secreting, the soft and brittle, cellular textures, and the non-secreting, coherent, and elastic ones; between the green cellular parenchyma, and the outer cellular membrane of the leaf. The distinctions are, however, very palpable, and in animal structures so important, that I purpose, in order to avoid confusion of terms, and yet preserve the distinctions set before us by nature, to use the term *corpuscular texture* for the very soft, opaque, and friable cellular textures, limiting the term *cellular* to those which are coherent, transparent, and elastic. For example, the liver and intestinal villi, I would say, are *corpuscular textures*; the lung a *cellular texture*;—the two former being opaque, friable, and incoherent; the latter transparent, elastic, and coherent; and if so, then there is *prima facie* evidence of an analogy between the blood of animals and the parenchymatous texture of a leaf.

REMARKS ON FEVER.

By WILLIAM DAVIES, M.D., Physician to the Bath United Hospital.

(Read at the Quarterly Meeting of the Bath and Bristol Branch of the Provincial Medical and Surgical Association.)

The few remarks which I am about to submit to this meeting have reference to a disease which has, perhaps, given rise to more discussion in the medical world than any other, and concerning which there still remain various and conflicting opinions,—this is fever; and in what is to follow, I would be understood as referring to the common or idiopathic continued fever of this country, the general features of which every medical man is familiar with, more especially those who have past any part of their professional career in Edinburgh or Dublin.

My remarks shall be first on the nature of continued fever; and secondly, on the pathology of head-complication, as illustrative of what has preceded, and with reference to practice.

The two following propositions must be granted:—First, that the disease is communicable—or that an individual subjected to a sufficiently close intercourse with another labouring under continued fever, may become similarly affected.

Second. That in order to a disease being communicable; it is essential that certain matter shall pass off from the body of the individual affected, and be received into that of him who is to suffer. It is unimportant whether this entrance is effected by inoculation, or by means of atmospheric transmission; at present we have to do with the latter.

Now, if an individual were much confined to an apartment in which there were patients labouring under continued fever, breathing air loaded with the exhalations from such patients,—and if, after a time, the individual thus circumstanced were to be seized with

the same disease, and others following after him subjected to the same influences, and sharing the same fate, with sufficient frequency and certainty to justify the conclusion that the one was the cause of the other,—and we were asked by what means or through what channel the system of the one became affected by the exhalations from the body of the other, we should, I think, have little hesitation in answering, that the matter exhaled from the bodies of the fever-patients entered the lungs of the healthy persons, along with the atmospheric air, and thus found entrance into the blood, where it accumulated until its amount was sufficient to impede the healthy functions of the body—that is, until fever was produced.

Again, if we were farther informed, that twenty previously healthy persons, from different localities, were similarly subjected to the influences above-mentioned, and that every one of these twenty persons was attacked by the disease, but that none of them were so until after an exposure of one or more weeks, some of them after a lapse of three months, and no two of them at precisely the same interval of time,—this would add confidence to our opinion, and we would say that the interval which elapsed between the first exposure and the seizure, marked the period during which the poison was accumulating in the blood, and that the difference, as regards length of interval, between exposure and attack, was an index as to the vigour or weakness of the different constitutions.

Our informant does not stop here, but tells us, that during the early period of the attack, and whilst it is at its height, the power to infect is not nearly so great as during the decline of the disease, and the advance towards convalescence. Here is another element in our belief, and we say, that whilst the disease is declining, and the patient recovering, the system is engaged in freeing itself of the poisonous matter, which is being evolved at the various emunctories of the body.

Now, these are not speculations, but carefully ascertained facts, and lead, I think, very conclusively to the belief, that in continued fever, the blood is primarily affected. I will not enter into the question, whether the constitution of the blood itself is changed by the action of the poison of fever, or, whether the blood merely serves as a vehicle for the conveyance of that poison in a free state to the various organs. This is a question unimportant as regards my present object, and moreover, we do not possess materials sufficient for its elucidation.

I will now proceed to a few practical deductions from, and illustrations of, the foregoing remarks.

Every practitioner who has seen much of continued fever in this country, is aware that the great danger he has to apprehend is the supervention of cerebral symptoms; here the intestinal follicular lesion is comparatively rare, and thoracic complication seldom

formidable;—the rock a-head he dreads is cerebral symptoms. Now, what I wish particularly to call attention to, is the fact, that when such symptoms do arise in the course of continued fever, they depend on a pathological cause, altogether distinct from that which is competent for the production of similar symptoms, when no fever exists—namely inflammation; and hence the remedies of inflammation are not always to be had recourse to, and never to the extent that would be advisable in primary encephalitis. The real cause of the head-symptoms would seem to be the effect which the poisoned or altered blood circulating in the brain produces on the functions of that organ; and strong evidence of the truth of this is to be found—first, in the progress of the disease, and secondly, in its morbid anatomy.

1st. In the progress of the disease, we find that the head-symptoms, during the early period of the attack, are confined to frontal headache and giddiness, with more or less of intolerance of light and sound; and as the disease advances, these conditions pass into great restlessness and sleeplessness, with active delirium, or it may be great torpor and drowsiness, with a wandering and confused state of mind when roused; and either of these forms may pass gradually into coma and death, or under more fortunate circumstances, into convalescence and recovery. All this would seem to mark the increasing influence of the poison on the blood, and through the blood, on the cerebral functions; and when this influence terminates short of death, we have another law of the action of poisons brought into play—namely, that after a time they lose their power over that particular system, and are discharged therefrom, leaving the individual in a fair way of recovery. Hence the wisdom of Cullen's statement,—that the proper object of treatment is “to obviate the tendency to death,” well knowing that if the patient can be kept alive, and free from serious local lesion, for a sufficient time, the system will shake off the load of poison by which it is oppressed.

2nd. In the morbid anatomy of the brain in fatal cases of fever, from which ample proof has been obtained that there is no necessary connection whatever between the symptoms presented during life, and the morbid appearances discovered after death. I have examined the head in several cases, and one since I have been physician to the United Hospital, in this town, when death has taken place by way of coma supervening on the continued fever, without being able to discover any cerebral lesion at all proportionate to the symptoms, not anything more decided than slight sub-arachnoid effusion and perhaps, a drachm of fluid in the lateral ventricles. Dr. Alison, M. Andral, and Dr. Graves, have placed on record abundant evidence of the inability of morbid anatomy, to explain the cause of head-symptoms in continued fever. The following case in point is from Dr. Graves's “Clinical

Medicine” :—He says, “He was a young man of robust habit and apparently good constitution, and laboured under the ordinary form of maculated typhus. Shortly after his admission he was attacked with delirium, which was soon after followed by coma and death. Now, suppose you were called to see a patient, not labouring under typhus, but exhibiting a similar train of symptoms,—that is to say, violent delirium, accompanied by flushing of the face, suffusion of the eyes, headache, and a tendency to get out of bed,—in fact, a state of furious excitement, requiring the restraint of the strait waistcoat, what idea would you be likely to form of the condition of the brain? If a patient of this kind had no typhoid symptoms, you would certainly say that he was labouring under meningitis or cerebritis; and if the case proves fatal, you would naturally expect to find lesions of the brain fully sufficient to account for all his symptoms. And you would in all probability find extensive thickening of the membranes of the brain, with sub-arachnoid effusion, or you would discover softening, increased vascularity, and suppuration of the encephalic mass. But here, a man in fever exhibits all the symptoms of cerebral inflammation; the cerebral affection runs on to a fatal termination with great rapidity; he dies comatose. And what do we find on dissection? Doubtful signs of congestion, and no distinct evidence of inflammation; a slight opacity of the arachnoid at the base of the brain, and about a tea-spoonful of clear sub-arachnoid effusion.” He then says, “This seems to prove that in the production of cerebral symptoms in typhus, some cause not to be recognised by the production of cerebral lesions, or in other words, something besides mere congestion or inflammation exists.” And this brings us back to our starting point—namely, that this “something,” which is neither inflammation nor congestion, is the effect of the poison of fever on the blood, rendering that fluid unfit for the support of the healthy functions of the brain.

It may be asked, why, in an attempt to fix on the blood as the starting point in fever, has no analysis of that fluid been submitted? The answer to this question is, simply, that no analysis has yet been made which has thrown any light on the matter, that is to say, the specific poison on which fever seems to depend has never been isolated. But this need not in the least surprise us, for there may be and doubtless are, many things in the blood which chemistry is unable to isolate,—urea for example. No chemist has yet succeeded in finding urea in healthy blood, yet if the function of the kidneys be arrested for a time, the presence of urea in the blood is easily demonstrated, showing that it does exist in healthy blood, although we are not able to find it. So again as regards sugar in the blood of diabetic patients. It was long before chemists succeeded in demonstrating the presence of sugar under such circumstances, and when this was done, it was by taking advantage of a well known

property of sugar—namely, fermentation. So that we are not at all justified in setting aside the evidence drawn from other sources, on the plea that organic chemistry has not succeeded in placing this poison before our eyes, for we must keep in mind, that whatever this poison may be, it is a matter with whose physical properties and chemical affinities we are wholly unacquainted, and that the only knowledge we possess concerning it, has been derived from a study of its effects on the living human body.

CASE OF PARTIAL AMPUTATION OF THE RIGHT FOOT.

By JOHN WICKENS WEST, L.A.C., M.R.C.S., &c.

Andrew Cobb, aged 25, by occupation a farmer, met with a severe accident to his right foot, August 20th, 1846. He had been engaged with his labourers at a thrashing machine, and his attention being suddenly attracted by one of his children approaching too near, he was put off his guard, and caught his foot in the machinery. It produced extensive laceration of the soft parts, and a comminuted fracture of all the phalanges of the toes excepting the little one, which was left entire. The metatarsal bones of the second and third toes were also fractured. The phalanges of the great and fourth toes were left in the machine, and those of the third merely hung by integuments. The bleeding at the time of the accident was considerable, but was timely arrested by a professional friend, who visited him in my absence.

On my arrival at his house about an hour afterwards, I found that the foot was merely bound up in a handkerchief, by the desire of the poor man, who was anxious for me to see him before an operation was performed. There had been no farther hæmorrhage, the artery, (popliteal,) being well secured by the tourniquet. On examination of the parts I found it quite impossible to save any of the toes, I therefore determined, in consultation with Dr. Crabb, to perform partial amputation of the foot; and as the soft parts beyond the toes were not much injured, I resolved to amputate as near to the seat of injury as possible, in order to retain for the poor fellow a useful limb. I commenced my incision on the dorsum of the foot, carrying it through the integuments in an oblique direction round the sole, thus securing a good flap. I then divided the tendons and other soft parts, and sawed through the centre of the metatarsal bones. The arteries were with some difficulty secured, especially the internal plantar; the outer being larger was more easily tied. The integuments were then brought in apposition, and retained by two ligatures, and the parts dressed with plaster and bandage. The man bore the operation remarkably well, and lost but little blood. Forty drops of the tincture of opium were given him shortly after, and I left him for the night.

21st. Visited him this morning, and found he had passed a restless night, and complained the bandage was too tight, and the stump painful. I slackened the

bandage, and ordered cold water to be applied frequently. Pulse 96; tongue white.

Mist. Salinæ, oz. iss., quartis horis. Haust. Sennæ-statism.

22nd. Has been more tranquil since yesterday, and got some sleep at intervals during the night. No hæmorrhage; bowels open; less feverish; pulse 90; tongue white.

23rd. Going on favourably, but the stump smells offensive, and is stiff and uncomfortable.

25th. At my visit this morning I removed the dressings, and found that partial adhesion had taken place; the stump discharged a thin fluid, but looked pretty healthy. I dressed the parts with soap-cerate plaster and bandage. Bowels open; pulse 96. To continue his medicine.

27th. On removing the bandage I found considerable discharge, very offensive and thick, and evident appearance of granulations. I ordered a bread poultice to be applied night and morning.

29th. The poultices have been continued since my last visit, and there is much improvement in the appearance of the parts, which are disposed to granulate freely. The ligatures are come away.

November 2nd. I omitted the poultices this morning, the parts looking so thoroughly healthy, and the granulations so exuberant, that I commenced applying caustic and dry lint, with straps of soap-plaster over.

3rd. The parts look healthy, and inclined to heal.

I continued the same treatment for three weeks, by which time the stump was completely healed, and the man was enabled, by the assistance of crutches, to get about.

December 12th. I visited him this morning and found he was able to put on an easy shoe, and could walk tolerably well with a stick.

I would remark, that I consider it far preferable, in all cases where it is possible, to amputate the foot at the centre of the metatarsal bones than to remove it at the articulation of those bones with the tarsal. The opening of joints, however small, is always attended by considerable irritation, and increases the sufferings of the patient.

Poole, December 26, 1846.

Hospital Reports.

WEST NORFOLK AND LYNN HOSPITAL.

COMPLICATED SURGICAL CASES AND OPERATIONS.

By CHARLES COTTON, M.D., F.R.C.S., &c.

(Continued from page 611, of last Volume.)

STRANGUATED HERNIA IN THE INGUINAL CANAL. (TWO DAYS.)

William Wicks, farm-labourer, aged 26, admitted at 10 p.m., October 27th, 1844. States that whilst unloading a cart of earth on the morning of the 25th a lump came down, which he was unable to return. He has been sick ever since, and the bowels have not acted. The surgeon who saw him having failed in the use of