culties, much more may be accomplished than is done at present. If, too, what is said in its favour be true, we have in the subcutaneous injection of mercury an efficient mode of treating these cases; for it is stated by the advocates of this method, that about a dozen injections, which of course the surgeon would personally administer, are all that are required to effect a cure; and for these reasons I have myself resolved to give this plan a fair trial in the out-patient department of the Manchester Infirmary.

There is one other point to which I should like to allude, which is this: we have satisfactory evidence—amounting, indeed, to demonstration—that syphilitic gummata, which untreated rapidly lead to a fatal issue, are quickly absorbed under treatment by mercury and iodide of potassium. We also know that, although these special tumours are not prone to soften in the centre, after the manner of cancerous and tubercular growths, yet they and other syphilitic lesions possess many features in common with cancer, one of which is the tendency they both show to cell-proliferation. Is it not, then, rational to hope that we may discover some drug or drugs which shall exercise as powerful a control over the so-called malignant growths, as mercury and iodide of potassium exercise over the poisoned products of syphilis?

Since the above was written, I have employed the treatment by subcutaneous injection of mercury in about a dozen cases of constitutional syphilis, and so far am by no means satisfied with the results obtained. The injections, administered as a rule three times a week, give considerable pain both at the time and afterwards; they sometimes, though rarely, produce abscesses; the patients become dissatisfied with the mode of treatment, and the symptoms do not yield so quickly as under the treatment by inunction or internal administration. The preparations employed have been corrosive sublimate in doses of one-sixteenth and one-eighth of a grain, and the cyanide of mercury one-tenth of a grain.

REMARKS ON THE PATHOLOGY AND TREATMENT OF CONTINUED FEVER.

By CHARLES MACLEAN, M.B., Applecross, Ross, N.B.

UNDER the above heading, I venture to make some observations, in order that a subject which I consider of importance may be properly ventilated in the profession, and in order that they whose opportunities for investigation are greater than mine can possibly be, in a scattered country practice, may give it some consideration. I feel great delicacy in coming forward, especially with anything like new views; and my apology, if any be demanded, is that I am influenced entirely by a sincere desire after truth.

Enteric fever, of a bad type, became epidemic in the filthy villages on the north-coast of this parish. I treated it at first in the usual way in every respect; but the remedies seemed to me to have little good effect, and altogether the mortality was rather high. This caused me to think of the pathology of the disease, and of the physiological action of medicines in connexion therewith. I considered what an important part the tendency to congestion played, and how frequently the immediate cause of death was traceable to the local manifestation of this tendency to capillary congestion; and I asked myself if there were nothing available either to prevent or to cure these baneful results. In considering the question, it occurred to me that, as bromide of potassium has been used, and with success, in engorgements of the internal organs analogous to that often present in fever, I was warranted in giving it a trial here. Pulmonary congestions were almost universally constant in greater or less degree. In one case, with well-marked bronchial congestion, and some indications also of commencing retardation of circulation in the proper lung-tissue, along with the usual cerebral symptoms, I tried the bromide, with the most satisfactory result. I gave two scruples of it, in divided doses, in the twenty-four hours. The uneasiness, dyspnoea, anxious and hurried breathing, and nervous tremors, gradually gave way, and the pulse became more normal in character when the drug took effect; the patient recovered well.

I have used this medicine frequently since then in this disease, and the effect has almost uniformly been, after a day or two of its use, marked amelioration in the nervous symptoms, as headache, subsultus, anxiety, and muttering delirium, usually with diminution of the pyrexia, and removal of the symptoms of congestion generally, indicated by the dicrotous pulse becoming less so in character, and more normal in other respects. Indeed, all the symptoms—general as well as connected with particular complications—have often appeared to be quickly modified for the better, after the full action of the drug. The patients, as a rule, got quiet sleep, whatever might have been their wakefulness,

anxiety, and other nervous symptoms, before. They felt, in fact, as well, as appeared to me, in a fairer way. I have never observed any (even temporary) bad effect to put down against the seemingly beneficial influence of the bromide; and all the cases in which I have used it have recovered, with the exception of one, the particulars of which I think it interesting to state, shortly, in connexion with this subject. It was that of a woman, with the usual bronchitic symptoms, diarrhoea, anxiety, and sleeplessness; pulse 130; temperature 103.5 deg. Fah. I gave the bromide, five grains every three hours, and found a marked improvement in two days. The pulse had come down to 115, and had not the quick irritable feel. Temperature, 101 deg. Fah. The breathing was calmer, and the nervous symptoms were improved; she expressed herself much better. Next time I found her much worse; all the symptoms had returned in force. I found, upon inquiry, that she had not taken any of the medicine for about thirty hours, because "she did not like it." I repeated it in another form, but with what regularity it was taken I cannot say, and the patient sank and died.

I was the more encouraged to use the bromide at first, because physiology and pathology seemed to teach that there were remedies in daily use in the treatment of fever that were either simply useless, or directly injurious. These that I have just used may appear bold terms, but I certainly felt convinced that we never can satisfactorily treat fever until we cease to treat symptoms (or names), and until we direct our energies to obviating or subduing the tendency to passive congestion of the blood in the capillaries, and thus defend life by concentrating our forces at those points at which we know it will be attacked.

Let me not be misunderstood when I say that life is endangered by this state of the circulation, for we cannot strictly say that the immediate cause of death in fever is always congestion of this or that organ, or even of a general nature; but I certainly believe that it is invariably traceable to it. For example, take asthenia as a cause of death, and a rather frequent one it is. First there is congestion, more or less general, of the capillary system. With this passive congestion there comes to be retardation—probably often almost amounting to complete stagnation—of the blood-current, and of course a deficiency in the supply of arterial blood to the part results, and the nutrition is impaired. Now this malnutrition will produce a corresponding impairment of function, including that of important parts in the economy, such as the nervous system, and this must react unfavourably on the circulation again, and so on, until a state of asthenia is induced; and as the heart itself, on account of its ceaseless duty, requiring an uninterrupted supply of blood, suffers early, and becomes weakened in action, can we wonder if asthenia comes on apace?

I shall state here what I believe to be the *modus operandi* of the cause of all the phenomena of continued fever (after the rigor, etc.) and I shall then call attention to the probable mode of development of this cause itself. The cells of the tissues in health are continually parting with azotised and non-azotised substances, the various elements of which compounds eventually become oxidised and go to form carbonic acid, etc., to be eliminated by the lungs and skin; and urea, uric acid, etc., to be excreted by the kidneys. It has been ascertained also, that the nitrogenous substance converted into urea and uric acid is derived not only from the products of the degeneration of the tissues, but also largely from superfluous albumen of ingesta; also that part of the said products of degeneration of cells eventually becomes again part of the solid structure of the body. To make up for this loss by degeneration, the cells are constantly taking up material from the arterial blood. Now in this disease the cells, though they go on degenerating as in health, are not able to replenish themselves, owing to the existing stagnation, and consequent want of renewal of arterial blood, in the capillaries. This same stagnation prevents not only the albumen newly formed from the food, but also that which the tissues have parted with, being taken up in anything like proper amount, by the exhausted cells. One of the obvious consequences of this is an increase of oxidation in the system, causing the rise of temperature, and an increased elimination. It is this circumstance that has probably given rise to the idea that there is an abnormal disintegration of the tissues in fever; but I ask, is not this increased elimination quite explicable by this theory of non-nutrition, as I may call it, or want of replenishing power in the cells, on account of congestion of capillaries supplying them, the degeneration being just as in health? This non-nutrition quite explains the softened appearance of organs, so invariably met with after death from fever of typhoid type. In short, is not the breaking down of this pernicious congestive barrier the one desideratum in the treatment of fever?

An accurate knowledge of the bearing and influence of this tendency to congestion in the pathology of fever must, I think, be of the greatest importance practically, and this must be my excuse for dwelling on the subject. If this be not the cause of the disease, what is? What

else can explain the phenomena? There has been a rather ingenious theory brought forward, that uræmia is induced from the kidneys not being able to eliminate all the urea, and that thus all the symptoms (or the later symptoms), are referable to uræmic poisoning. But can the uræmic theory explain the remarkable uniformity we observe in the time of duration of fever, etc.? There is undoubtedly an excess of combustion going on, for reasons to which I have before alluded, but at the same time the kidneys, as a rule, act accordingly. In the cases which I have seen, and I have seen some severe ones, there has been a very free elimination of nitrogen by them. It would be a very serious matter for millions of people if, for instance, their consuming of nitrogenous food, in excess of what was required, were to be visited on them in uræmic poisoning.

Thus far I have been speaking of the immediate cause of the fully developed fever; but I think it very interesting, pathologically, to trace the matter as nearly to the source as possible; and it is by doing so, I think, that the greatest light can be thrown on the whole subject. And I cannot help thinking that our attending to the state of things in the two great stages into which the disease may be divided, as we shall presently see, may lead to the most important practical results, and that we may be able not only to treat satisfactorily the fever proper, but also, by modifying the first stage occasionally, to mitigate, if not prevent it altogether.

There have been various opinions regarding the mode of action of the zymotic in the system. The most likely view is, that the poison acts directly on the nervous system, for the symptoms at the outset partake largely, if they are not altogether, of a nervous character. But I think that the theory that it acts as a direct sedative on the nervous system is negated by the fact that the symptoms in the beginning point unmistakably to irritation of that system. For instance, there is invariably more or less feeling of cold (rigor) at the outset of a fever of any consequence; and this, as we all know, is produced by over-stimulation of the vaso-motor nerves, which causes contraction of the vascular coats, and diminution of vascular calibre. Now after this state of stimulation has lasted a certain length of time, in obedience to an universal natural law, that over-stimulation shall be followed by a proportionate paralysis, a reaction comes on in the opposite direction, and there is relaxation of the coats of the vessels; which brings us to the congestive state, which we saw was at the root of the fully formed fever. The mistake of Cullen's theory seems to be that, although he spoke of spasm, still he seemed to share in the opinion that there is direct depression (or paralysis) of the nervous system, two states that appear rather incompatible with one another.

Now may we not reasonably suppose that the action of the zymotic is quite over, or its strength quite exhausted, when the rigor, and other symptoms of nervous irritation, are past? For it seems far-fetched to suppose that the virus, after powerfully stimulating, should suddenly change its tactics and paralyse the nervous system. At any rate, the resulting paralysis is not only quite explicable independently of this supposition, but it is a necessary consequence of the stimulation.

We have thus two great stages of fever; the first that of exaltation, or stimulation; the second, reaction, or depression; and this explains the great uniformity in the course and phenomena of fever.

I may now briefly notice how favourably the action of bromide of potassium may compare with that of some other medicines often used in fever. What can opium do (I mean in the stage of depression, or pyrexia), when nervous energy is kept in abeyance, as we saw, not only directly from the reaction consequent on the first stage, but also eventually by the congestion existing around the nerve centres, in common with that of other tissues, and impeding their nutrition? What can it do, except, by further paralysing the coats of the vessels, effectually stifle any attempt at rallying? Opium is prescribed, in the pyrexial state, hesitatingly, and very properly so; but why use it at all? I have seen the greatest benefit apparently follow from the use of the bromide in the circumstances in which opium is said to be admissible. Indeed, the use of the narcotic in fever, and the administration of strychnine in recent apoplexy, seem to be parallel modes of treatment. Opium may be said to act in a positive manner on the nervous system, and to favour congestion, whereas bromide of potassium is said to act by its constricting effect on the minute vessels; and this is very likely, judging from its effect as a hæmostatic in various passive bleedings, its influence in removing hyperæmia, etc. Now it is surely this constricting influence that is imperatively required here.

Speaking of opium, I think it would be very interesting to try the effect of narcotics, or other nervine sedatives, in the first stage; particularly those, perhaps, whose primary stimulating action is most insignificant.

It will appear, from what I have been saying then, that the medicines to be used in fever (after pyrexia has come on), should belong to the class

of stimulants, as a rule. Now I believe the bromide may be considered the safest and truest stimulant here; for, from its mode of action on the small vessels, it must be evident that it goes to the root of the matter, and is not calculated to overdo what is intended.

Wine is one of our most powerful agents for good; but, at the same time, its indiscriminate employment cannot but be attended with some danger, on account of its action being chiefly confined to the heart and large vessels, and the possibility of its causing inflammation instead of healthy circulation in the congested parts. Where the vital powers are very low, and the pulse indicates want of power of the heart and large vessels, it is often invaluable. I was impressed with the good effects of wine in one case in particular, before I began to use the bromide. It was a case in which there were sibilant *râles* all over the lungs, much dyspnoea, lividity of face, pulse scarcely to be counted, dark dry tongue, and the patient was in a state of semi-unconsciousness. I looked on the case as desperate. I thought that giving antimony, or ipecacuanha, would be like applying emollients to a conjunctiva where there was nothing but passive congestion of the vessels; for here, as in that affection, the disease is quite the reverse of acute, and depressing, or relaxing medicines seem, indeed, uncalled for. Again, local depletion seemed less than useless, for not only could the patient not afford a drop of blood, but the practice of putting leeches on the chest to relieve congestion in the lungs appears, to say the least of it, rather unscientific. So, as a *dernier resort*, I ordered wine, and instead of giving diuretics, which I thought might injuriously concentrate the blood, I gave diluents, which I thought would serve the purpose of diuretics, and wash the blood, as it were, and not have their bad effects. I gave simply plenty of milk and water, to serve both as diluent and food. The patient rapidly recovered under the stimulant treatment.

Now in cases like this, in which the vital powers are low, or where we can predict that they will become so, I think the greatest good will accrue from giving the bromide in combination with wine; for the former will act as a *vis a fronte* to the circulation by its action on the minute vessels, while the latter will be a *vis a tergo*, and perfectly safe in this way, owing to the clearance effected by the bromide.

I think it very important to bear in mind in fever, not only that it, with its complications, is not of an acute or high character, but that it is indeed the very reverse; and that if we administered medicines that we would give in acute affections, they will certainly have the opposite effect to that intended.

ON ENLARGEMENT OF THE TONSILS AS A CAUSE OF NIGHTMARE.

By J. WARRINGTON HAWARD, F.R.C.S.,
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VARIOUS evils have been attributed to enlargement of the tonsils, some of them, perhaps, more imaginary than real; but it has occurred to me to see several instances of distressing nightmare in children clearly referable to the condition of the tonsils; and as I have not anywhere seen the connexion pointed out, it may be worth while to draw attention to it. The case which first led me to observe the fact was a very striking one. An intelligent, and not at all nervous, girl of thirteen years, had for several months been subject to occasional attacks of nightmare, which were increasing in frequency and severity. A short time, usually about an hour, after going to bed, the child arose with a loud scream, and, on the parents going into the room, was found sitting up in bed, the eyes vacantly staring, and the face wearing an expression of extreme alarm. Although the eyes were open, she did not appear to be awake, and required moving and loudly speaking to before she seemed to appreciate the presence of those around her. She would then give a sigh, say that she had been frightened, she did not know by what, and presently fall asleep again. These attacks occurred sometimes several times during the night, and for several nights in succession, and then were absent perhaps for some weeks, when they returned. She was a healthy looking child, and had been nurtured with every care; and the parents were much distressed by these symptoms, fearing they might be premonitory of some serious cerebral affection. She had been treated without benefit by various medicines. She was brought to me in October 1870, on account of an attack of stomatitis; and the parents then mentioned the occurrence of the nightmare. On examining the mouth, I noticed that the tonsils were greatly enlarged, and it seemed to me possible that the