of the *ascending frontal*, *ascending parietal*, and *postero-parietal* convolutions. The tumour had grown downwards as far as the floor of the lateral ventricle, compressing the brain-substance in its course. It also impinged on the other hemisphere. The point of origin of this tumour, and the mode of growth, warrant the conclusion that the paresis of the left leg, which was the first and for a long time the only motor symptom, was due to the affection of the cortex in the region where experiments in the monkey would lead us to localise the leg-centres.

I will mention, lastly, two cases reported by Bourneville, which, though not uncomplicated cases of crural monoplegia, yet deserve to be taken in consideration with the others. They might also be referred to as instances of partial hemiplegic epilepsy. The first of these* is quoted by Charcot and Pitres. This was the case of a girl, aged 18, who had been affected with infantile hemiplegia of the left side at the age of four. She was subject to epileptic attacks, beginning in the left leg, which was paralysed. There was no facial paralysis. After death, a patch of degeneration was found in the right hemisphere, occupying the upper half of the ascending frontal, the bases of the first and second frontal, the anterior part of the postero-parietal lobule, and the whole of the internal aspect of these regions (fig. 17). Secondary

Fig. 17.

degeneration of the motor tracts of the spinal cord existed. The second case was a similar one of infantile hemiplegia, followed by partial epilepsy; the spasms being limited to the right leg, but gradually invading the right arm and the right side of the face. The position of the lesion, diagnosed during life, was the *upper extremity* of the (one *cm.* in extent) *ascending frontal and parietal convolutions*, and the internal surface corresponding, or paracentral lobule. This region was the seat of softening. No other lesion existed. In this case also, secondary degeneration was found in the right half of the spinal cord, in the postero-lateral column.

These cases, I think, suffice to prove that the lesions which have been described in connection with crural monoplegia very closely agree with the position of the centres for the movements of the leg in monkeys. In the absence of other cases of lesion serving to determine the exact position and extent of the leg-centre, M. Bourdon† has endeavoured to arrive at this by another method. The method he adopts is to examine the brain in cases of amputation or arrested development of the lower extremity, with the expectation of finding atrophy of the corresponding centre, in accordance with Luys' views. Whether these expectations are altogether justifiable may be questioned.

But, theoretical considerations apart, the practical difficulties in the way of a satisfactory solution of this problem are very great, and I do not think that, up to the present at least, they have been disposed of successfully. An important communication on this subject was recently presented to the Société de Biologie (January 5th, 1878) by M. Féré, well known for his valuable researches in cerebral topography. M. Féré shows that neither the method of ascertaining the existence of atrophy, by comparing the relative size of homologous convolutions in each hemisphere, nor that founded on the advance or recession of the fissure of Rolando (round which the motor centres are grouped) is reliable; inasmuch as in the perfectly normal brain there are great variations and want of symmetry in the convolutions and fissures in the two hemispheres, and there is no absolutely constant relation between the position of the fissure of Rolando and a certain cranial region, or in its position in the one hemisphere as compared with the other. Hence

the necessity of receiving all statements with regard to atrophy with extreme caution, unless substantiated by a thorough microscopical investigation of the parts said to be so affected.

With these considerations, I will now give the results of M. Bourdon's analysis of cases of amputation. In one of amputation of the thigh, thirty-five years before death, atrophy was said to exist at the upper extremity of the ascending frontal, at its junction with first frontal convolution of the opposite hemisphere. In a second, of amputation of the thigh, fifty-two years before death, atrophy was said to exist in exactly the same situation. In a third, of amputation of the thigh, twenty-one years before death, a linear depression was found interrupting the continuity of the second frontal convolution in the opposite hemisphere.

To these cases, which M. Bourdon quotes from M. Luys, a fourth is added, observed by M. Landouzy. This was a case of arrested development of the right leg at the age of eighteen months, death occurring at the age of forty-five. The left hemisphere was smaller than the right, and the fissures of Rolando were unsymmetrical. The left ascending parietal convolution appeared somewhat smaller than the same convolution in the right hemisphere. There was also asymmetry of the pons and medulla, the left side being the smaller.

None of these cases appear to have been investigated microscopically. If we regard them as satisfactory, which I think we are scarcely entitled to do, we do not find any more exact correspondence as regards the situation of the leg-centre, than in the cases of lesion already mentioned. I think, however, that, though the exact limits are not defined, the evidence points to a position closely in harmony with that defined in the brain of the monkey; and that the conclusion formed by M. Lucas-Championnière is well justified, viz., that, to expose the leg-centre, it is necessary to apply the crown of the trephine over the upper extremity of the fissure of Rolando.

[To be continued.]

CLINICAL OBSERVATIONS

ON A

FAILURE IN THE ANTISEPTIC MANAGEMENT OF A CASE OF EMPYEMA.

By JOHN DUNCAN,

Surgeon to the Royal Infirmary, Edinburgh.

GENTLEMEN,—On November 20th, 1877, Dr. Balfour sent to us for operation a patient who had been admitted to the medical house, suffering from empyema. The vicissitudes in her history since she came under our observation deserve your attentive consideration.

Four years ago, she had a severe attack of pleurisy, from which she never completely recovered; but she dates her present illness from exposure to cold six months before admission. Pus was three times drawn from the chest by means of a Bowditch's syringe while she was under Dr. Balfour's care; but the relief was only of temporary duration. On November 23rd, I made a free incision between the sixth and seventh ribs in the axillary line, and evacuated a quantity of pus, which my house-surgeon (Mr. Black), partly from measurement, estimated at more than three hundred ounces. Those of you who were present will remember the extraordinary force with which the matter was projected the moment the cavity was opened. For the next month, the progress of the case was all that could be desired. The temperature, which, before the operation, had varied between 99 deg. and 101 deg., fell to normal, and the highest reading recorded is 98.8 deg. The discharge rapidly diminished until only about a tablespoonful daily (probably less) of sero-purulent fluid came away by the drainage-tube.

On December 23rd, we dressed the wound under the spray from a new steam spray-producer, which we afterwards found had been discharging a carbolic solution of about one to a thousand, on account of the imperfect boring of the nozzle. You may recollect that I then speculated on the probable consequences, and it is certain that my fears were speedily realised. Next evening, the temperature stood at 100.6 deg.; and perhaps I cannot give you a more graphic description of the condition of affairs than by reading to you the morning and evening temperatures of the next few days.

		M.	E.
December 24th	...	98.4	100.6
" 25th	...	100.6	102.2
" 26th	...	100.8	103.2
" 27th	...	100.9	104.3
" 28th	...	102.4	102.4

* Gazette Médicale de Paris, December 1876.

† Recherches Cliniques sur les Centres Moteurs des Membres. Paris, 1877.

At the same time, the discharge at once became thickly purulent, and, in three days, had increased to fifteen ounces *per diem*.

I need not detain you with a further history of the case. It is the record of a struggle to perfect the drainage by elaborate washings out, by various forms of tubing, and by suction-apparatus. With it all we have only been partially successful. The patient is now doubtless recovering; but her wasted condition shows the drain upon her system made by the constant copious discharge and by the feverishness, which we have never been able entirely to subdue.

There are many points in the case to which I should like to draw your attention. I should like, for example, to compare with you the effects of great tension in this and other cases on inflammatory action and temperature, and to discuss the best methods of securing efficient drainage of these large cavities. But, for the present, I think it better to confine my remarks to one subject: the bearing of the case on Mr. Lister's method of treating wounds.

And, first, it appears to me to bear a remarkable testimony to the value of the method, and to the truth of the theory on which it is founded. Let us look at the facts. After a month of uninterrupted progress, we recognise a defect in our manipulations; are alarmed for the result, and have our fears speedily and very seriously confirmed. I cannot escape the conclusion that there was here a relationship of cause and effect, and, if so, it is a proof of the practical value of antiseptics more convincing than many records of successful cases which possibly may be paralleled under other modes of dressing. But, as if to make assurance doubly sure, on the same day, the same spray-producer was used while I excised a mamma; and in this patient also the signs of putrefaction were speedily apparent. It becomes impossible to suppose that mere coincidence could account for the double sequence. But I think our case carries us farther. Most surgeons admit that antiseptic dressings are perfectly wholesome and safe; but a curious variety of attitude is assumed with reference to the special advantages claimed for them, and the reason of their success. Some hold that carbolic acid acts by diminishing the vital activity of the textures; others that it alters, as smoking a ham does, the conditions favourable to the development of the bacteria of putrefaction. But, if it be admitted that the defective spray was, in the case before us, the cause of the evil results, what other hypothesis can be framed to account for it than that on which the antiseptic system is founded? How, for example, could the spray modify the vital actions or alter the developmental conditions of the parts within the thorax which it never reached? If bacteria and putridity came from within (and it is impossible to deny that sometimes it may be so), how could the pumping of a more or less carbolised spray outside the chest-wall affect their presence? Or, if the noxious agent came from without, what other constituent of the air than organic germs could be thus affected? In short, if the spray be of real value—and these cases seem to prove that it is so—the conclusion seems inevitable that putrescence is the chief of surgical evils; that it is the product of bacteria, and that, in the immense majority of cases, bacteria come from without.

But now, gentlemen, having looked at our case from one point of view as a striking illustration of the truth of the antiseptic system of surgery, I think we may also learn some lessons from it as an example of the failure of the system.

As a practical method, there can be no doubt that this is a weak point. Success depends on careful attention to a series of minute details, the failure of one of which vitiates the whole procedure. Like a chain, its strength is equal only to that of its weakest link. Under such circumstances, occasional mistakes are as certain to occur as occasional accidents on a railway; and, from the same causes, the deficiencies of our mechanical appliances, and especially the fallibility of the human agency concerned in the manipulations. I need hardly say that a thousand errors are no proof of the necessary falsity of the system. If, by other means, it be proved true, practical success or the reverse bears only on its application and applicability. But frequency of failure might necessitate its abandonment, or at least be a strong argument against its usefulness. It would necessitate abandonment if, in failure, the system proved injurious. It does so under certain circumstances. In compound fracture, if the injection be successful, the subsequent progress of the case is most remarkable in many ways, and singularly beautiful; if it fail, my experience is that the effect is very disastrous; and it is often a most difficult problem to determine whether or not to treat antiseptically when some hours have elapsed after the injury. But, in the vast majority of instances, no such question arises. If antiseptics do not prevent putrefaction, the worst that can be said of them is, that they are useless. Nay, I believe that, in some respects (I need not enter into details), they are absolutely beneficial to the patient, and are certainly advantageous in preserving the general amenity of the atmosphere.

But yet again, frequent failure might make antiseptics valueless in comparison with the cost, the labour, or the success of other and incompatible methods. I say incompatible, because, of course, whatever may be advantageously combined with this method ceases to be a rival to it. In this connection especially, the question of frequency arises. Men are not prone to publish their mistakes; but I believe that a statistic would be useful showing the frequency with which antiseptics fail in the hands of men of ordinary intelligence, who are determined to carry out the system on the basis of its fundamental hypothesis.

Here let me digress a little to point out to you certain difficulties which stand in the way of obtaining such a statistic. There is an indubitable difficulty in determining what are and what are not aseptic cases. There is a broad margin of doubtful ground which prejudice on this side or that is prone to arrogate to itself. Let us consider the grounds on which the distinction is to be made. It is evident that neither the general condition of the patient nor the healthiness of the wound can be accepted as criteria for such a comparison. That would simply beg the question. We must have distinct evidence that the discharges are or are not septic. Now, this is not so simple a matter as you would at first be inclined to suppose. The odour is not a reliable test, and for two reasons: one is that many aseptic cases, from chemical changes in sebaceous or other secretions, from reactions between these and the contents of the dressings, and from other causes, produce odours which, to say the least, are often exceedingly disagreeable; another is that the injurious products of putrescence, although frequently associated with disagreeable odours, do not themselves emit them, and are not their necessary concomitants. In five cases, I have failed during the past *semestre* to preserve wounds from putrescence by means of carbolic acid. In the case which affords the text for these observations, and in one other, there was no disagreeable odour whatever, although the discharge was very copious. In the mamma to which I have already referred, the most odious butyric smell pervaded everything. In a fourth case, that horrible smell, which is attributed to indol, could not be prevented with the utmost care; and, in a fifth, yet another variety was unpleasantly perceptible. I feel tolerably certain that some odours indicate putrefaction, and I think it likely that education may do much to produce an appreciation of the differences which exist between them and others, just as it enables some men to tell with remarkable certainty the vintage of a first-growth claret. But I feel equally sure that putridity may produce its noxious results without thus manifesting its presence; and I frankly confess that, in a large proportion of our cases, I do not succeed in coming to a conclusion by the unaided sense of smell.

But you will say the microscope should enable you to put your mind at once at rest. To a certain extent, you are right; but it is not so easy as many of you might be inclined to think. There is a class of spontaneously moving bodies, in shape and appearance closely resembling bacteria, which occur, it seems to me, with equal frequency in discharges which, on other grounds, might be pronounced septic and aseptic. I think distinctions may be seen in their refraction, the character of their movements, and their uniformity of size; but I own to having at first received a shock to my beliefs in watching these bodies. I fancy, however, that we have in acetic acid a means of distinguishing them more definitely. I had heard that this method had been tried by others, and I have been pleased to find that, if you apply it to a slide on which vigorous bacteria from putrid water are disporting themselves, you only slowly arrest their movements; but, if you bring it into contact with the bodies of which I speak, they rapidly dissolve and disappear. I have not yet satisfied myself by a sufficient number of observations; but I believe this to be the case, and, if so, the microscope becomes a reliable test: only, however, negatively reliable. There are septic discharges in which you will find no bacteria. I have watched discharges daily in which they were abundant, and have frequently found that they diminish in size and number, and ultimately become completely invisible to the ordinary powers of the Hartnack's microscope. In our present patient, for example, it is only within the last few days that the field of view has not swarmed with moving rods. This is coincident in her with general improvement; and I think it corresponds usually with a return to health, a reacquisition by the tissues of that power whereby the multiplication of these low forms is prevented in them and in contact with them. But, however that may be, you will see that we cannot absolutely decide by means of the microscope between septic and aseptic cases. We must assist it by other means, of which doubtless there are many. Thus the presence of sulphuretted hydrogen may be detected by chemical measures or indicated by blackening of the protective. But it is not pathognomonic; it is not always present with bacteria, and it may be present when they are absent; and the same may be said of ammonia, on which some have laid great stress. In short, I suppose (at least until the law

against vivisection is improved away) that we have no certain, clear and easy test for the pyrogenous and phlogogenous products of the growth of bacteria, simply because we do not know them, but only their frequent concomitants.

Well, then, you can understand that, when I say it is desirable to have some idea of the frequency with which antiseptic treatment fails, I desiderate what is not easy of attainment. Having no absolute test, we attain certainty of asepticity only by ascertaining that the testimony of all the tests I have mentioned is coincident. Now, I do not think that the statistics which have been published fulfil this rigorous requirement. They have been used to show the general goodness of results under antiseptic management, and doubtless in that view they have a certain value of their own. And here let me say that the idea which appears to possess some men, that the questions which underlie the antiseptic method, or its value, can be at present determined by general and comparative statistics, seems to me curiously erroneous. In those confusingly able lectures of Dr. Burdon Sanderson lately published, much stress is laid on the abolition, in certain German clinics, of septicæmia and pyæmia. Such facts are of some value, I admit; but, when we consider the mass of septic cases that necessarily remains in every hospital, and the uncertainty, which I have just pointed out, as to the proportion of failures, it is evident that other factors (better drainage, greater cleanliness) have also been at work; and I will venture to add that antiseptic management, although I hope and believe it will enormously diminish mortality in the cases to which it is applicable, can never abolish the septic results of surgery. And so, on the other hand, for similar reasons, the demand (made, I think, by Mr. Holmes) to compare the statistics of eclectics and antiseptics can lead to no conclusion whatever, but only to confusion and error. The more limited inquiry which I suggest is, perhaps, more feasible and useful; and I hope, at some future time, to make some contribution to it. Meanwhile, I am not in a position to do so, because I have not myself invariably applied the rigid tests I have mentioned. I do not believe that the frequency of failure will be found to militate much against the usefulness of the antiseptic method. I feel assured (inasmuch as I know of only five cases in which I have not succeeded when I might fairly have hoped to succeed during the past six months) that the percentage of failures, taking into account certain dubious cases, is not greater than ten per cent.; and that, with more experience, I shall be able still farther to reduce it. There is nothing mysterious or difficult in the process; but, as I have said, it involves perpetual attention to minute details on the part, not only of the surgeon, but of his assistants; and a certain number of mistakes will, therefore, always occur, the probable proportion of which it would be well to ascertain both generally and for the varying sizes and kinds of wounds.

Hence arises also another interesting point of inquiry. To what extent may we class our failures under the heads avoidable and unavoidable? How much may be due to errors of manipulation? How much to other causes? We do not yet know whether, in the human subject, putrefaction may sometimes come from within. Most certainly, such a result must be exceedingly rare. But I have not myself the slightest doubt that there is great variation in the susceptibility to inoculation of different patients. We know, of course, that, according to the constitution of the patient and the character and locality of the lesion, the effects of the contact of putrescent material vary infinitely. In this empyema, we have seen much suffering result; in the excision of the mamma, none at all; and, in the abscess of the tibia, very little. But I think there is reason to believe that gross errors of management may sometimes be made with impunity; while, in other instances, the slightest flaw inevitably leads to putrescence; that, in short, the soil is sometimes so unfavourable, that many germs may fall upon it and die; while, in other cases, one will be sufficient to insure luxuriant growth of bacteria.

CASE OF ACUTE PHOSPHORUS-POISONING.

By W. T. MARTIN, M.D., Surgeon-Major.

R. G. B., AGED 31, servant, of active and moderately temperate habits, was admitted to the Royal Victoria Hospital, Netley, at 10.30 A.M. on December 18th, 1877. He stated that he had had liver-disease in India seven years previously, when serving in the 66th Regiment; and that he had been ailing for about ten days. On admission, he was very weak, anxious, and depressed; appeared drowsy, but complained of sleeplessness. No spots were observed on the skin, which, with the conjunctivæ, was of a distinct yellow tinge. The tongue was coated with blackish fur; bowels constipated; belly tympanitic; abdomen

and right hypochondrium slightly painful on pressure; area of hepatic dulness not increased (diminished, if anything); and there was no vomiting. The condition of the respiratory system was normal; pulse 88, weak; temperature in the axilla 98 deg. He had passed urine. The case being regarded as one of blood-poisoning by bile, he was ordered pills containing calomel, scammony, and colocynthis, a warm bath, turpentine fomentations, and milk diet. In the afternoon, the temperature rose to 99 deg.; but was not afterwards recorded, as, when visited in the evening, he was sleeping quietly.

December 19th, 10 A.M.—He was much worse, lying on his back in a state of stupor, with eyelids closed, pupils dilated, and insensible. The skin and conjunctivæ were more deeply jaundiced; the extremities were cold and bluish, and a rash (petechial?) was noticed on the forehead. The pulse was 84, very weak and intermittent; respirations 48, without stertor; temperature 98 deg. The tongue was coated with black fur; the lips covered with sordes; the breath was offensive; the belly was more tympanitic; the bowels were still constipated, and the bladder was slightly distended with urine, which, on withdrawal, was found to be of a smoky colour.* The patient's condition strongly reminded one of typhus. Four grains of calomel were placed on the tongue; hot turpentine fomentations were applied to the abdomen and legs; the nape of the neck and loins were freely blistered with the strong liquor ammoniæ. An enema of castor-oil, turpentine, and tincture of assafoetida brought away a considerable quantity of black pulsatous feculent matter. After a transient rally, during which he took some beef-tea and brandy, the stupor increased, and the patient died, without convulsions, at 5 P.M., having just previously vomited a quantity of black matter similar to what was afterwards found in the stomach.

The following particulars regarding the case prior to his admission to hospital have been kindly furnished by Surgeon-Major Watts.

"He applied to me on December 12th, complaining of being unable to retain anything on his stomach; that whatever he took in the way of food was at once vomited. His pulse was quiet; his tongue a little furred; and I was inclined to attribute the symptoms to the effects of spirit-drinking. I ordered him a sinapium to the epigastrium, and an effervescing mixture with hydrocyanic acid. On the 14th, he presented himself at the office, and had his medicine repeated; he still complained of vomiting after food, pain in the stomach, and of feeling altogether ill. On the morning of the 16th, I visited him in his room; he was complaining of great pain in the epigastrium, and his belly was somewhat tumid and a little tender on pressure. The vomiting, however, had subsided to a great extent, and he was able to retain food on his stomach. His pulse was a little accelerated; but there was no notable increase of temperature. On the morning of the 17th, I noticed considerable discoloration (brown to yellow tinge) of the skin; but the conjunctivæ were not yellow. The belly was tumid; tongue furred; pulse quiet. The bowels had been twice moved. He said he was better. I ordered infusion of gentian with bicarbonate of soda. In the evening, he was decidedly worse; and he complained much of thirst and insomnia. The following morning, he was moved into hospital."

A *post mortem* examination was made forty-three hours after death by Surgeon-Major Boileau, M.D., Assistant-Professor of Pathology, who has been so good as to contribute the following notes. The length of the body was sixty-six inches; the girth of the thorax thirty-five inches; weight 137 lbs. The integuments, especially those of the upper parts, were tinged yellow. There was considerable hypostatic congestion; slight rigor mortis. The pupils were dilated, and the conjunctivæ of a deep yellow. The abdomen was extremely tympanitic. There was a cicatrix on the body of the penis. The muscles were of a healthy red appearance. *Head*.—The pia mater was rather freely injected; the arachnoid presented a good deal of opacity; the brain-substance was firm and normal on section; the lateral ventricles contained a small quantity of serum. *Neck and Thorax*.—The mucous membranes of the epiglottis, larynx, trachea, and larger bronchi were of a deep purple colour. The œsophagus contained a very dark coloured tenacious fluid; its mucous membrane was pale, and presented some dark striae. The lungs were normal, except that, posteriorly, they were engorged with blood. The surface of the pericardium had an icteric tinge; its cavity contained about a drachm of fluid. The substance of the heart was fatty; its surface was pale and unhealthy looking. There were no clots in the cavities, only a small quantity of dark fluid blood in each. The endocardium of the left ventricle presented several ecchymotic patches. The valves were healthy. The weight of the heart was five thousand nine hundred and twenty grains; its bulk

* This urine was not tested; but some drawn from the bladder after death presented the same physical characters, and was found to contain blood. After boiling and filtration, Pettenkofer's and the nitric acid tests for bile were applied, with negative results.