

Bramwell, the evidence showed that he was laboring under delirium tremens. After the act he grew calm and said that he knew perfectly well what he had done and that his wife was in league with men who were hidden in the walls. "Baron Bramwell, who favored hanging insane men who committed homicides when acting under an insane delusion, if of sufficient intelligence to understand the nature and quality of the act and its consequences, tried the case and charged the jury, "That there were two kinds of insanity by reason of which a prisoner was entitled to be acquitted; probably the jury would not be of opinion that the prisoner did not know the quality of his act, that it would kill and was wrong, but it was still open to them to acquit him, if they were of opinion that he was suffering from a delusion leading him to suppose that, which if true, would have justified him in the act; one more remark he would make, viz.: That drunkenness was no excuse and that a prisoner cannot by drinking qualify himself for the perpetration of crime, but if through drink his mind had become substantially impaired, a ground of acquittal would then fairly arise.

The prisoner was acquitted. Under the English law, there is no right of appeal to the convicted homicide as in the American States, and so it is difficult to find the decision of English higher courts on the questions involved in the discussion.

In the American States no person is executed except on the decision of the highest court of the State if the accused desires it and appeals; in England the appeal does not lie as a matter of right and so the opinion and dicta of the English trial judges form the real body of the law of England upon these questions.

Baron Bramwell undoubtedly regarded Watson as entitled to an acquittal, and the case shows a remarkable result in this respect; had he been insane and committed the homicide under delusions which dominated his will and controlled his action, he would have been convicted if he had sufficient intelligence to understand the nature, and the quality of the act, but the drunkenness which had caused the attack which resulted in delirium tremens with a diseased condition of the brain also resulted in a delusion, which controlled his mental powers, so as to render him irresponsible at law.

In 1888, Baron Pollock held, that the law was the same where insane predisposition and not physical weakness was the proximate cause of the intoxication." The English Home Secretary, Mr. Mathews is one of the ablest men connected with the English Government.

Under the English system he has the power to commute or modify the sentence of the courts in criminal cases, and he exercises it with as much effect and more in many cases than would the reversal of the Appellate court, if the right of appeal existed.

No eye in Great Britain sees more clearly or more intelligently the action of the criminal courts than his. It is his province to correct errors and redress grievances and abuses, if such exist or occur in the criminal jurisprudence of Great Britain. He has recently named a commission composed of Mr. J. S. Wharton, Chairman, Sir Guyer Hunter, H. P. Mr. E. Leigh Pemberton, Assistant under Secretary of the Home Department: Mr. Daniel Nickolson, Super-

intendent of the Broad Moor Criminal Lunatic Asylum, and Mr. C. S. Murdock head of the Criminal department, to inquire into the best mode of treatment and punishment for habitual drunkards.

Mr. Matthews says regarding the appointment of this committee, "Great difference of opinion has arisen as to what kind and degree of punishment for offenses committed by habitual drunkards would be the most effectual, both as a deterrent and with a view to the reformation of such offenders; it appears to me that advantage would result from an inquiry being made into the subject." It may be fairly claimed so far as the British Islands are concerned that the old common law rule no longer is enforced there, and that inebriety as a disease is now not only recognized as an existing fact, but that the jurisprudence of that country is receiving such modifications as are necessary to fit it, for the advance made by scientific research.

We are doubtless near similar results in the American States.

## ON CHOREA, VIEWED FROM THE STAND-POINT OF COMPARATIVE PATHOLOGY,

WITH AN ENTIRELY NOVEL THEORY AS TO THE CAUSE OF THE MUSCULAR TREMOR AND INCOORDINATION, AND A NEW THERAPEUTICS.

Part of a Clinical Lecture delivered February 3, 1893.

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[Stenographically reported for THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION.]

*Gentlemen:*—I am going to occupy your attention this morning with the discussion of a subject which is of great scientific interest, and which may prove to be a discovery of considerable practical importance. Some years ago, as you know, I gave much attention to the subject of the physiology of the choreic movements. At that time, I made a number of experiments upon the lower animals, especially upon dogs, and succeeded in demonstrating, in the dog, that these movements had their origin in the spinal cord. I found that in the choreic dog, after dividing the spinal cord in the dorsal region, the movements continued in the posterior extremities just the same as before, save that, after the division of the cord, the muscular movements in the fore-limbs and hind-limbs were no longer synchronous; that is to say, rhythm did not agree as before.

On coming to apply these observations to man, I must tell you that we have in the human subject two varieties of choreic movements: in one form, they correspond with those in the lower animals. This is the variety known as "St. Vitus' dance," which is rare in adults but common in children, and undoubtedly affects the spinal cord, as I shall presently explain. The second form is common among adults but rare in children. In it the movements are more violent and even convulsive. These have their origin in the cortex of the brain, from minor lesions in the motor areas; but there may be widespread and destructive lesions in the motor regions of the cortex. I may illustrate this by a case that I saw several weeks ago. It was that of a boy, who was found in an out-house, unconscious, pulseless, appar-

ently dying, and, indeed, at first, was thought to be dead by those who found him in this condition. He was suffering from poisoning by inhaling coal gas. He was carried into his home and resuscitated; but it was found, after regaining consciousness, that he had violent, apparently purposive movements of the arms and legs, which he could not control. The limbs were thrown about by irregular, muscular contractions, and he was kept in bed with difficulty; in other words, he had a furious attack of acute, cerebral chorea. I attributed the chorea to multiple cortical embolism, the result of the arrest of circulation during the period of suspended animation, and I said that the case would die. The patient did die a few days afterward, but the choreic movements never ceased until death. Owing to some misunderstanding, I was not at the autopsy, and no parts of the brain were saved. It was found intensely congested, but there was no careful microscopical examination of the cortical regions of the brain, such as would be required to establish my diagnosis. This is to be regretted, but cannot now be helped.

Returning now to the form of chorea first mentioned, which usually occurs among children, I have arrived at the conclusion that the cause of the choreic movements is a paralysis, or depression, of the inhibitory function of the cells of the spinal cord. You know, from your studies in physiology, that there are in the spinal centres, as elsewhere in the brain, motor cells, and others which inhibit or arrest the discharge of motor impulses. It has been denied that inhibition exists; but I think the researches of Gaskell have demonstrated that in the heart (and as I believe, everywhere in the body), there is a period of functional excitement with discharge of force and destruction of tissue, and a period of functional quiet with repair of tissue. The accelerator nerve stimulates to functional activity; the inhibitory nerve to functional rest and reparative activity.

Now, it has occurred to me that the choreic movements in the form under consideration, as they occur in the child, are due to loss of inhibition. If you will observe a choreic child attempt to pick up an object such as a piece of paper from a table, you will see that the muscular movements are exaggerated so that the hand moves through a much wider range of motion than in health, the result being that the child will be obliged to make several attempts before the object is attained. The wider range of motion in this case is evidently due to a failure to arrest the motor impulse at the proper period. There has been a failure of the inhibitory function of the spinal cord to arrest the movement at the proper time. This explains the causation of the peculiar muscular movements in this form of chorea. I believe that it is based upon a new observation in the physiology of the spinal cord; it is new, at least, as far as my reading goes.

For some time I have had this in my mind, and have been casting my thoughts around endeavoring to find out how to confirm this theory. It finally occurred to me that I had been teaching you, in my lectures on therapeutics, for many years, that quinine has the property of exciting the action of the spinal inhibitory cells. You recall Chaperon's experiments, which established the fact that in small doses quinine causes in frogs a lessening of reflex activity, which, however, is restored by section of the medulla, thus showing the independent action

of the spinal cord when relieved from the inhibition coming from above. This led to the discovery of the inhibitory centres of the medulla by Setschenow and the location of Setschenow's centre. Small doses of quinine stimulate Setschenow's centre, and lessen reflex activity; but large doses cause permanent paralysis of reflex activity. It is evident that the larger doses act upon the spinal motor cells in the same manner that the small doses affect the higher centres. There are many other facts which demonstrate the existence of spinal inhibition; but their discussion would lead us too far afield, especially since they do not bear directly upon the subject under consideration to-day.

Some years since Ringer and Murrell brought forward the theory that atropine is a paralyzer of inhibition. You remember the experiments of Fraser. He had killed some frogs with atropine, as he thought, and threw their bodies at night on the laboratory floor; but the next morning, on coming into the room, to his surprise he found all the frogs alive and in active convulsions. The only plausible explanation of this is that atropine, in the proper dose, paralyzes both spinal motion and motor inhibition, and that the effect of inhibition passes off and allows excessive motor discharge, and this produces violent muscular movements, not because of motor excitement, but because the restraining force of inhibition has been lifted off from them.

Sedgwick, of London, some time ago, objected to the theory of spinal inhibition, because of his discovery that atropine prevented the primary loss of reflex movements from being produced by quinine, which I referred to a few moments ago, as demonstrated by Chaperon. I think, however, that this fact cited by Sedgwick is really in favor of the theory, rather than in opposition to it, as he supposed. Quinine stimulates inhibition and atropine paralyzes it, therefore atropine would naturally set aside the physiological action of quinine. Does it not become apparent to you that Ringer and Murrell are correct in explaining the action of atropine? Can it be otherwise than that atropine and quinine oppose each other in their action upon the spinal cord?

In casting around for some means of testing my theory of the production of choreic movements, I first obtained some choreic dogs, to try the therapeutic experiment upon. I will first call your attention to this long roll of blackened paper upon which you observe this kymographic tracing, the register of the dog's choreic movements. Here you notice the continuous and rhythmical contractions of the muscle indicated by the needle upon the paper as it revolved upon the drum of the instrument. At this point where the movements begin to increase in their range, the animal had a hypodermic injection of atropine. These injections were repeated until two and a half grains (Gr. 0.15) had been given. Coincident with the administration of atropine the tracings show increase of amplitude, until they are increased fourfold. This increase seems to be attributable to the effect of the injections and, thus far, the results are exactly in accord with previous reasoning. The animal at this point, had an injection of six grains (grams., 4) of quinine, and immediately the movements declined and, in the course of from twenty to thirty seconds, entirely ceased. This is beautifully shown by the tracing. The amplitude of the excursions of the needle rapidly diminishes until they

cease absolutely. Now as to this action of quinine there can be no possibility of doubt. The atropine I have only experimented with once in a case of chorea; but the quinine I have used in a number of dogs and am positive that the quinine arrests the choreic movements in dogs, so that, as far as this goes, it carries out my theory.

My next line of investigation was to study the condition of the reflexes, such as the knee-jerk, in chorea. *A priori*, what would we expect to find in regard to the knee-jerk, in chorea? The motor-cells of the cord are in a state of weakness in a case of chorea; but the inhibitory cells are in a state of greater weakness. This would lead us to expect that in the first place, the reflexes would be less than normal. When, however, a voluntary movement is made, on account of the failure of inhibition, the motor impulses from neighboring cells would overflow, and reinforce the motor impulses beyond the normal amount and the reflexes would be increased. (Explanation of reinforcement, illustrated by drawing a diagram of spinal cord with motor and inhibitory cells, upon the blackboard.) In the normal condition, this overflow would be checked by the inhibitory cells, but when these are weakened experimentally, or by disease, it is not so checked and the reflexes are extraordinarily increased. I wrote to Dr. Wharton Sinkler requesting him to communicate to me his observation as to the condition of the reflexes in chorea. He replied that the phenomena are different from what might be expected, the reflexes are diminished in all cases of chorea; but that he had observed the curious fact that the knee-jerk in chorea is greatly increased by reinforcement. That is to say that, at the time the knee-jerk is tested, if some other muscular movement be performed, like extending the arms or tightly clenching the fists, the knee-jerk is greatly exaggerated. This I explain by the weakening of inhibition and the consequent overflow of motor impulse as already mentioned.

So far this is of scientific importance only. It appears to me as if, in the practical study of chorea, we have hitherto overlooked the phenomenon of inhibition. The phenomena of chorea, like those of hysteria, are not phenomena of increased excitement of motor centres; but of paralysis of inhibition. The conjunction of increased muscular and nervous discharge with evident weakness of the spinal centres is explained by the fact that the motor cells are weak; but the inhibitory cells are weaker still. Sometimes in chorea in the child, as commonly in the dog, the movements are rhythmical, and although they are constant the child does not get tired. No one in health could continue this motion for half an hour without fatigue. How can this be explained? In the anatomical rooms, you have learned to refer anomalies to a reversion to a lower type. You have seen how a stump is produced by evolution to a certain point for a certain purpose in the case of the animal, and that is altered by higher evolution in man. Now, I believe, that we may have a physiological reversion as well as an anatomical reversion, and applying this to the spinal cord, we may have a reversion of the function of the motor cells to those of the lowest type of nerve-cell in which the discharge of force is rhythmically continuous. Just as the beating of the heart never ceases during life, so it is the function of the primitive nerve-cell to unceasingly give off force. The strange phenomenon

of lack of tire in choreic movements, then may, in this way, be explained on the principle of reversion,

Applying these investigations to cases of chorea. It occurred to me to give quinine a trial. I may say, that it has been only during the last ten days that I have thought of this subject from the clinical standpoint. I have thus far had the opportunity of employing it in only one case in the human subject. It was a child with well marked chorea, brought by her mother to the Out-patient Department. She was ordered full doses of quinine, and was brought back in four days with the report that she was very much improved. I spoke to Dr. Pearson, of the Veterinary Department, and asked him to try the alkaloid in choreic dogs. He told me in the first place that he had never found anything that would do choreic dogs any good; his experience is that chorea in dogs is incurable. Here is an English setter dog suffering with chorea. One week ago he could not stand upon his feet, owing to violent choreic movements. He had quinine given freely by the mouth and within twenty-four hours, the movements decreased; he now walks about and is apparently almost well; at least, he has very little left of his chorea. So far as practical results go, it therefore looks as if the theory would stand the test of experience; but whether our present prospect is simply a mirage or not, time alone can tell.

I now bring before you two children suffering with chorea. The first one is seven years of age, and small and poorly developed for her age. The second is ten years of age and appears to be in a better nourished condition; but in the second, the chorea is more marked and of shorter duration than the first, in which, according to the mother, it has existed more or less for four years. I will put both of these patients upon the quinine treatment and will report the result at our next meeting.

## ADVANCEMENT OF SURGERY.

A Paper Read before the Vigo County Medical Society, April 1890.

BY JOHN E. LINK, M.D.,  
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Why I should have been selected to read a paper to the Society on the "Advancement of Surgery" is incomprehensible to my mind, unless it was for the purpose of inducing me to post myself more thoroughly upon the literature of the subject. Sure it is, that I, probably of all the members, have paid less attention to the serial productions, as published in our overloaded journalism, than any other member here. And I think it is a well understood fact, that I read less of our periodical literature, as well as our secular papers, than any other member of this community. What has stood the test of ten years' popular favor becomes a part of our texts; and compared to-day with what has been introduced as novel and good, not one-tenth part bears the test of the decade. For this reason I have declined to place any of my thoughts or methods into the hands of publishers, and only a few of the things that presented themselves as original, from observation or thought, have found their way into print through the pen of others more ambitious to write. And I say to you to-night that I had far rather have come here before you, presenting a patient with oral explanation, than to attempt to write. It is generally understood that the only way for a man to bring himself into prominence