

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

nightmare might depend on the obstruction to respiration thus produced, with the consequent non-aëration of the blood, and cerebral congestion. This idea was confirmed by the mother saying that she always snored loudly, and that the attacks were generally worse when she had a cold. As soon as she had recovered from the stomatitis, I removed a portion of the tonsils, and from that time until now she has never had another attack of nightmare. Since then I have seen three similar cases, all in children, in each of which, after I had removed the tonsils, no recurrence of the nightmare took place.

It is worth while, therefore, I think, when a child suffers from night-terrors, to inquire into the state of the tonsils as a possible cause. The kind of nightmare thus produced seems to differ from that having its origin in gastric irritation or dentition chiefly in this: that whereas this last kind occurs, as a rule, only once in the night (as Dr. West points out), and the child then sleeps quietly, that due to enlarged tonsils, especially when the attacks are worst, often recurs several times in the same night, and is invariably observed to be aggravated by the child catching cold.

While speaking of the removal of the tonsils, I may say that I almost always use the guillotine, which, with the addition lately made of a strong wire to keep the mouth open, seems to me the instrument by which the operation can usually be done much the most quickly, a point of especial importance with children. It is very seldom necessary to give any anæsthetic; but if a child be very resistant, it is better to give ether, and open the mouth by Mr. Smith's gag, which affords an excellent view of the parts, and is not at all in the way of the operator.

SUDDEN DEATH FROM HEART-CLOT DURING CONVALESCENCE FROM PARTURITION.

By GEORGE BOWMAN, M.B., Manchester.

MRS. W., aged 37, was safely delivered by me on February 2nd, of a full-grown child. The labour (her seventh) was in most respects straightforward and easy, as had been all her former ones. The first stage being rather tedious from inaction of the uterus, I prescribed about two scruples of powdered ergot, which in about ten minutes excited the uterus to contraction. The head was born with the third pain. The rest of the labour was in every respect natural; and the patient appeared to be making an excellent recovery, when, on the morning of the tenth day, at two o'clock, I was called up by the husband and requested to come to see his wife, with whom, to use his own expression, "it was a case of life or death." When I arrived at the house, I found my patient dead. Her husband had left her about two hours before in excellent spirits, with the anticipation of sitting up on the morrow. She had been asleep about an hour, when the nurse, who was sleeping beside her, was awakened by the baby (who was at the breast) crying. On attempting to quiet it, she spoke to Mrs. W., but received no answer. Becoming alarmed at the coldness of her limbs and fixity of countenance, she ran for the husband. Mrs. W. breathed twice or thrice before the nurse left her, but death took place before her return into the room.

My friend and neighbour Dr. Brierley assisted me in making an examination fourteen hours after death. The body was very well nourished; both the chest and abdomen were moderately covered with fat. *Post mortem* lividity had taken place to a most unusual degree at the back of the neck, and the under surface of the arms. In the latter situation, in fact, discolouration had commenced when I was called to see her in the early morning. The heart was very pale in colour, and, as the microscope showed, was undergoing fatty degeneration. The walls of both ventricles were very thin; the left ventricle was empty and firmly contracted, the right ventricle was full of blood. The valves were all healthy. In the right ventricle, and attached to the chordæ tendinæ of the tricuspid valve, was a large and straggling dark-coloured fibrinous clot. From its situation and firmness, it had evidently destroyed the competency of the valve, thus producing an obstruction to the circulation through the lungs. The pulmonary artery was perfectly free. The lungs were found to contain very little blood. Evident traces of old tubercular deposits which had undergone calcareous degeneration were found at the apex of the right lung; the left was perfectly healthy. She had a slight attack of hæmoptysis about six years since. No other organs were examined, on account of the objections of the husband.

In none of the cases of death from heart-clot which I can find reported has death come on so insidiously. The patient has generally been out of bed and slightly exerting herself. Here she was in bed and asleep. Death evidently took place without the least movement. This case in

some particulars corresponds with the description given by Dr. Meigs of sudden death from heart-clot, in Sir James Simpson's *Selected Obstetrical Works*. The coagulum or fibrinous polypus attached to the auriculo-ventricular valve in this case had so increased in size, that probably portions of it would be impelled by each contraction of the right ventricle through the semilunar valves into the pulmonary artery, thus so far interfering with the healthy action of the valves as to allow regurgitation.

RARE DISLOCATION OF THE HUMERUS.*

By A. W. STOCKS, Esq.,

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AN elderly lady, not very stout, crossing a street in a hurry, fell on her right hand and doubled her arm up, so that the hand came into contact with the upper part of her sternum; she also received a bruise on the left side of her face, on its coming into contact with the ground. It was found that she had lost the power of motion in her right arm. Medical assistance was immediately obtained, and an examination of the limb was made. No dislocation or fracture was discovered, but there was great pain down the whole limb.

Four days afterwards, I was requested by her medical attendant to see her with him, and on my first visit failed to discover any displacement of the bones of the arm. On a second consultation (next day), however, I became convinced that the head of the humerus was displaced; but, as the opinion of my friend did not coincide with mine, no attempt at reduction was allowed, and it was decided that the opinion of one of our infirmary surgeons should be obtained. He saw her on the same day, and in opinion agreed with my friend that there was no dislocation, accounting for the slight swelling by saying that there was effusion into the joint and surrounding tissues. Rest, evaporating lotions, etc., were recommended. With these remedies I was not satisfied, and a further consultation was fixed. Five days afterwards we saw her again (ten days after the accident); and, although our opinions were the same, it was resolved to complete the diagnosis by attempting, under chloroform, a reduction of the dislocation, should one exist. All doubt as to the exact nature of the case was at once done away when, on moderate extension with the heel in the axilla, the head of the humerus visibly and audibly slipped into its normal position.

It might appear that in any case, on fair examination, there could be little room for doubt as to the existence or absence of a dislocation of this humerus; but as there was in this instance such serious and persistent difference of opinion, showing how slight the distortion was, and as the case appears to be of a somewhat unusual character, I beg to give a description of it.

The shoulder, at first sight, gave no indication of any abnormal position of the head of the humerus. There was no undue prominence of the acromion, nor perceptible flattening of the deltoid muscle—conditions almost invariably present in dislocation of the humerus. The arm was capable of very extensive movements; it could be brought to the side, raised to a right angle with the chest, and extended forwards. The only motion which was restricted, and that to a slight degree, was the backward one: of course, none of these movements could be accomplished without considerable pain. The sole alteration in the figure of the joint was a slight flattening on its anterior aspect, rendering the coracoid process just perceptible to the eye, and a slight bulging under the posterior edge of the acromion.

It will be remembered that in the normal condition of the shoulder-joint, when the arm hangs perpendicularly by the side of the body, the head of the humerus projects slightly beyond the anterior edge of the acromion process, and that there is a corresponding hollow or depression under the posterior edge of that process. A condition the exact reverse of this was the whole distortion found in this case.

In describing the dislocations of the humerus, all authors, I believe, affirm that "flattening of the shoulder and the prominence of the acromion" are "common to all luxations of the humerus" (*System of Surgery*, vol. ii, p. 821; Bryant's *Practice of Surgery*, p. 791; Erichsen's *Science and Art of Surgery*, 4th edit., p. 299, etc.) If these very palpable symptoms had been present in the above case, no controversy as to its exact nature could have existed for one moment; it was the absence of these "common" signs which formed its great peculiarity. On the other hand, there can be no room for doubt that there was a dislocation, as, immediately on the bone very sensibly slipping into its place, our consultant exclaimed, "Well, if I had not seen it, I would not have believed it."

* Read before the Manchester Medical Society,