fact that there are often in the patient's family persons who have that or some other neurosis; that is, some other nervous affection of which there is no morbid anatomy. Why is it that we speak with most confidence of the inheritance and interchangeability of those very diseases of which there is no morbid anatomy? The occurrence of these neuroses cannot be adduced as evidence that epilepsy is a disease primarily nervous. I wish to put forward, again, another hypothesis; viz., that the cells suffer secondarily, as a consequence of arterial disease; that there is thrombosis or embolism of small arteries in most cases of epilepsy proper. I have held this opinion since 1864, as regards those epileptiform seizures in which there were no signs of gross organic disease. It first occurred to me on finding epileptiform seizures in patients who had valvular disease of the heart. I have since applied it to epilepsy proper. It may be said, "Your statement is only an hypothesis". So is any other view which can be taken of a disease of which there is no morbid anatomy.

REPORT ON THE ATTACK OF DIPHTHERIA IN THE GRAND DUCAL FAMILY OF HESSE-DARMSTADT.*

By DR. EIGENBRODT,

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II.—THE CAUSATION OF THE OUTBREAK.

HAVING spoken of the previous throat-affections of the members of the Grand Ducal family during the last four years, I now proceed to some remarks on the etiology of the outbreak of diphtheria.

Although Princess Victoria has been good enough to write down for me the names of all the persons with whom she had spoken on the days preceding the outbreak of the disease, I have not succeeded up to the present in finding that any of these persons or their relations had been ill. However, their number is so considerable, and my time so pressed in consequence of professional duties, that it has not been possible up to the present to carry out these investigations in such a way as I should have wished.

Professor Oertel having mentioned, in the JOURNAL for January 11th, a series of facts which militate against the idea of an autochthonous origin of the epidemic, I will now simply refer to his remarks without repeating them.

The hypothesis that Princess Victoria was infected by a slight case has become still more probable to my mind, in consequence of continued investigations. It does not yet appear to be generally known in the profession that diphtheria sometimes appears in so slight a form that the patients suffering from the affection do not feel themselves really ill, but merely indisposed; and that, consequently, they have no occasion to interrupt their usual occupations. I therefore would like to suggest that such cases, which generally escape medical observation, should be published, if some fortunate accident should afford the opportunity of recording them. It might be interesting, perhaps, to learn that I had the opportunity of observing here two such slight cases during and shortly after the epidemic in the Grand Ducal family, the patients not having had any intercourse with the New Palace. On November 27th, 1878, I was called to see a lady who was suffering from "splitting" headache. I could not, however, see her before the morning of the next day. Complaining very much about her headache, she mentioned, merely by the way, that there was a little discomfort in the throat during deglutition. The palatal arches, tonsils, and uvula were greatly congested, but only a little swollen. There was no fever. On repeated examination in the evening of the same day, there was no change to be detected. On the next day, I was assured that "the throat was all right". Examination revealed, however, that the tip of the uvula was covered with a very well developed diphtheritic membrane. This was very small indeed on the anterior part of the uvula (about one millimetre in breadth). I succeeded, however, in turning up the uvula with a spatula, and discovered that the membrane extended two millimetres upwards on the posterior side. In the evening, I again examined the throat. The membrane had extended two millimetres further up posteriorly, but had not changed otherwise. On the morning of the next day, not a trace of this affection was to be seen; the uvula and all the other parts had regained their normal appearance. If this lady had not had the headache, she would not have sent for me, and none of the prophylactic measures would have been taken, which I ordered at once in order to protect those who were about her.

On December 22nd, a cook asked me to examine her throat, because she felt some pain in deglutition. I could not see anything abnormal.

On the 24th, I was asked by her master to examine her again, because the throat had become more sore. I found the patient in bed with slight fever. There were two well-developed diphtheritic membranes on both tonsils, each about one centimetre in extent. On the following day, these membranes had completely disappeared; the tonsils were still greatly swollen and congested; their surfaces had an apparently normal condition. In order to avoid infection, the cook was removed to the hospital, and the physician in charge was at the same time told of the appearances observed on the previous day. Without this communication, a correct diagnosis would have been impossible; the patient would not have been isolated, but would have been put in one of the general wards together with other patients.

In another instance observed in 1877, the infection of a person through a very slight case could not be prevented, in spite of the patient's isolation. On June 5th, 1877, I found in a boy aged 7 a diphtheritic membrane, about half a centimetre in diameter, on one tonsil; at the same time, there was a very slight degree of fever. On the morning of the next day, this membrane had completely disappeared, the boy being quite free from fever and well in all respects. In the afternoon, I was called in again by the mother, and now found membranes on both tonsils; on one of them occupying a space about a centimetre in diameter, whilst the other was a little smaller. There was a higher temperature than on the previous day. Three days later, these membranes had vanished entirely; there were no other symptoms of disease. Nevertheless, the boy remained isolated up to June 15th, i.e., up to the tenth day after his falling ill. A female servant, who had been separated during the entire period from the patient, cleaned the sick-room on the 15th, and afterwards slept in the same. Five days later, she fell ill with headache. On June 21st, I found diphtheritic membranes on both tonsils, extending on the next day over the palatal arches and the uvula. There was at the same time high fever for two days. Cure was effected after five days. By a fortunate accident, I was also enabled to fix with certainty the period of incubation in the case of the little boy who had infected the servant. It had lasted nine days. The boy had, on May 26th, played with another who became a patient of mine on May 27th. It was a case of typical diphtheria, with swelling of the submaxillary glands and fever lasting eight days.

I have communicated these cases fully, because they appear to me to be suitable contributions towards the explanation of the origin of diphtheria. I believe that more importance is to be attributed to the propagation of this disease by slight cases than has been done up to the present by the majority of physicians.

In order to characterise the intensity of the epidemic in the Grand Ducal family, I must state at once that, in all cases without exception, there was considerable swelling of the cervical lymphatics. In the case of Princess Alice, in which the swelling was less intense than in the other patients, there were tumefactions of the glands at the angle of the jaw on both sides, attaining the size of a walnut.

Further details of single cases of the epidemic, especially regarding the course in so far the latter has not already been described by Professor Oertel, I intend to communicate in a subsequent article.

THE MISLEADING EFFECTS OF ATTENTION IN CASES OF FACIAL NEURALGIA FROM DENTAL IRRITATION.

By A. STEWART, F.R.C.S.E.,

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DR. CARPENTER's contribution, "The Effects of Attention on the Bodily Organs", in the JOURNAL of December 14th, Dr. A. Hughes Bennett's test-case in that of October 12th, and Dr. Donkin's valuable paper (October 26th), have probably convinced most readers that the effects of metal plaques, magnets, and solenoids, as observed by Charcot on hystero-epileptics at the Salpêtrière, may be regarded as what I think may be called *hysterical facts*—unreliable.

The doings at the Salpêtrière are so fully reported in the above noted and other recent numbers of the JOURNAL, that its readers are fortunately in early possession of sufficient evidence on both sides to determine their judgment; but they would do well, I think, in addition, to peruse Dr. Carpenter's volume on Mesmerism, Spiritualism, etc. If it be found that Charcot, Burg, and others, have mistaken mental for physical results, at any rate medical and surgical practice will have benefited by the prominence given to "attention" as an *ignis fatuus* in diagnosis that may beguile.

It has not been left out of consideration by careful observers, as the following extract from Holmes's *System of Surgery*, vol. iv, page 73, article "Diseases of the Joints", by Mr. Athol A. Johnstone, shows.

"When this morbid condition (hysteria) of the nervous system prevails, a local direction is often given to the perverted sensibility by some accidental concentration of the attention on the part. The physiological effects of undue attention on organs are of great interest, and serve to explain many curious phenomena exhibited in certain disturbed conditions of the nerve-centres."

The effect of "attention" in localising pain has continually to be kept in view in the practice of dental surgery, otherwise disappointment in the results of operations would be frequent.

In a large proportion of cases of facial neuralgia dependent on dental irritation, the patient has a foregone conclusion that a tooth which he indicates is the source of the pain: a conclusion so firmly held, that he has resolved on the extraction of the tooth, and is astonished and incredulous when told, after examination, that not it, but another, is really the seat of the pain. It is often difficult, and sometimes impossible, to compel belief that a tooth he had no reason to suspect, probably in the opposing jaw and several teeth distant, is the cause of all he has suffered, and should be extracted rather than the one he was prepared to lose.

As cavities that produce acute dental irritation are generally recent, sometimes cause no pain in the tooth, and are often such as cannot possibly be seen or felt by the patient, he is obliged to search for the source of pain, and any tooth on the same side with the pain that presents some altered appearance, is fixed on, and immediately, somehow or other, has pain transmitted to it. A dark stain merely—a chip, probably an old one—a root however small, and especially a cavity plainly seen or felt, suffices to direct attention and pain to a particular tooth, although the stained part be sound, the chipped surface harmless, the root incapable of mischief, and the cavity dormant. There being dental irritation somewhere, the most trivial change of appearance in a neighbouring tooth suffices to locate the pain in it and stop the search for its real seat. Hence the numberless extractions performed without benefit; the operator being guided by the patient, especially when a tooth with a cavity is confidently stated to be the painful one. The opinion of the patient should go for little in such cases, and ought never to prevent a thorough examination. It is no excuse for an insufficient examination, and consequent extraction of the wrong tooth, that the patient insisted on its extraction; yet I am sure that, among the poorer class, no further inquiry is made in most cases than "Which tooth is it?"

Hysteria, in this manifestation of the effects of "attention," plays no part. There are the realities, inflammation and nerve-irritation. Why the resultant pain can be fixed as described, depends probably on the common origin of nerve-supply to the teeth permitting the action of a subtle mental power over sensation that will no doubt, sooner or later, have a physiological explanation.

CLINICAL MEMORANDA.

GARGET AND DIPHTHERIA.

FROM the description given by Mr. Smee, at the Pathological Society, of that form of cow-disease called "garget," it would appear to be of an ordinary inflammatory character, arising sometimes from cold, sometimes from blows, sometimes from retained secretion, and sometimes idiopathically. When it results from cold, there would seem occasionally to be enlargement of the glands of the neck, and here and there paralysis of the legs, mostly the hinder ones. Were it a disease of a septic character, it would certainly be conveyed to almost every cow in the drove; and, were it of a distinctly diphtheritic nature, we should expect to find patches of membrane in the throat and on the udders. If diphtheria were capable of being conveyed to man by the milk of a cow which is simply suffering from a disturbed state of the system, then we should be compelled to alter our present belief, that like produces like, and accept the theory of heterogenesis, which, I consider, as yet has no certain foundation whereon to rest.

During the past year, we have had a slight outbreak of diphtheria at Surbiton, caused, I believe, by the mixing of sewage-contaminated water (which had been used for washing the cans) with the milk. It was limited entirely to those who had been supplied with milk from one particular dairy, and ceased almost immediately upon a change being made in the water. If milk, after the process of milking, be capable of contamination, and of producing, as has been proved, scarlatina and enteric fever, it can equally well, I should say, produce diphtheria and cholera. In order to be able to assign the cause of any particular septic disease to the cow, I cannot help thinking it is necessary for the animal to have the particular train of symptoms peculiar to that disease.

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GARGET.

WITHOUT at all entering into the merits of the theory of a connection between diphtheria in the human subject and some one of the several affections of the udder in the cow known as "garget," I have thought that, at the present time, the following statement of facts might be useful, both as a precedent in dealing with cases of "garget" and as indicating what in one case at least proved a ready chemical test for evidence of that disease in milk.

Eighteen months ago, I received information that, amongst the cows at a dairy-farm, supplying milk for sale to dealers in a town for which I am medical officer of health, was a cow suffering from garget. A veterinary inspector was at once despatched thither, and found this to be the case; the fact, indeed, was not denied by the owner of the cows. All the milk ascertained to have been sent from the farm in question and remaining unsold, was in consequence seized by the inspector, and I gave a certificate that it was unfit for human food, on which it was destroyed by a magistrate's order under Section 116 of the Public Health Act. The sanitary authority, on my report, subsequently gave directions for proceedings for a penalty to be instituted in the case. In the absence of evidence of disease following the consumption of that portion of the milk which had been sold, it was necessary for me to state in evidence on what grounds I considered the milk to be unwholesome. Although microscopic examination gave undoubted evidence of pus-corpuscles, it was difficult thus to demonstrate small quantities of blood in the milk. The guaiacum test for blood, however, gave a well marked and unmistakable reaction with the garget milk, but not with ordinary milk. To a small quantity of the garget milk on a watch-glass were added a few drops of tincture of guaiacum, and then of solution of peroxide of hydrogen (in the shape of a few drops of sanitas fluid); a beautiful and persistent blue tint was at once developed.

On my evidence that the milk contained pus and blood, a conviction ensued, and a heavy penalty was inflicted.

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DIPHTHERIA AND TYPHOID FEVER.

THAT the same poison which originates typhoid fever often causes diphtheria; that the two diseases often occur side by side, in different individuals, in the same house; and that, therefore, we must look for their same source of origin, the following cases show.

1. On October 30th, 1875, I was called in consultation some distance in the country, to a large private school at V—, to see Mr. R., who was supposed to be suffering from typhoid fever, though he was not. There were, however, five or six cases of typhoid fever in other rooms that I saw. While these cases were in progress, and within a few days of my visit, a younger child developed pharyngeal diphtheria and died. These cases occurred at a large house, standing by itself in several acres of ground; they were traced to the well-water (there was no other water-supply); there had been a great rainfall, the cesspools had filled and overflowed, and, the well being near and on a lower level than the cesspools, the overflow had found its way into the well, which was marked by making the side of the well at which it entered slimy and blackish.

2. At the beginning of January 1875, Mr. T. of Rugby developed diphtheria, which was followed by paralysis. In the same house, on January 15th, 1875, Mr. T.'s servant sickened with typhoid fever, went through a severe illness, but recovered. During the last days of December 1874, there being something amiss with the pipes which carried the water-supply from the town to the cistern, the water was cut off for one day to remedy the defect. On that day, a pump in a yard at the back of the house was used for their water-supply; this well was much fouled from adjoining stables. There was, however, another possible source of poison; for the trap to the water-closet was defective, and allowed an escape of sewer-gas into the house. Both sources of poison were remedied, all the family were sent out of the house, and we had no further cases.

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SEPTICÆMIA AND SCARLATINA.

IN connection with Mr. May's communication on the above subject in the JOURNAL of December 21st, I wish to mention a case which occurred lately in my practice. On the 11th of September last, I was asked to visit Mr. M., a farmer, aged about sixty-five, who resided in the country. I saw him about 7 o'clock p.m. He was lying in a dull stupid state, and could not be roused to complete consciousness, although he made an attempt to answer when urgently spoken to, and apparently tried to put out his tongue when I ordered him. The pulse was 120,