

plied over the thorax in bronchitis, they are of no avail, and for a similar reason.

As every one's experience must have taught him, leeching and cupping over the liver in painful affections of this organ are very often unsatisfactory in their results; but sometimes they give relief; and when they do, may not the cause be ascribed to their influence over the parietal layer of the inflamed peritoneum over the liver. The adhesions between the anterior and upper surface of the liver and the abdominal walls and diaphragm, so often met with in the dead house, are proofs of the not unfrequent occurrence of such peritoneal inflammation, excited by the deranged condition of the liver.

Facts of this kind, and I need not multiply them, seem to me to point out to us the internal inflammations in which we may anticipate that direct bleeding will be of service; and also to explain why it so often fails to answer the objects anticipated from it in the cure of internal diseases. I am aware that I shall be told in answer to this, that, nevertheless, relief is very frequently found in practice to follow the abstraction of blood from the skin in cases of internal diseases, in which there is no vascular connexion between the skin and the internal part; and that a striking proof of this may be observed, for instance, in the case of cupping over the loins in affections of the kidneys. But I would ask, in the first place, whether the practice does not frequently fail to give relief; and, secondly, whether it is certain that the relief, when obtained, may not be often very fairly attributed rather to the rest and other remedies, always, and in most cases of necessity, coincidentally enjoined with the cupping. Are the cupping and the subsequent relief truly cause and effect in every case, or merely antecedent and sequence? This much is undoubtedly true, that the pain in question and the kidney affection are very frequently relieved without any such local appliance, and solely by the use of those most powerful of remedies in these diseases—rest and warmth. Nevertheless, I do believe, that cupping does sometimes, in diseases of the kidney, give local ease, though I could never satisfy myself that the loss of blood had an undoubtedly direct influence over the progress of the kidney disease. I account for the relief to the pain thus given by the leeches or cupping in this way: Inflamed organs very frequently excite diseased actions in the parts around them; abundant proofs of this we have in the adhesions attending pericarditis, hepatitis, and pneumonia. It is, as I believe, to this irritated condition of the tissues around the kidneys, secondarily excited, that the loss of blood gives relief. To suppose that one drop of blood can be thus directly taken from those organs, is of course an entire, though, perhaps, not uncommon, delusion. The proof that the diseased condition of the kidney does excite changes in the parts immediately around the organ, is often in the hands of the pathologist; he not unfrequently finds the cellular tissue around the kidney firmly adherent to its capsule, and united to it by unnatural attachments.

I shall, perhaps, be also told that this explanation of the effects of blood-letting is of too mechanical a kind; and that it takes no account of that nervous element with its many inscrutable mysteries, which necessarily takes part in the operation. I may be told that dry cupping relieves bronchitis and renal congestions; that leeching at the epigastrium and abdomen relieves gastrodynia and intestinal pains, that blisters on the calves of the legs relieve cerebral disorders, etc., and I will anticipate the objection by saying, that if such results do occasionally really follow as consequences of such antecedents, the truth of the main facts which I insist on are in no way invalidated. That irritation of the skin may act in a reflex way upon internal organs, is a fact beyond dispute; and that it may influence the circulation of the blood in internal organs through the medium of the vaso-motor nerves, is very possible; but the influence of such irritation over the circulation of internal organs in disease, has as yet to be demonstrated.

I have said nothing here of the change of type theory of disease, as explanatory of the modern views concerning venesection. Because the subject would lead to a wide discussion; and because, if the views here sustained are correct, we have in them a sufficient explanation of the change in practice, without being forced to call in such an intervening deity to solve the difficulty. According to these views, venesection, rightly used, must have been of service in those other days, just as it is of service in these present days; it was not more requisite, then, in the cure of certain diseases, than it is now. The theories which men held of the nature of disease in the so-called sthenic times of inflammation led them, logically, to abuse the excellent remedy; and the injuries and mischiefs which followed upon such abuse, not noted clearly by them, but

distinctly revealed to us by the light of modern pathology and physiology, and by the touchstone of modern clinical observation, have led us, in late years, unduly to depreciate its value.

The views which I have taken the liberty of thus laying before the society, have necessarily been given in an incomplete form, for it would have been too much taxing the attention of the fellows to have attempted their full illustration. I cannot, however, help adding that this view of the mode of action of venesection is in entire accordance with the modern practice of administering stimuli in certain stages of inflammation, and that it explains away what has been denounced as an unseemly anomaly, viz., the simultaneous abstraction of blood and administration of alcoholic stimuli. The conclusions arrived at, summed up in a few words, are as follows:—

1. Venesection has no *directly* beneficial influence over the course of inflammations, either external or internal.

2. But venesection is at times of great service, *indirectly*, in the course of certain internal inflammations, and in the course of certain internal chronic diseases, and of all disorders which occasion congestion and oppression of the heart.

3. The use of the venesection is in all cases alike. It acts by relieving the cardiac oppression; it neither arrests nor modifies beneficially inflammation.

4. There is a marked distinction to be drawn between the effects of bleeding in inflammation, and the local abstraction of blood from the inflamed part. Local abstraction of blood materially influences the inflammation, reducing the most characteristic of its phenomena. But local abstraction of blood can only modify, in this way, the inflammation of internal parts, when there is a direct vascular connexion between the part inflamed, and the part from whence the blood is taken.

5. It is not denied, that local irritation of an external part, where there is no direct vascular communication between the skin and the inflamed part, may influence an internal inflammation, by reflex action conveyed thence from the skin through the vaso-motor nerves of the inflamed part.

CASES OF NERVE-DISORDER.

By C. HANDFIELD JONES, M.B., F.R.S., Physician to St. Mary's Hospital.

SERIES III.

My intention in publishing the following series of cases is chiefly to adduce evidence in support of the view that nervous tissue, either dynamic or conducting, may be so enfeebled by some obscure influence as to be disabled from performing its function; that, in fact, paralysis or paresis may thus occur without any discernible structural lesion. This view is received and acted upon by many intelligent practitioners, who well know the difference between "anæmic" paralysis and that resulting from organic lesion; but it is not distinctly recognised by our teachers and lecturers, except as one of the freaks of hysteria. Yet, to judge from my own experience, the state in question is by no means rare; at least, in minor degrees. As far as the evidence we at present possess extends, the view above enunciated seems quite undeniable. Dr. Abercrombie's cases of "simple apoplexy" prove it almost conclusively; and I can hardly imagine that microscopic examination would add much to their authority. It is the fashion, I know, with many, to call all cases of disordered nervous action, which are not referrible to structural change or to familiar causes, *hysterical*—a term which is meant to imply at least a certain amount of mental cooperation in the production of the symptoms. To this coarse, careless practice, I beg to take the strongest exception. I affirm, with the fullest conviction, that the sufferer from nerve-debility, or paresis, may have a far better regulated and stronger mind than he who pronounces with so much levity on the nature of the malady he is asked to relieve. Let the strong unbroken man be exposed to the depressing influence of a long series of enfeebling unwholesome occupations, intensified by mental anxiety and unrelieved by relaxation, or to malarious emanations, or any like cause, and it will soon be seen whether he will think hysterical either a sufficient or just description of his *disease*.

The nervous system in the present day is certainly hard worked; and can it be matter of wonder if it often give way, and express its fatigue by actual failure of functional power? I do not, of course, doubt the existence in many cases of defective volition or disordered imagination; I do not wish to erase from our nomenclature the term "hysterical paralysis"; but I

do wish to introduce one which shall express such conditions as result from purely physical disorder, the will still bearing up against the depression. The appellation of *Neurolysis* appears to me a convenient one, or *Neurotaraxis* where the phenomena are more significant of irritation than of simple exhaustion.

The causes producing the enfeeblement of the nervous tissue may doubtless be various, and it will very much depend on the previous state of the system whether they prove effective or not. Damp cold seems to have been the exciting cause in Case II, influenza in Case I, malaria in Case XI, Series I (ASSOCIATION MEDICAL JOURNAL, 1855). Professor Trousseau believes the poison of diphtheria, by its general action on the system and on the blood, to give rise to the paralysis which is often observed as a sequel of the disease.

The diagnosis of functional paralysis must often be very uncertain, and a cautious tentative proceeding may be the only means of deciding. Further experience is requisite to enable one to lay down general rules. However, the absence of pain and tenderness in the head or spine, of signs of central irritation, and of disorder of the intellect; wasting of the muscles; and previous general debility, with exposure to enfeebling causes,—would be evidence in favour of the existence of functional paralysis rather than organic. Reflex movements are not usual, I think, in neurolysis; but they occurred in Case IV.

In dealing with functional paralysis, it is of great importance to commence treatment early. In conjunction with appropriate tonics, the local application of an interrupted galvanic current to the muscles has appeared to me far the best means of restoring their power. This recommendation of early treatment coincides with that given by Dr. West in his lecture on *Infantile Paralysis*—an affection, I apprehend, often identical with the one we are considering.

CASE I. W. D., aged 26, male, was admitted into St. Mary's Hospital on September 10th, 1858. He was ill one month with an inflamed sore-throat, which has got quite well; but since then he has begun to lose the use of his limbs, the legs being weakest. He can now stand, but not walk; the legs are very numb and cold, but he is conscious when they are touched. He has no pain at all. The intellect is clear. No reflex actions are excited on tickling the feet. The pupils are large, and scarcely act at all. The skin is cool; the pulse natural; the tongue white; he has no cough; the sphincters act. The heart-sounds are natural. He has never had syphilis. He lives in a damp locality. He can lift up his legs in bed. Being unable to find any indication of organic disease of the nervous centres, I determined to try the effect of nervative tonics, and prescribed the following.

R Strychniæ gr. 1-20; quinae disulphatis gra. ij; acidi sulphurici diluti m̄viii; aquæ ʒi. M. Fiat haustus ter die sumendus.

Ordinary diet and porter were ordered.

September 14th. He feels much better, has more use of his legs and hands, and can walk a little. The bowels are open.

September 17th. He has been worse since September 15th. To-day he has sore-throat, and much diarrhœa, with copious perspirations. His appetite is lost. He has "no use of the hands or legs again". The medicine was repeated, eight minims of tincture of opium being added to each dose.

September 18th. His tongue is very much furred, yellowish; pulse 120, full and weak. He has been purged three times this morning. There is no pain anywhere. He feels very sick, and has a bad headache; the skin is hot and moist. He cannot raise or turn himself in bed. The medicine was ordered to be taken four times a day; and six ounces of port wine were given.

September 20th. He complains of bad cough. Pulse 96, full, occasionally intermittent. The bowels are quiet. Two grains of disulphate of quinine were added to each dose of the medicine.

September 21st. He has had no sleep, from cough. The pulse is natural, 88. The sputa are of rusty colour, but watery. The tongue in the morning was dry, and brown in the centre; at 2 P.M., moist, and coated in the middle.

September 22nd. He feels much better; his appetite is improving. The bowels have not been open for two days. The tongue is cleaner. He has no use of his limbs.

September 24th. He has no use of the hands: they feel numb, but are sensitive if touched. He can sit up in bed better. The tongue is clean. Galvanism was ordered.

September 25th. Galvanism was applied yesterday with

great benefit: he is able to move his hands and arms much better. His appetite is very good: he sleeps well.

October 4th. The legs are now more weak and numb than the thighs. A much stronger current is necessary to excite the leg-muscles than those of the thigh. The calf-muscles are greatly wasted. The feet used to be very cold and numb, but are not so now. The hands feel weak, and are rather numb; he has a sensation of "pins and needles" in them at times.

R Strychniæ gr. 1-16; tincturæ ferri sesquichloridi m̄xv; acidi hydrochlorici m̄ij; aquæ ʒss. M. Fiat haustus ter die sumendus.

October 8th. Great benefit has resulted from the galvanism, and he can now walk and feed himself. He cannot bear so strong a current as before.

The same treatment was continued till the end of November, when he was discharged, with only some degree of weakness remaining in the legs, which was referred to the knees. It seemed that the extensor muscles of the thighs had not regained their full power: he was able, however, to walk very well, and his arms had become quite strong. The dose of strychnia was gradually increased to one-ninth of a grain three times a day. A curious circumstance observed by himself was, that his feet swelled during the night, so that he could not get his boots on in the morning; but, after walking about the ward a little, the swelling subsided. His urine was free from albumen, and was otherwise healthy, except that, on one occasion when he had catarrh, it deposited a good deal of uric acid.

REMARKS. It seems to me impossible to regard the paralysis occurring in this man in any other light than as a functional disorder. Whatever change may have actually taken place in the grey matter, or in the nerve-tubes, it surely was not one of irremediable lesion. No softening, or extravasation of blood, or even inflammation, can, I think, have occasioned the symptoms. The paralysis, after a brief amendment, recurred, and became greatly worse under our inspection. This relapse was attended with all the marks of great general debility and prostration. A passive flux took place from the skin and the pulmonary membrane. Under a sustaining treatment he passed through this state, and began to recover power. In promoting the return of muscular strength and voluntary power, the galvanic stimulus was of the greatest use. It seems to me most reasonable from all this to regard the paralysis as the result of simple debility of the nervous tissue, probably of the spinal grey matter. The brain seems to have been unaffected; there was no giddiness or impairment of intellect, or of the special senses. The cord must have been enfeebled in a considerable extent, at least as high up as the origin of the brachial nerves. Sensation and motion were both impaired, the latter most. The arms recovered much sooner than the legs, contrary to what usually occurs in cerebral palsy. The swelling of the feet during the night is to be ascribed, I believe, to weakness of the vaso-motor nerves, aided by the relaxing warmth of the bed, causing undue dilatation of the arteries, and some subcutaneous effusion. This occurrence of nerve-debility during sleep is often observed, and accounts for such phenomena as the more frequent happening of epileptic paroxysms, hooping-cough, and asthmatic attacks, as well as diarrhœal, choleraic, and malarial seizures, during the night. The case may briefly be entitled as an extreme instance of the usual debility of influenza.

CASE II. J. Edwards, aged 30, young looking, was admitted October 11th, 1858, under Dr. Sibson. Four months ago, he was in wet grass all the morning. His legs became stiff the next morning, and his knees weak; he felt as if he were standing on pins. The arms also became affected, but only numb. Subsequently, his masticating powers and his voice became impaired; he had double vision for about a month, and afterwards singing noises in his ears. On admission, he was observed to lift the jaw with his hand in speaking, and to shoot out his tongue involuntarily. He cannot raise his arms about his head when he lies down. He walks slowly, lifting his feet. When he gets on his bed, he does it in a series of jerks, and then falls flat. Both hands are sensitive. The pupils act. He has vomiting. Pulse 64; respiration 32.

October 14th. He complains of pain at the back of the head, and of the arms being more powerless. He cannot open and shut his mouth without the help of his hand, when lying in bed. The power of deglutition is impaired. Memory is perfect.

October 17th. When his wife came into the ward, he went to meet her, pointed to his throat, and nearly fainted. He be-

came excessively pallid; his skin was hot, and covered with sweat. Pulse 120. Soon afterwards, he sank and died. He had taken iron and quinine, and improved somewhat for a time.

POST MORTEM EXAMINATION. The body was spare, of medium height. The thymus gland was enlarged; it weighed 537 grains. The heart was full of fluid blood, and healthy. The lungs were healthy. The liver, kidneys, and spleen, were immensely congested, but firm, and quite normal in appearance. The stomach was almost empty. The intestines were empty and contracted. The superficial vessels of the brain were slightly congested. The brain substance was firm; the grey matter was darker than usual; the vascular points were more numerous and evident. The lateral ventricles contained a little serum. The choroid plexuses were rather congested. Nothing abnormal was found in the brain; all the parts were very carefully examined. The spinal cord appeared to be firmer than usual in the part corresponding to the lower dorsal and upper lumbar regions; the upper part was much softer than this, but sections of both these portions revealed nothing unnatural in appearance or worthy of remark. Mr. Gascoyne, who conducted the autopsy, informs me that he considered the softening as a *post mortem* change. Dr. Sibson and I carefully examined portions of the spinal cord and pons Varolii the next day under the microscope, but we could discover no organic morbid change; there was no alteration of the coats of the minute vessels, nor glomeruli. The kidneys were also examined microscopically, and found healthy.

REMARKS. In this case, the nerve-disorder was diffuse, but incomplete; it seemed to affect all parts of the cord and the medulla oblongata, but did not cause any actual paralysis till the last. Even the centres of vision seem to have been temporarily disordered. The fatal event probably resulted from arrest of the heart's action, induced through the vagi. The impairment of masticating power shows that the seat of origin of the fifth pair was involved in the paralyzing morbid action; and, from the proximity of that of the vagi, it cannot be matter of surprise that the latter nerves should come to suffer also. As to the existence of any organic alteration, the negative evidence to my own mind appears satisfactory, but I do not expect it may be fully so to all. At the least, however, the case seems well worth recording, if only as adding something to the probability of the existence of central dynamic changes, which may induce the gravest peril, without leaving their record in any organic alteration. (I wish to acknowledge gratefully the kindness of Dr. Sibson in allowing me to make use of this case.)

CASE III. J. C., aged 30, male, was admitted December 27th, 1858. He had been ill two days. He went to sleep on the 25th for one hour, and, on waking up, found he had no use of the left hand. He is not aware that he had pressed his arm in any way while lying asleep. He was quite sober at the time. He has but feeble grasping power with the left hand. He experiences sensations like "pins and needles" in the thumb only. The left hand and wrist are cold and dry, and do not get warm and perspire like the other hand. The tongue is clean; the appetite good; the bowels open. He feels in good health.

℞ Quinæ disulphatis gr. iij; strychniæ gr. 1-20; acidi sulphurici diluti g. i; aquæ ʒi. M. Fiat haustus ter die sumendus.

September 31st. On being galvanised, he soon became faint, but found immediate benefit in the arm. He seems to be a weakly nerved man.

℞ Anamonæ sesquicarbonatis gr. v; tincturæ cinchonæ ʒi; spiritûs ætheris sulphurici compositi ʒss; infusi cascariæ ʒi. M. Fiat haustus ter die sumendus.

January 3rd, 1859. He had been galvanised three times: he can grasp much more strongly. There is much less numbness, but the wrist still feels weak. The mixture was continued; and he was ordered to take a scruple of saccharated carbonate of iron three times a day.

January 5th. He is further improved; the hand and wrist now get warm and perspire like the other. He ceased attendance.

[To be continued.]

ST. BARTHOLOMEW'S HOSPITAL. An endeavour is now being made on the part of the treasurer of this admirable institution to convert the large gravelled airing ground forming the quadrangle of the hospital into a garden, for the benefit of the patients; the shrubs already planted appear to thrive very well.

A Course of Lectures ON URINE, URINARY DEPOSITS, AND CALCULI.

DELIVERED AT THE PATHOLOGICAL LABORATORY,
DURING THE SESSION 1857-58.

BY

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LECTURE I.

Note-Book; Conical Glasses for examining Urine; Quantity of Urine; Colour of Urine; Smell of Urine; Clearness or Turbidity; Consistence; Deposit; Specific Gravity; Methods of taking the Specific Gravity; Reaction; Acid Urine; Alkaline Urine, from the presence of Volatile or Fixed Alkali.

[Concluded from page 272.]

Specific Gravity. By ascertaining the specific gravity of a specimen of urine, we are enabled to form a rough estimate of the quantity of solid matter dissolved in the fluid; and, by measuring the entire quantity of urine passed in the twenty-four hours, we have data for judging approximately of the quantity of solid material removed from the organism in this secretion in twenty-four hours.

The specific gravity of healthy urine may be considered to be about 1015, and the quantity of solid matter passed in the twenty-four hours at from 800 to 1000 grains. It has been considered sufficient to calculate the quantity of solid matter from the specific gravity by multiplying the number over 1000 indicating the specific gravity, by about 2.5. The result will give an approximation to the quantity of solid matter in 1000 grains of urine. This calculation is by no means correct, and is useless in careful investigations. Its inexact nature is shown by the fact that three very different numbers have been proposed, namely, 2.58, 2.33, and 1.65. When it is considered how widely different the composition of the solid matter may be in various specimens of healthy urine, it is obvious that results obtained in this manner must often be very wide of the truth. Take, for example, *albumen* and *common salt*. A fluid containing 136.4 grains of the former in 1000 grains will have a specific gravity of 1030; while one containing only 80.0 grains of common salt in the same quantity will have a specific gravity of 1064. This clearly shows that any attempt to calculate the quantity of solid matter in an animal fluid cannot be very exact. In investigations, therefore, where any approach to accuracy is required, we must evaporate a given quantity of urine (1000 grains) to dryness, at a low even temperature, and weigh the solid matter. In practice, this operation takes some time; and physicians are compelled, as a general rule, to be content with taking the specific gravity. In many cases, the information gained by this simple operation is very important. Thus the urine may be not more than 1002 or 1003—a condition commonly met with in hysteria. A patient may be continually passing urine of specific gravity 1010 to 1012, which is commonly the case with albuminous urine passed by patients suffering from certain chronic kidney diseases. Urine containing a very large quantity of urea, so much that crystals of the nitrate of urea are formed upon the addition of nitric acid (*excess of urea*), usually reaches 1030 or a little higher; and, in cases of confirmed diabetes, where very large quantities of sugar constantly escape from the organism, the urine has a specific gravity of 1035-1040, or even higher.

Methods of taking the Specific Gravity. The specific gravity of a fluid is obtained most correctly by ascertaining the weight of equal bulks of the fluid to be examined and distilled water respectively. For this purpose, a small bottle, with a tubular stopper, holding exactly 1000 or 500 grains of distilled water, at a temperature of 60°, is the most convenient form of apparatus. All that is necessary is to fill the bottle carefully with the urine, wipe it dry, and then weigh it, after having counterpoised the bottle. The number of grains which the fluid weighs is the specific gravity in the case of the 1000-grain bottle, double the weight for the 500-grain bottle, four times the weight for a bottle holding 250 grains, and so on in like proportion.

This method, although perfectly exact, and readily per-