us saw what there was to be seen, and notes were taken. In this way it is not difficult to get a large experience of direct value for practice. This very simple method has, I suggest, some advantages over reading papers and more elaborate demonstrations, although these are necessary for organisation of know-

ledge. Each learns something, however little, at first hand. And since the whole body, as well as the organ principally diseased, is searched through, it is in some ways very much more teaching than the exhibition of interesting specimens of disease of isolated parts, valuable as that is. Some of the cases we had to deal with were "coroners' cases"; some of them were cases of sudden or of rapid death.* As early as possible in our career we should familiarise ourselves with cases of acute disease; with emergency cases—with cases that will not wait. In some of those we saw, the history was, from the circumstances, imperfect. I do not think that this was altogether a bad thing, for we are called on to give legal evidence in such cases. To take one example: we may be asked to say what caused the death of a prostitute who, when last seen alive, was drunk, and who was found dead in a gutter with a bruise on the head, and about whom there was a doubtful history of a fit. Reasonable or not reasonable, we have to go into the witness-box and say whether we think a patient died of drink, of poison, or of injury, or of disease, or that we do not know of which he died. In some of the cases we dealt with, the patients, if I may use the word, were seen when dead. When called to a dead person the ordinary medical difficulties are over, but very often great responsibilities of other kinds begin.

Pathology is, of course, now well taught in hospitals. But I am urging that a man should go on learning pathology for himself—go on being a student as long as possible. Most of the work on pathology will be done by those who have large and special opportunities. Scientifically I am a democrat, and should like every one to be a pathologist for himself, so far as possible. Many a man learns quickly, and is very zealous when a student; he is, apparently, a man of promise, and then stops short and ceases to improve. Every man should be original. He may make no discoveries; that is to a great extent a matter of chance. I mean that he should think for himself. If he do not himself make *post mortem* examinations, he will have no trustworthy materials for such thinking on cases of living people. It is one thing to possess knowledge, and quite another thing to be possessed by that of other people. Besides, in private practice are seen some kinds of cases we do not sufficiently often see in hospitals. Some of the most useful cases I have seen have been in the private practice of my friends, although I have had a large field for investigation in hospital.

Besides, in private practice work is done under responsibility. Whilst studying at schools, this excellent discipline is not nearly so great. There is nothing like responsibility for sharpening the wits; and *post mortem* examinations will prevent our being too sharp. Coroners' cases are important, not only for the sake of knowledge, but for discipline. A man has to bring his thoughts quickly to a focus. A collection of good reports on these cases would be very valuable for many practical purposes. I do not suppose that our editor has any space to spare, or I would suggest a department in the JOURNAL for brief reports of coroners' cases.

Many *post mortem* examinations are necessary for passing through a kind of inversion of our knowledge—from the systematic to the clinical. Systematic medicine assumes the *post mortem* examination, and goes back to the symptoms; clinical medicine begins with the symptoms, and looks towards the morbid anatomy and pathology. The latter is, of course, infinitely the more difficult.

A man who has learned properly the symptoms of cerebral hæmorrhage, those of uræmia, those of epilepsy, etc., finds the problem turned upside down when he comes to the bedside. These several kinds of cases look very much alike at the bedside. The question at the bedside is not, such as, "Give me the symptoms of cerebral hæmorrhage, of uræmia, etc. That is going from the necropsy back to the symptoms. The question at the bedside is, "Here is a man in a fit—what is the matter with him?" That is looking from the symptoms to or towards the necropsy. No one is ready to answer the latter question even plausibly, unless he have seen or made many *post mortem* examinations. For at the bedside, a medical man has to be original; has to think for himself.

The process of inversion is not altogether pleasant. Indeed, a man who begins with a good systematic knowledge will probably find himself confused during the process of inversion. When, after looking from the necropsy back to the patient, he begins to look from the patient to the necropsy, and sees, for example, a patient who died "with all the symptoms of cerebral hæmorrhage," he is taken aback when at the *post mortem* examination he finds nothing. Let him persevere, and all will come right in time, or much clearer. Nevertheless,

less, I would grant that, if a man wish to be thoroughly comfortable in his diagnoses, he should never make a *post mortem* examination.

A practitioner must not be a pathologist only, although, unless he be a pathologist, he cannot be a good practitioner. Division of labour, strictly or figuratively, is the law of everything whatever. Some of us work chiefly at physiology, some of us at pathology, and some of us at clinical medicine. But to be a good practitioner, a man must know much of all three, and most of us now-a-days carry on the three lines of investigation. Every case is a departure from healthy states, and no one is fitted to begin the scientific study of diseased persons unless he know much of the anatomy and physiology of healthy people. To a great extent, diseases are, metaphorically speaking, experiments, anatomical and physiological, on the human body. If it be not profane to say so, I would suggest that anatomists and physiologists might have more recourse to these "experiments." Charcot, in doing the best kind of pathological work, has, I should suppose, done as much for the anatomy and physiology of the spinal cord as any one living. He is clinical all round. The three things make up the clinical problem. I do not mean that we have simply to draw incidentally on our anatomical and physiological knowledge when we see a patient, but that anatomy and physiology are integral parts of every case we have to do with.

When we stand at the bedside of a patient, when we come close to our clinical work, there is before us an anatomical, a physiological, and a pathological problem. A great part of our clinical knowledge of cases is really nothing more than anatomical and physiological knowledge. The word disease is too vague; it is used in three senses, each of which should be individualised. Since I am about to speak of cases of disease, it is not needful to add, were it correct to do so, the adjective morbid to the terms anatomy and physiology. I can best illustrate by diseases of the nervous system.

In each case: 1. There is alteration of structure of some organ; here is an anatomical problem. 2. There is alteration in the proper functional activity of that organ; here is a physiological problem. 3. There is a change in nutrition of tissues of that organ; here is a pathological problem. In many cases, we cannot carry out this threefold scheme. But we should attempt it in every one, in order that we may realise vividly what it is we do not know. There is a wider pathology than abnormal nutritive changes in one organ. There is not only the organ diseased or most diseased; there is also the rest of the patient to whom that organ belongs, or did belong. I say, "or did belong," because sometimes part of an organ is annihilated. No clinically minded man ever forgets the wider pathology. We no longer believe that a patient is "attacked by disease," although we still use that expression, the metaphysic having long since died out of it. It would be a better metaphor to say that the patient breeds the local disease himself, or that it has grown out of the whole of him; or, better still to say, of many cases, that a certain part of a universally unsound system has fallen to pieces. So we examine our patient all over; we try to get to know as much as we can of the pathology of all important organs of the living patient.

Even yet our pathology is not wide enough. We must not consider the patient himself as more than a detached unit of his family; we have to note the tendencies he inherits, as well as to examine him to see how they are particularly evidenced in one branch or twig of a family tree.

If we take a case of loss of speech, the threefold distinction in clinical investigation can be easily illustrated. To locate the lesion is nothing other than an anatomical conclusion. To ascertain that it has destroyed speech, that it has left the patient capable of understanding what is said to him, etc., is a physiological (and psychological) investigation. Pathology is concerned only with the nature of the lesion and with its mode of production. Nearly all that has been written on aphasia is anatomical, physiological, and psychological disquisition.

To take a still more simple case in illustration: a man has hemiplegia. To think of the case as one of paralysis only, is like the habit of thought of old-fashioned zoologists, who spoke of animals as if they had nothing particular inside them, or, as Forbes said, as if they were skins stuffed with straw. We have three very different things to do, each easily done in most cases of this kind. From noting the region affected—face, tongue, arm, leg, etc.—we conclude that there is a lesion of the opposite corpus striatum. Now, this is only anatomical knowledge. Speaking figuratively, it is only an experiment made by disease on an organ. To locate disease is an admirable thing; but localisation is not the most important clinical thing. If we stop in this stage, we know nothing of any value for rational treatment of the patient; we may have some good empirical expedients. Next, observing that the region mentioned is paralysed, we conclude that there is loss of function of some nerve-elements—probably destruction of them. This—

* I mention with much gratitude the help I had in this, nearly my earliest pathological work, from Dr. Shann, and Mr. S. W. North, of York. I learned many things of inestimable value to me, and especially as to cases of emergency.

physiology—also is a knowledge by itself, of no avail for therapeutical purposes in such a case. But, lastly, by examining our patient all over—for now we have done for the time with his paralysis—and from certain empirical evidence, not needing to be stated now, we conclude that loss of function of the organ is caused by cerebral hemorrhage, that a clot has smashed up part of the corpus striatum. This is not enough. There is yet the wider pathology. A patient looks on this illness as an accident; the pathologist never takes that view of it. We often get to know with certainty that the local pathological change is but a local manifestation of a slowly progressing wide state of degeneration; that the man is rotten all over, and that he has one day broken down suddenly in a certain place; that he has chronic Bright's disease, atheromatous arteries, and an hypertrophied left ventricle. By considering these things and their interaction, we see that he has long been preparing for what seems to him to be only an accident. I submit that the process by which, in such a pathological state, he comes to have the local lesion—why an artery bursts—is in chief part a physiological problem; there is a physiology of the organism made up of bad materials as well as of the healthy organism. Further widening our pathological investigation, we may find that the patient is a twig of a gouty family tree.

Now the pathology of lesions is the basis for treatment. It may be said that we can do nothing for the lesion in the case instanced. I grant that. The more we face the facts, that nerve-fibres are smashed up, and that a mass of blood is lying abroad in the *débris*, the less hopeful we feel. But the knowledge of what we cannot do is a gain for therapeutics. We do not try to do the impossible. We have much to do if we consider the patient's wider pathology. We take care of what is left of him, and do not treat what is lost of him. We must be realistic. A part of the patient is annihilated, and to treat the local lesion is to treat a hole in the nervous system. If the clot be a very small one, the patient rapidly gets well, and then, obviously, the thing of real importance is to take stock of his general condition. In a case of slight and transient hemiplegia, it is good work to localise the lesion, but it is far better to examine the patient's heart, arteries, and urine. A man who has one day's trifling incapacity in speech, or even only a trifling bleeding at the nose, is, if he have any chronic Bright's disease too, in a worse case—is less likely to live, I mean—than a patient completely and permanently hemiplegic from embolism. Although I have spoken of a patient's getting rid of the paralytic effects of small lesions, I did not say we cured him. It is perfectly certain that a person recovers from hemiplegia, although the local destruction is not, or is but partially, repaired. Why recovery, or what anyone calls recovery, occurs in these cases, is an anatomico-physiological question.

Once more I urge that anatomy and physiology are integral parts of our clinical knowledge or directions of investigations of particular cases. In a large class of cases, where the functional affection is not negative, but the exact opposite—morbid exaltation of function—it is imperative to keep the physiology of the cases distinct from their pathology, or we shall not see what the really important medical problems are. If we take the case of epilepsy, we see that the distinction is of practical moment. A man is subject to epileptic fits. He is apparently well; then suddenly he is convulsed all over; next day he is apparently well again. Now, in this case, we cannot yet carry out the threefold method, but we should attempt it. Our anatomical knowledge is defective; we have not yet got to know the organ in fault—the particular part, I mean. Thanks especially to Hitzig and Ferrier, we have got to know much of value as to localisation in epileptiform seizures; but, as to epilepsy, nothing at all certain. But we feel sure of our physiology—that nerve-tissue somewhere is highly unstable, and that it occasionally liberates much energy, or, in other words, discharges excessively. The excessive movement outside is a certain sign of an excessive nervous discharge inside. And since in nearly every patient the fits are always the same in kind, however different in degree, we infer that a group of cells have become permanently abnormal, highly unstable, or, metaphorically speaking, "explosive". What I wish to urge is, that this is only an abnormal physiological state, only a great excess of a normal physiological condition. If a healthy man move a limb, there is a liberation of energy by some parts of his nervous system; and if he be convulsed all over, there is only a great exaggeration of such a normal process. To repeat, our anatomical knowledge of epilepsy is only vaguely inferential. Our pathological information is *nil*. We have only certainty as to the physiological process. If we did know exactly the locality of the abnormal physiological condition, we should still have the pathology to find, but we should know where to search for it. The pathological question, the more important thing for medical men, is this: By what abnormality of nutrition is the local hyperphysiological condition produced? Facts as to the causes of epilepsy are

wide of the mark unless they bear on this one point. Enumeration of the so-called causes of epilepsy is valuable, but the facts gathered are only some materials towards ascertaining the direct pathological causation. The man who finds out by what pathological process it comes to pass that certain parts of the nervous system become so physiologically abnormal that they occasionally discharge excessively, will have done work which has not yet been done—the best medical work on epilepsy. Putting it somewhat too narrowly, the question is: "In what tissue of nervous organs does the abnormal nutritive change begin?" It is often assumed, without evidence, that it begins in nervous tissue.

The illustration from the case of hemiplegia was, as to its pathology, very roughly handled. We must carefully distinguish betwixt direct and indirect pathology. The lesion on which, so to speak, symptoms directly hang, is often very indirectly produced. The distinction betwixt direct and indirect pathology is necessary for precision in prognosis and in therapeutics.

There are very few nervous diseases in the sense that the essential elements of nervous organs, cells and fibres, go wrong primarily; in most cases they suffer indirectly. For precision in pathology, and, consequently, in therapeutics, we do not think of nervous organs as being made up solely of nervous elements. The ingredients of a nervous organ are not only nerve-cells and fibres, but also the subordinate elements, blood-vessels and connective tissue. And nearly all diseases of the nervous system of which there is a known morbid anatomy are diseases beginning in the subordinate elements; the nervous tissues are innocent but suffer. Most of them are not in a strict sense nervous diseases at all. Hence a study of pathology corrects specialism. To take but one case: the commonest nervous system is an arterial affair. Hemiplegia, in the vast majority of cases, is owing to blocking up of a diseased artery or to rupture of one. Nerve tissue is not here in fault, but suffers. It deliquesces (as in softening), or is smashed up by irruption of blood. A man's nerve tissue does not begin to soften; it is often locally starved because its arterial supply is cut off—it is boycotted. If a man can take care of his arteries, he need have but little fear of hemiplegia and apoplexy. His nervous tissues will take care of themselves. If we could conceive a man so badly educated and so curiously minded that he knew nervous symptoms well and nothing else, he would, if he made *post mortem* examinations, become de-specialised. He would find that he must start anew by studying cardiac, arterial, and renal diseases, syphilis, gout, rheumatism, and so forth. Out of that new study he would get a reasonable basis for prognosis, and for the care of his patients. In a word, he would find that the pathology of most nervous diseases was indirect, and would cease to take a merely nervous view of them.

The best illustration of indirectness of pathology is given by that most important clinical group of cases, syphilitic affections of the nervous system. Without denying that syphilis may primarily affect nerve-tissue, what I only feel sure of is, that it begins in subordinate tissues of nervous organs. The most nearly direct method of "attack," if I may use the word, is when a nerve trunk is the seat of syphilitic disease; but in other cases the process is indirect. Thus the commonest kind of so-called syphilitic hemiplegia depends directly on local softening of the brain, and indirectly on syphilis. This is an excellent illustration, showing how pathology gives precision to therapeutics. What a man has really to treat, if he is trying to cure a patient of syphilitic hemiplegia of this kind, is "local softening of the brain," not syphilis. The order is this: a man has a chancre; he gets rid of it and of subsequent secondary symptoms. Months or years later, when apparently well, except perhaps for headache, some of his cerebral arteries are becoming syphilitically diseased, and then one unfortunate day a branch is blocked up, and he becomes hemiplegic. A very little change happens that day, although the consequences are grave; but for that little seeming accident there has been long, slow, insidious preparation. The syphilis is slow, the thrombosis is rapid. This case for treatment, so far as the hemiplegia is concerned, is as certainly one of local softening of the brain as hemiplegia from ordinary embolism is. To call it syphilitic hemiplegia is all very well, but to think of the paralysis as being a direct result of syphilis is crude pathology.

Next as to therapeutics. Suppose the patient recovers rapidly under iodide of potassium, should we say we had cured him? There are the facts that he is syphilitic, that he took the iodide, and is now well again. No one denies this sequence. But, then, it so happens that there is another fact. It is quite certain that some hemiplegic patients get well without any drugs whatever. Whether a patient recovers from hemiplegia or not is a question of the size of the lesion. Those who make *post mortem* examinations do not invoke shrinking or disintegration of the plug, or re-establishment of collateral circulation, because they know that they find holes in the motor tracts of

patients who have recovered from hemiplegia—the patients got well, were not cured. So that the hypothesis that the iodide cured the patient is not warranted by facts. Of course, we should treat the patient for syphilis, for besides more obvious reasons there is the strongest presumption that other cerebral arteries are diseased, and we may rid them of disease. If the paralysis be transitory, we go on treating the patient for syphilis, to prevent further paralysis. Everybody has a well-grounded faith in treatment of syphilis—or of its recent effects at any rate. But I submit that if we could sweep away every vestige of syphilis by drugs, we should not, by so doing, cure the hemiplegia. It is easy to let ignorance stand to us for knowledge. We may believe we have cured our patient by drugs, because we do not know that hemiplegia will pass off without the use of any. But confidence is not always a sign of sagacity, but may result because we have not made numerous *post mortem* examinations. In the case instanced, there is a plug in the vessel, and consequent local softening. For drugs to do anything towards ridding the patient of his hemiplegia, they must help to get out the plug and to restore starved nerve tissue. Now, as a matter of fact, there is in these cases actual destruction of nerve tissues. If we could look into the man's head we should see that a part of his brain is boycotted. We cannot get at it by drugs.

There is another kind of so-called syphilitic hemiplegia, essentially unlike the one already mentioned. The facts are that a man, the subject of syphilis, has pain in the head for weeks—the best time for anti-syphilitic treatment—significant of the formation of a cortical gumma. One day he has a convulsion, very often beginning unilaterally, and after it he is temporarily hemiplegic (sometimes monoplegic). The process by which the hemiplegia results from syphilis is a doubly indirect one. No one supposes that the gumma discharges; but that nerve-cells round about it do. Thus, the stages are—(1) Formation of a gumma; next (2) induction of instability by nerve-cells, exactly as by a glioma, possibly by a sort of encephalitis; then (3) sudden excessive discharge; and (4) consequent hemiplegia.

Syphilitic paralysis of a cranial nerve, and the two kinds of syphilitic hemiplegia, are utterly different in the pathological changes on which the symptoms directly depend, although syphilis is respectively the direct, the indirect, and the doubly indirect cause of them.

There is a class of cases of nervous disease—the neuroses—of which the pathology is unknown: chorea, epilepsy, insanity, neuralgia. In these diseases, little or nothing definite has, according to most authorities, been found *post mortem*; and, curiously, the fact that nothing is found, used to be considered proof that there was nothing to find. The statement that we do not know what there is in a particular disease, is sometimes taken to be equivalent to saying that there is nothing. We call the neuroses functional affections—a term, I submit, which should be kept for physiology. There can be no alteration of function without some material change. A man who does not make *post mortem* examinations may look on slight and transitory local paralysis as not depending on a material change, but if he did make such examinations, he would not hold that hypothesis. I repeat that we do not know the pathology of the neuroses. But now comes the curious point. We speak most confidently of the inheritance, interchangeability, and fundamental community of pathological character of those very diseases of which the ascertained morbid anatomy is nothing, or next to nothing. Thus there is assumed to be a community of nature betwixt epilepsy and insanity. It may be so. For my part, I have not heard of any facts tending to prove anything of the kind. The evidence adduced goes only to prove that many epileptics become insane. The neuroses are spoken of confidently as being nervous diseases in the sense that the pathological changes begin in nervous tissues. Where is the proof when we know nothing of their pathology? There is no proof. Again, pathology prevents our ideas on this subject being out of focus. Suppose a man has epilepsy, or paralysis, or chorea, and suppose that all his relations had the nervous symptom or disease, hemiplegia; is there any proof that he inherits a tendency to a nervous affection—that his epilepsy is owing to his nervous tissues *beginning* to go wrong? Not the smallest; because the hemiplegia is owing to arterial changes. If the family history in such a case tends to prove anything, it tends to show that the pathology of the patient's epilepsy is primarily arterial, and only secondarily nervous.

There is a metaphysical pathology. The cases are those on which we either do not obtain *post mortem* examination, or find nothing *post mortem*. It is rather difficult to define metaphysics. Some people call psychology metaphysics; some call anything very difficult and complex about mind and body metaphysics; some use it merely as a term of abuse. It is, I think, a great pity that some metaphysics is not taught to students before they enter the profession. This may seem a strange remark, but the reason for making it is not to urge that they should

be metaphysical, but for the diametrically opposite reason that they should be less metaphysical. It is a mistake to suppose that those who write books on metaphysics are the most metaphysical. They have, at any rate, the knowledge that they are dealing with metaphysics.....

A good deal under the guise of practicality is pure metaphysics. There was once a man who could conceive an abstract Lord Mayor. The conception he had, so he averred, had neither head, arms, legs, nor corpulence; it was not an image of any particular Lord Mayor, nor a fusion of several, but an abstract Lord Mayor. Well, we think this metaphysician was too confident in his powers of conception. But do we not imagine ourselves capable of the same kind of marvellous feats? Let us look at a case of aphasia. A man does not speak, and yet can understand what we say to him, and can think—on ordinary things, at any rate. These are the facts; no one disputes them. Now comes the metaphysician, who proffers the explanation that the patient has lost words, but retains the memory or ideas of words. There are, it seems, words, and also memories or ideas of words, which latter, somehow, are not words. Now, what is an idea of a word which is not a word? It is, like the abstract Lord Mayor, simply nothing at all.

We should deal with the difficult and the complex in as realistic a manner as we do with the simple. The hysterical patient, who is said to have paralysis of the will, has some material change. What it is we do not know; and we never shall know, if we be content with metaphysical explanations which, in one sense, explain everything, but really explain nothing. I have long expressed the opinion that, for the scientific study of diseases, we should regard them as examples of Dissolution—using this term as the opposite of Evolution. In this way, we shall avoid the errors of confusing the psychical with the physical, and shall steer clear of metaphysical explanations.

THERAPEUTIC MEMORANDA.

HERPES ZOSTER OCCURRING DURING ARSENICAL TREATMENT.

A GIRL of ten years of age came under my care, in December last, for chorea. She was treated by arsenic, in doses of three minims of the solution, three times a day. After taking this for two months, the chorea having considerably improved, a well-marked attack of herpes zoster took place on the right side. There was no pain, nor any of the constitutional symptoms, which often supervene after a prolonged course of arsenic.

Mr. Hutchinson, many years ago, called attention to the fact, that herpes occurs in patients taking arsenic, with sufficient frequency to render it probable that the drug is the cause of the eruption. This is the first instance which has come under my notice, although almost all the cases of chorea which I have treated during the last four years (chiefly out-patients at a children's hospital) have taken arsenic.

The cases collected by Mr. Hutchinson are chiefly patients with other skin-affections; but one of them is a case of chorea, related to him by Dr. Woodman, in which shingles occurred after thirty days' treatment by three-minim doses of Fowler's solution. Dr. Duffin has recorded a case of psoriasis treated by arsenic, in which the eruption appeared simultaneously with the ordinary constitutional symptoms. More recently, Dr. Finlayson has described two cases—one of acne, and the other of chorea—in which herpes occurred during arsenical treatment.

I may add that, in my case, the treatment was suspended on the appearance of the rash; but, in some of the cases previously described, although the arsenic has been continued, the herpes has run its course, and disappeared in the ordinary time.

A. M. EDGE, M.D., M.R.C.P., Manchester.

SURGICAL MEMORANDA.

POSITION IN THE TREATMENT OF DISEASE.

HAVING seen in the JOURNAL for the 15th ultimo an article on "Position in the Treatment of Disease," I was reminded of several cases of inguinal hernia met in my practice, in which, when ordinary means failed, I have easily reduced the protruded bowel by placing pillows under the patient's hips so as to elevate the pelvis, removing the pillow from under the head, keeping the patient in the supine position all the time, and allowing the "force of gravity" to act. A few minutes generally were sufficient to effect reduction easily.

R. FULLERTON, B.A., M.B.