secretion; and to the care necessary to avoid premature inhibition, by opiates, of its secretion and removal.

Certainly we may wish to understand the underlying processes that affect the secretion of mucus, and if, as is perhaps the case, the primary change, in a catarrh, is not so much one of bacterial infection as a change in the physiological equilibrium of the mucous membrane that allows scope to bacterial activities, it seems likely that, when we do attain such understanding, we may come to appreciate how it is that, during the epidemics we call "catarrhal," there are associated cases and groups of cases, with affection of the cerebro-spinal system, not "catarrhal" in our modern sense, and yet a part of the "epidemic."

To Fernel, Benedetto, Ballonius, and other physicians of the sixteenth century these nervous forms of disease were alternative to those we now call "catarrhal"—whether respiratory or gastro-intestinal—and the epidemiological unity of what we now call encephalitis, myelitis, and the like with what we now call influenza was explained by them in terms of the famous doctrine of "catarrh," whereby an "excretion" of the central nervous system, if "repulsed," set up nervous disease, but, if determined to a mucous membrane, produced bronchitis, diarrhoea, and so forth.

To these physicians, then, a "defluxion" through the ethmoid safeguarded the brain against encephalitis; we, in thought, reverse the process and consider cerebro-spinal meningitis, encephalitis, and poliomyelitis as due to organisms reaching the nervous system by the ethmoid route, after having set up or failed to set up a preliminary "catarrh" of the nasopharynx. But we may well consider anew whether the factor which turns the scale to the one side or the other may not be sometimes a condition of the mucous membrane depending on vasomotor or trophic disturbances, and sometimes an alternative condition of the meningeal and cerebro-spinal structures.

In a word: although the explanations of Fernel and Benedetto may seem to us absurd, were they not right in discerning an essential and epidemiological unity between cases that seem to us, clinically, to be totally antipathetic?

Again, Mr. Benians's notion, that it is possible that a primary alteration in the state of the mucous membranes may allow harmlessly "carried" micro-organisms to gain access to the blood and lymph channels and spaces, is perhaps to be collated with a rather curious passage, in Falconer's celebrated account of the influenza of 1803,⁶ that seems to deserve citation. On the occasion of a *post-mortem* examination on a young woman who had died of influenza, upon opening the trachea it was observed that, "on viewing the inner membrane of the trachea with a magnifying glass, ulcers were clearly perceived at the edges of the small holes which supply the trachea with mucus, to defend it from the acrimony of the air."

Now here again is an instance of an observation, worthy of attention, that has been rendered a little ridiculous by the assumption of a particular explanation. It may, however, be put side by side with what is said by Micheli, who in a very valuable paper⁷ seeks to explain the "incontestabile unità fondamentale" of the many diverse clinical forms manifested during the present epidemic period by suggesting that groups of different habitual or occasional microbic inhabitants of the tracheo-bronchial mucosa become suddenly awakened into or permitted virulence by the superintention of a hypothetical "virus primitivo."

In Mr. Benians's hypothesis micro-organisms replace Falconer's "acrimony of the air," and an alteration in the state of the mucosa replaces Micheli's "virus primitivo."

There are obviously, therefore, several points to be considered relating to these matters which would seem not unsuitable for the application of such judicious hypothesis as may stimulate further observation and inquiry. And inquirers might be well advised to adopt as their motto the sufficiently hackneyed but nevertheless still pointed sentence of Baglivi:

Novi veteribus, non opponendi, sed quoad fieri potest, perpetuo jungendi foedere.

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A CASE OF ENCEPHALITIS LETHARGICA TREATED BY HEXAMINE.

BY

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THE following case of lethargic encephalitis, in which recovery followed large doses of hexamine, is worthy of record, although I do not wish to attach too much importance to a single case.

I first saw the patient, a girl aged 12 years, on the evening of January 25th, 1920.

The parents definitely fixed January 17th as the day on which she became really ill, but careful inquiry elicited the fact that for quite a week previously she had been unusually quiet, had not wanted to play, and had slept a great deal. They also told me that on the last day she was at school she had gone to sleep on her desk and that the teacher had failed to rouse her, so let her sleep till close of school, and then sent her home with some other girls. She never made any complaint of headache or other pain; she simply wanted to be left alone. Gradually this lethargy became so deep that they could not get her to take food, and for several days she had nothing. For several days, too, she had passed both urine and faeces without seeming to be aware of the facts.

I found her lying in bed on her back with her legs straight down and close together; her arms were close to her sides; with bent elbows and clenched hands lying on the chest near the chin; her mouth was wide open, and her eyes slightly so. She was breathing regularly, but rather deeply, though not noisily. Her face was placid but expressionless. When spoken to loudly she would open her eyes a little, and for a moment seemed to be looking at one, but then relapsed into her former state. The conjunctivae were slightly congested; the pupils were equal and reacted sluggishly to light, and there was a slight corneal reflex. When the lids were raised she moved her eyes slowly from side to side, but the movements seemed to be quite aimless, as if she did not see. There was no squint or ocular paralysis. When I came to move the limbs I found that a distinct effort was required, owing to a certain degree of tonic rigidity which involved all the muscles of the body; it could be overcome easily, and when the limbs were put into a new position they remained there for a short time, but gradually resumed their previous position. A distinct effort was required to close the mouth, and when the effort was relaxed it opened rather quickly. Attempts to move the arms from the sides, straighten the elbows, or unclench the hands met with distinct resistance; and the same was true if any attempts were made to move the joints of the lower limbs. It was not a temporary contraction of the muscles brought about by the moving of the limb, but a continuous condition and the muscles felt firm. These passive movements elicited no sign of pain or consciousness. The muscles of the neck were in the same condition; the abdominal wall was strongly retracted, the abdomen being typically scaphoid. It seemed to me that the superficial reflexes were abolished, but there was a very marked *tache cérébrale*. The knee-jerks were distinctly lively, I think exaggerated, and a slight but positive Babinski's reflex was present. In both cases the reaction seemed to be more marked on the right side. I tried to get her to swallow by placing a few drops of water on the tongue and allowing it to trickle into the pharynx, but there was no response.

There was no sign of any injury anywhere. The ears appeared to be normal. There was no oedema nor any acetone in the breath. Heart and lungs were normal. Temperature 99°.

After my examination I was standing at the bedside looking at her. Her chest was partially uncovered by the clothes when she opened her hands, grasped the clothes, and made an attempt to pull them up. It was very feeble and unsuccessful, but accompanied by a marked tremor of the hands. Her mother told me she had at times tried to do things like that, and occasionally had spoken a word or two, but they could make no sense of it.

Beyond the usual childish ailments and an attack of tonsillitis about eighteen months previously, there was nothing to note in the medical history, and she had been

thought to be very well until this illness commenced, though she was not what would be called a robust child.

The first impulse in a case of this kind is to diagnose meningitis. But the expression of the child's face, her attitude in bed, and the complete absence of pain in the premonitory stages compelled me to reject this. I had never seen a case of encephalitis lethargica, but the mental picture of it that I had formed as the result of slight acquaintance with the literature was recalled by this case. Sequelae of injury and ear disease were evidently out of the question; and so was any disease of vascular origin. There was nothing to suggest nephritis or diabetes, though unfortunately I never examined the urine. Neither did I perform lumbar puncture. It might have been difficult to do on account of the rigidity, and I doubted if one would get information of value, but it would have been more satisfactory. One could also definitely exclude any form of poisoning. However faulty my line of reasoning may have been I came to the conclusion that I had to deal with a case of encephalitis lethargica, and I gave an extremely gloomy prognosis. I knew of course that mild cases did occur, and that recovery under various lines of treatment, or with none at all, had happened, but this was not helpful.

So far as treatment was concerned, I contented myself that evening with ordering nutrient enemata, and directing that nothing should be given by the mouth as it was obviously dangerous. When I got home I looked up what literature I had by me, but found nothing I could get material help from. Hexamine seemed to be the only drug likely to be of any value, and I searched for information about it. I found there was a general consensus of opinion that it was rapidly absorbed from the stomach into the blood, and that it rapidly passed into the tissue fluids. Some said it passed into the cerebro-spinal fluid, others that it did not. Then there was also a general opinion that it only acted by liberating formaldehyde, and that it only did this in acid media. If this is so then it is difficult to see how it can be of any service either in the tissue fluids or cerebro-spinal fluid, as these are always alkaline. Then I remembered hurriedly reading an article in the January number of the *Medical Review* about intravenous injections of this substance, and looked it up. It was an abstract of an article in the *Wiener klinische Wochenschrift* of November 6th, 1919, by F. Deutsch. In this abstract Deutsch is credited with the opinion that the bactericidal action of urotropine is much greater *in vivo* than *in vitro*; also that sufficiently large doses cannot be given by the mouth, as the stomach will reject them. He had used large doses of urotropine intravenously in acute rheumatism—as much as 90 grains daily for six or seven doses—with nothing more serious than a transient rigor and rise of temperature, and usually with no untoward symptoms at all. I came to the conclusion that this was a possible line of treatment for my case; but before finally deciding I would see the patient again. I saw her the next morning (January 26th). The condition was unchanged. The mother said that once during the night she had put her hand to her neck and said something about beads, otherwise she had lain just as I left her. I therefore decided I would try the drug, and the same evening gave her 45 grains dissolved in 10 c.cm. of normal saline in the left median basilic vein. She winced a little as the needle was passing through the skin. I noticed not the slightest change in her condition while I stayed there.

January 27th—Morning: No change in her condition; not retaining the enemata; passes urine unconsciously. Evening: No change.

January 28th—Morning: There is certainly less rigidity of the limbs and the pupillary and corneal reflexes are more lively. Knee-jerks less marked, and Babinski's test yields a feeble but distinctly flexor response. Mental condition unchanged. Gave a second dose of 45 grains of hexamine. Evening: About the same.

January 29th—Morning: Distinct improvement. Has spoken several times, and seems to have some idea of things about her. Rigidity less, and moves her limbs a little. Cannot be roused to answer.

January 30th—Morning: Still better; asks for things, and answers when spoken to, but in a weak, tremulous voice and not alertly. When speaking there is marked tremor of the jaw. Rigidity everywhere is much less; pupils react very well. Gave a third injection of 45 grains as before. Gave marked evidence of feeling the needle.

She was able to swallow a little water, and now she began to have liquid food.

February 2nd and 5th: Had fourth and fifth injections respectively. From this time onwards improvement was steady and uninterrupted. She began to ask for solid food and for her toys, and in a few days was able to sit up in bed. At the end of the third week she was able to get up and sit in a chair for a short time, and was quite bright and cheerful. My last visit to her was on March 4th, when she was going about the house and quite lively. I have seen her on several occasions since. She began to go to school half-time in June, but after three weeks complained of headache, which disappeared again when she stayed at home. She seems now to be as well as ever she was.

From one successful case of this kind it is easy, of course, to draw extravagant conclusions as to the value of the line of treatment adopted. I do not wish to do that, but in any similar case, or even in a case of meningitis, whatever its nature, I should without hesitation try the method. It is only by repeated trials that the value of any treatment can be verified. As regards this line of treatment, I have satisfied myself that it can be used without fear of any serious ill effect following. None whatever followed in this case, and in certain other cases in which I have tried it, of quite different nature, I have given it with impunity, though I have occasionally seen a rigor follow, usually about four hours later, and accompanied by a rise of temperature up to 101° or even higher. The effects have always passed away in about twelve hours. I should certainly not try it in any case enfeebled either by long illness or any other cause.

In the August number of *Medical Science* there is a long series of abstracts from various sources about the disease, amongst others from certain articles by Netter of Paris, an admitted authority. He apparently regards hexamine as the best drug to use, but recommends oral administration in preference to intravenous. In this case it could not have been given that way. I do not know in what doses he gave it intravenously, and that may conceivably make a difference.

I have recently seen the master of the school she attends. He thinks she is altered in disposition and mentally, not being so bright, and wanting to keep away from the other children. Her mother says she will not go out to play with her friends as they are "so noisy."

TWO CASES OF INTESTINAL TUMOUR: OPERATION: RECOVERY.

BY

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I HAVE had recently in the Cottage Hospital, Fleet, two cases of intestinal tumour which seem to have distinctly interesting features.

CASE I.

A. G., a widow, aged 52, with four children, a thin spare woman, accustomed to land work, gave no previous history of illness of any kind, except that she had suffered much from constipation. A few days before admission she had pain in the right lower quadrant of the abdomen, but kept at work for three or four days longer until the pain got so severe she could only work kneeling. She then consulted Dr. Alban Ward of Crondall, who found a large swelling in the region indicated and sent her into hospital.

A hard tumour could be felt a little above the caecal region; it was fairly movable laterally, and a provisional diagnosis of possible malignant disease of ascending colon was made. There was no sickness nor other signs of obstruction. An exploratory operation was advised, and in performing it the honorary secretary of the hospital, Surgeon Rear-Admiral Axford, C.B., kindly assisted me.

On opening the abdomen in the middle line an intensely hard tumour was felt, about the size of a large hen's egg, immediately behind and partly embracing the ascending colon. Although freely movable with the bowel, it had all the appearance *in situ* of malignant disease of the colon, and a decision was at once come to to excise freely. The appendix was seen to be quite healthy and some distance from the growth. An end-lateral anastomosis was made between the ileum and the transverse colon and the gut removed from the hepatic flexure to the caecum. The tumour, when cut into, was found to be of scirrhous-like hardness, but though attached to the wall of the bowel, had not invaded it.