and says that they only differ in the attacks being multiple, and in the circumstances under which they occur. Neither of these features seem to me to establish any material distinction between the two classes. Multiple attacks are, we well know, met with every now and then in chronic epilepsy. As to the attendant and originating circumstances, when the visible result is so similar, it seems to me very much like begging the question at issue, to affirm that the condition of brain they produce is unlike that which prevails in all cases of essential epilepsy. It is fully conceded that, among the cases of epilepsy occurring in our large towns, it is rare to meet with one where the morbid condition is not one of anæmia and weakness and undue mobility, rather than of sthenic excitement and plethora; yet I can by no means admit that the latter condition is to be excluded from our list of causes, and I believe the case I narrated at the beginning of this lecture to afford a fair instance in point. To the above evidence must be added that of Schröder van der Kolk, to whom we are much indebted for his minute examination into the changes produced in the nervous centres by this disease. He entirely dissents from the view that suddenly induced anæmia is the essential precursory condition of the epileptic attack. Among other observations, he says: "Equally little does this view find support from the fact that, before an attack, epileptic patients are usually more excited, more lively, and more irritable, and their face and their head manifest a greater degree of congestion. In one case, I was able to ascertain this with precision. In a young man aged 19, who, under the use of digitalis, had continued for a considerable time free from attacks, I found the heat of the head excessively great in comparison with that of the cheek," the temperature of the forehead and vertex exceeding that of the cheek by 13° Fahr. Scarcely fifteen minutes after this observation had been made, a violent attack came on. (Sydenham Society's translation, p. 299.) He has found positive advantage from the use of derivatives, issues, and setons or cupping to the neck; not only the epilepsy, but the consecutive dementia, being removed or greatly amended thereby.

I have often spoken to you of the varying quality of inflammatory disease; and I hope, from this review, you will be inclined to agree with me, that the same holds true of convulsive disorder.

PLUGGING THE POSTERIOR NARES IN CASES OF PERSISTENT EPISTAXIS.

By J. INGLEBY MACKENZIE, M.B. Cantab., M.R.C.S.,
Surgeon to the Sidmouth Dispensary.

THE remarks on this subject by Dr. Thompson of Bideford, and his account of his method of introducing a plug into the posterior nares, are most interesting. A somewhat similar case came under my observation within the last few weeks.

I was sent for to a patient, of full habit, subject to frequent attacks of rheumatic gout, who was suffering from epistaxis. Plugging the anterior nares had been adopted prior to my arrival, though ineffectually. This was repeated, the plugs being saturated with tincture of matricaria. Cold affusions were applied, but without success. I then proposed plugging from behind. I passed a No. 10 elastic catheter (in which I had cut an opening opposite to the usual one at its extremity) through the nostril, till I was able to seize it and draw it forward through the mouth, and passed the string to which my plug was attached through the eye made in the catheter, which, when withdrawn through the nasal orifice, enabled me, by tension on the string, to bring my plug into its required position. I fixed the end of the string to the forehead with strapping. The effect of this plugging was satisfactory; and it became necessary in three or four days to remove the plug.

I have failed to find any account of this part of the operation in any of the text-books at hand; but the plan adopted was a very simple one. I re-inserted the string in the eye of the catheter, and ran the catheter along the string up the nostril, detaching the plug, which had previously been loosened by injections of lukewarm water, caught the plug at the fauces with dressing-forceps, and simultaneously withdrew it and the catheter from the nostril.

The operation, minor as it is, is one requiring some little manual dexterity—the more especially if the patient is, as in the case to which I referred, highly nervous; which feeling was greatly increased by the sense of suffocation experienced during the operation. Bellocq's instrument is not always at hand; but there are few surgeons who do not carry an elastic catheter in their pocket-cases, and consequently have the means ready to give relief to a state which is to a patient at once distressing, if not terrifying.

CONTRIBUTIONS

TO A NEW CHAPTER IN THE PHYSIOLOGY AND PATHOLOGY OF THE NERVOUS SYSTEM.

By THOMAS LAYCOCK, M.D.,

Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh.

II.—THE THEORIES OF NERVOUS INFLUENCE ON NUTRITION AND FUNCTION.

THE nervous system influences the nutrition and functional activity of its own tissues, and of the tissues and organs generally, in various ways. First, the so-called influence of the mind on the body is due practically to the direct action of certain encephalic centres subservient to thought and feeling upon other encephalic and special centres, and through these upon tissues and organs. Secondly, the influence of the body on the mind is due practically to the reverse influence of afferent nerves, tissues, and organs upon certain special and encephalic centres, and through these upon the cerebro-mental organs. Thirdly, sympathies of organs as to function and nutrition are due to the action of organs and tissues upon certain nerve-centres, and through these centres upon other tissues and organs. An example of this kind is the sympathy between the uterus and the mammae. When motion and sensation accompany this class of sympathies, they are termed diastaltic and reflex actions—as, e.g., vomiting during pregnancy. Fourthly, the motor centres act directly upon the motor structures, whether they be voluntary, involuntary, or vascular (vaso-motor), and so regulate motion of limbs and organs and the distribution of the blood. And, fifthly, the system of motor centres regulates the processes of secretion and nutrition, the temperature of the tissues, and those fundamental processes included under the term *eremacausis*, in which chemical affinity plays so important a part. For about three centuries, scientific and clinical research has been directed to the elucidation of these points, with more or less clearness of perception as to their bearings on each other, but very often with only a dim knowledge of the true relations of things. We have the same difficulties to overcome now as were encountered formerly; so that a history of the methods hitherto followed, and a note of the causes of failure, will help us to a better plan and greater success.

Very soon after the revival of letters in the fifteenth and sixteenth centuries, the advances made in human anatomy led the way to a clear perception of the fact that the nerves and nerve-centres had a direct influence upon the nutrition of tissues, and on functional activity generally. A learned lady named Oliva Sambuco (*Docta Mulier*, Haller, *Element. Physiol.*, tom. iv, p. 404) seems to have been amongst the earliest to attempt an explanation of it. She looked upon the body of man as a tree having its roots in the brains, where a juice, termed *succus alibilis*, was taken up, and diffused thence throughout the body, as well for nutrition as for emotion and sensation. As medical science advanced, the nervous juice or *succus* became a *spiritus*; but the theories of the "animal spirits" did not differ fundamentally from that of a juice. The brains were still considered the source of the influence which modified nutrition, and the nerves the channel. Speculation, clinical observation, and experimental and anatomical research, were all industriously used to unravel the mystery, and essays and treatises followed each other rapidly during the whole of the last century. Voss, in his tract, *De Nutritione Imprimis Nervosa*, 1789, gives a good critical summary of all that was known physiologically up to that date. The hypothesis of all that was known physiologically up to that date. The hypothesis of animal spirits had vigorous opponents, especially towards the close of the century, when the phenomena of electricity and galvanism began to be more carefully investigated; and an antagonistic theory was advocated—viz., that the nerves were solid tense cords, and conducted vibrations, and not a fluid. Thaer of Göttingen, in his treatise *De Actione Systematis Nervosi in Febris*, discusses exhaustively the neurology of fever, both physiologically and pathologically. It is a question of perennial interest; and it will perhaps surprise many to be informed that all the chief facts of modern vaso-motor physiology and pathology are lucidly brought under review by Thaer. Boerhaave, Swenke, and

Brown Langrish had already promulgated a theory recently advocated by Virchow and others, and confuted by Haller, that heat is developed in fever by the increased friction of the blood in the vessels. Indeed, the age went beyond modern doctrines. For example, there was a theory (first promulgated by Willis) as to the cerebellum being the source of the animal spirits, and as to its influence by their means on febrile phenomena, not known to modern inquirers. Nor were precise clinical observations wanting. De Haen anticipated the leading results of recent thermometric observations in fever. He used the thermometer clinically, and demonstrated, amongst other points, that in the cold stage of fevers, and with rigors and an extreme sensation of cold, there was actually a largely increased temperature—as high as 104°. Dr. Home also found the same. The relations of the pulse to heat were also carefully observed, and the influence of heat generally on vital action. (De Haen, *Ratio Medendi*, tom. ii, p. 8; Home, in *Med. Facts and Experiments*, p. 220-21.)

The influence of the nervous system on the blood is a question lost sight of, I think, in modern discussions as to change of type and the therapeutic value of blood-letting; yet the fact that the first and second cups of blood drawn had a layer of fibrine ("inflammatory crust"), which the subsequent cups had not, was formerly made a matter of observation and inquiry, and finally referred to the nervous system. There was a school of mathematical physicians, who attributed the difference to the greater velocity with which the blood flowed after the first cups were drawn; but Hewson found, after experimental research, that there was a diminished proportion of fibrine in the later drawn blood, even when there was no appreciable difference in the velocity, nor, so far as he could discover, in any other circumstance. It was consequently argued, that there was a change in the composition of the blood, due to a change in the influence of the nervous system, which latter was caused by the blood-letting (*Speculations and Conjectures on the Qualities of the Nerves*, by Samuel Musgrave, M.D., F.R.S.) In common with Frank and others, Musgrave also called attention to the important clinical fact, that changes in the nervous system had a share in the causation of dropsical effusions. The fact as to dropsy may be easily verified,* yet it is constantly overlooked.

The sympathies of organs and parts had formerly a large share of attention, on account of their practical importance; and many efforts were made to elucidate them. The adaptation of parts and organs to each other was so obvious, that it necessarily entered largely into the theories of the day. The facts bearing on neurology were well observed and clearly stated. For example, the tension and inflammation of the gums in dentition were recognised as the exciting causes not only of the gastric and intestinal disorders of dentition, but also of infantile convulsions. It was as to the explanation of this *consensus* (as they termed it) between the facts, that writers differed. The Stahlian, of whom Platner was a leader in Germany, and Whytt in a modified way in Britain, maintained that these sympathies were due to the action of the brain, or of a hypothetical sensorium commune, the seat of the *anima*, which was the absolute cause; but sensation and consciousness were held to be identical with the *anima*. Vieussens and others, but more especially John A. Unzer, maintained, however, an opposing doctrine. Unzer was the first to differentiate clearly the various forces operative in organisms, and may be said to have been the founder of modern mental science, as distinguished from speculative philosophy and the physiological systems founded thereupon. Haller, too, although in opposition to the Stahlian with his theory of a *vis insita* (who, however, objected to it, that it was only a sort of elastic force), also affirmed with Whytt, that sensation was the immediate cause of adapted actions. In this way, the *consensus* of parts and all morbid sympathies and reflex actions were generally attributed either to an *anima* or to sensation. Now, Unzer was clearly opposed, on this fundamental point, not only to Stahl, but also to Haller, Whytt, and all their numerous followers, down to Alison, Reid, Carpenter, and the many moderns who have discussed the facts with a view to an explanation. He maintained, in the most explicit language possible, that it was not to sensation or consciousness, but to changes in the nerves and nerve-centres, that the *consensus* and sympathies of parts were due. He was the first to use the phrase reflex action as explanatory of this theory of causation; and he affirmed that these actions occurred "aut conjunctam cum conscientia aut non conjunctam." A few years subsequently, Prochaska adopted Unzer's views in part, together with his phraseology, and especially maintained with him that reflex actions occur "mens inscia vel vero conscia." So overwhelming, however, was the influence of the dominant school of sensation, that these views passed almost wholly out of consideration; and, when recently revived, it was thought by many the ques-

tions involved were merely speculative. It is curious and instructive to observe how precisely the same prejudices operated then as now against the reception of the plainest facts. Mind and mental phenomena without consciousness, says the modern objector, is a contradiction in terms. How can you possibly have sensorial action without sensation? And so Platner, criticising Unzer's doctrines and facts, declares, in the lofty style some philosophers are apt to assume, that they are intolerably absurd: "Sunt ista quidem ejusmodi, quæ ferri non possunt sine indignatione; stimuli nervorum prætereuntes cerebrum, motus a nervis absque cerebro suscepti: cerebri impulsus sine animi perceptione; actiones animales sine animo; animus sine animale vigore." (*De Causis Consensus Nervorum Physiologicis*, Lipsie, 1790.) An astounding list of heresies, indeed, and a lesson for the intolerant.

The position which Marshall Hall occupied in 1832-33, when he promulgated afresh the doctrine of reflex action, was that of a modified Stahlian. While he stoutly maintained that the reflex actions dependent on a portion of the cerebro-spinal centres, which he subsequently termed the true spinal system, were essentially independent of sensation, because unconsciously performed, those with which sensation and volition were constantly associated he attributed, with Haller, Whytt, and others, to the sensation or the volition. He declares: "I have restricted reflex actions to the true spinal marrow. Those who have imagined that a similar reflex action belongs to the cerebral or to the ganglionic system, have, I think, only confused the subject; and added what is really hypothesis and conjecture." (*New Memoir on the Nervous System*, 1843, p. 4.) With him, the "cerebral system" was the seat of the "soul, the immortal soul". In truth, on these points, he wholly differed from Unzer and Prochaska. As to the sympathies between organs and tissues involving nutrition, he was silent. How, for example, a certain condition of the testes in song-birds excites to song or causes the growth of male appendages, did not enter into his physiology at all; and this in common with most modern physiologists. The sympathies as to nutrition, and of the body and mind, remained unexplained by the doctrine of reflex action.

Dr. Marshall Hall was an ardent expounder of the Hallerian hypothesis of a *vis nervosa*, which had displaced the hypothesis of a spirit, just as the latter displaced that of a juice or fluid. This *vis nervosa* also partly displaced in his etiology the *anima* of Stahl, and the sensation and consciousness of Haller and others; upon this point he followed Prochaska. The discovery of Galvani, towards the close of the last century, gave more definiteness to the doctrine; and recent researches into the relations of heat, electricity, galvanism, and magnetism, have not only led to the theory of a correlation of forces amongst physicists, but also brought them into more philosophical relations with the *vis nervosa* and the vital forces. The tendency of the new school of physics is obviously both metaphysical and mathematical; this is particularly manifest in the new views as to chemical affinity. Hitherto, however, it has not touched upon the teleological questions involved in the relations of sensation, consciousness, and the will, to the chemical and other changes influenced by the nervous system and the *vis nervosa*. Nor is modern physiology more available; for, at this moment, physiologists maintain the old doctrine in all its fulness, that sensation and other states of consciousness are the immediate causes of adapted acts. Some even distinctly affirm that the sensibility of the spinal cord is the cause of the adapted or purposive actions manifested by a headless frog. This question is, in truth, one of fact and not of theory; and when it is solved, a large further step in advance may be made. I shall endeavour to solve it in another communication, and show that it is, and has been so long, a subject of controversy because of errors in method; in short, that the fact is to be determined by clinical observation, and that alone. With the solution of it, various terms now vaguely used, such as irritation, sensation, sensibility, consciousness, and the like, will either have to be used in a more definite sense, or else give way to terms drawn from the phraseology of the sciences which treat of molecular forces and motions.

While the controversies as to spinal reflex action were going on, a new set of phenomena manifested through the brain, and hypotheses as to new forces founded thereon, began to attract attention in England. These not only involved old questions as to sympathies, and as to how far consciousness and will were causes of the phenomena of life and mind, but extended into regions of speculation hitherto untrod either by science or philosophy. Although Sir W. Hamilton advocated the

* As I have shewn in my paper on the Influence of the Nervous System in the Production of Dropsies.—*Edin. Med. Journal*, March and April, 1866.

* I was the first to direct attention to Unzer's great work, and to his influence on Prochaska, in a review published in the *British and Foreign Medical Review* for July 1847. In consequence of this notice, I edited, in 1851, a translation of both Unzer's and Prochaska's works for the Sydenham Society. Unzer's first treatise, systematically developing his views, appeared in 1790; this complete exposition twenty-five years later. In the review itself, and in the introduction to the translation, will be found an examination of these questions. I have also systematised my views, which differ fundamentally from current theories, in my work, *Mind and Brain; or the Correlations of Consciousness and Organisation*. Simpkin and Marshall.

doctrine of Leibnitz, that cerebral activity might, and did, go on independently of will and knowledge, he did not mean to affirm, also, that it was independent of consciousness. On the contrary, he maintained in all its force, albeit in another shape, the fundamental physiological doctrine of Stahl, Haller, and others; and he endeavoured to prove, by observations on himself, that, even in the most profound and apparently dreamless sleep, consciousness still continued. Now, this fundamental point involved all the cerebral or higher phenomena of the nervous system, as well as the spinal or lower, such as sensation, and consequently all those included under mesmerism and its offshoots, electro-biology, phreno-mesmerism, and spiritualism. Mesmerism and homoeopathy arose contemporaneously during the last century; both were legitimate descendants of preceding schools. Mesmer was the successor of Van Helmont, who was born at Brussels in 1577, and died in 1644, and was influenced by C. Agrippa. Van Helmont perceived in all bodies a physiological principle of movement which he termed *Blas*, very much as Von Reichenbach thinks he discovered a new force in nature which he termed *Od*. The term *Gas*, by which Van Helmont designated aeriform matter, is now in universal use. His sentient and intelligent cause, termed *Archæus*, was the antecedent to Stahl's *anima*. The fundamental dogma of homoeopathy, that like cures like, was an offshoot of the Hermetic school of medicine of the previous century, which deduced the therapeutical doctrine of signatures from the same dogma. With this was joined a mystic alchemy in the doctrine of infinitesimal potencies. These were obviously absurd; not so the therapeutical results. And, when taken in connection with the curative effects of mesmerism, they led to the consideration of two practical points; namely, the influence of the mind on and through the brain on the production and cure of disease, and the influence of "nature" in restoring to health. The new ideas thus arising have advantageously influenced therapeutical observation of late years, primarily through the labours of Sir John Forbes, who instituted careful inquiries into the alleged facts of both homoeopathy and mesmerism. A careful sifting of the latter class seems to me to lead to the conclusion, that a method of great practical and scientific importance, for artificially exciting changes in the brain, and through these on organs and tissues, has been added to science; and that the true facts serve to throw much light on the fundamental laws of life and mind, albeit not in the direction which the deductions therfrom have taken. The chief cerebral phenomena resulting from the changes thus induced by the processes of mesmerism, hypnotism, and electro-biology, are anesthesia, sleep, dreaming, somnambulism, exaltation of the faculties, hallucinations, delirium, and insanity, most commonly temporary only; some of these are physiological, some pathological, all toxicological; to differentiate them when produced artificially, we might term them dynamical. The changes induced in bodily organs and tissues have also been long known as the results of imagination. The operators and experimenters in this department, for the most part, wholly ignored the facts of cerebral pathology and toxicology. Some had little or no knowledge of the functions of the brain and the physiology of consciousness; others misapplied, or neglected to apply, the knowledge they undoubtedly possessed. Preferring to assume the existence of a new force in Nature, they examined the phenomena manifested in relation to the hypotheses they formed as to its laws of action exclusively, and wholly neglected their relations to those already well known and attributed to certain laws of life and consciousness. In this way, by rejecting the facts and conclusions of modern research, they caused the rejection of their own by modern inquirers. The most favourable example of the method are the researches into *odyle* or *od* or *odylic* force by Reichenbach, which were enthusiastically taken up by my late friend Professor Gregory.* Their labours have had, or are likely to have, the same advantageous influence on neurology and psychology that formerly alchemy and the Hermetic system of philosophy exercised on chemistry and the biological sciences.

The phenomena of spiritualism may be taken in connection with those of mesmerism and its offshoots; but bearing more directly upon the great problems of life and mind, and more particularly on those relations of knowledge and the will to organisation which bring medical science not only into the courts of law, but into the very core of all systems of education, morals, and philosophy; so that, whether we consider the physiological and pathological bearings of these speculative schools on cerebral physiology, pathology, and therapeutics, or the number of those who follow them, amongst whom are men of high attainments, the facts they advance are worthy of special examination. It must not be forgotten that these sects have a wide-spread circle outside the profession, and influence not only religion, morals, and philosophy, but also legis-

lation and practice and the business of life. Mesmerism and spiritualism, and indeed all mysticisms, are only the outward signs and manifestations of those yearnings after the spiritual and the infinite which are the great characteristics of humanity in its higher development, and which at this moment involve the interests of millions in Europe and America. If the profession, then, mean to be the guides scientifically of these new movements, as it ought to be, the phenomena in question must have a place in the proposed new chapter. I shall, therefore, give them special consideration. The vital forces, and Nature with its *vis medicatrix*—the more special "forces" of the current schools of medicine, so constantly referred to by practitioners—must, however, be examined cursorily in their relations to other forces and systems, so as to clear up our notions of what we understand by them, and prepare the way not only for more definite generalisations as to these fundamental points of theory and practice, but for a right estimate of the facts and theories just referred to which are outside the pale.

CASE OF INTUSSUSCEPTION OF THE RECTUM.

By ROBERT VACY ASH, Esq., Grantham.

I WAS some time ago called to the following singular case.

Catherine T., aged about 6 years, was first taken ill with extreme straining when at stool. Almost in the act of defecating, the motion ceased to come; still the straining continued, with the loss of a considerable quantity of clear viscid mucus. After this had continued for two days, I was called in. On examining the rectum, in one of her straining fits, the external sphincter was found dilated to the size of a shilling; and, at the depth of about three inches, was seen a large round fleshy looking tumour. At each effort of straining, this came down and pressed on the sphincters. On passing my finger up and round this mass, it appeared to be about the size of an egg, round, hard, and fleshy to the feel, with a constricted neck, not unlike an inverted uterus. I pushed it up as far as the length of my finger would admit; but it returned on the sphincters immediately on the first effort of straining. On inquiry, I found the child had been subject "to her body coming down" since birth; so I naturally concluded it was a case of prolapsus recti of an extreme form. I ordered four minims of tincture of opium and four grains of nitrate of polish in camphor mixture every four hours, with a suppository of opium and acetate of lead at bedtime. The opium internally tended to stop the excessive tenesmus; but the suppositories would not stay up.

On the following day, I again introduced my finger and tried to slip the intussuscepted part through the neck, but failed. The sphincters were more relaxed; the mucous discharge continued. The pulse was 130, weak. There was tenderness over the left iliac fossa. I ordered hot poppy-head fomentations.

She continued in this state for some days; when one morning I discovered a greenish looking spot on the tumour. This increased in size and depth, and the surface of the mass looked of a deeper congested colour. The mucous discharge was more in quantity, and streaked with blood. She complained of extreme tenderness over the abdomen. The pulse was wiry, and 130 per minute. I ordered plenty of nourishing food, with calomel and opium at bedtime, and cold brandy and water to relieve the exhaustion and vomiting.

On the following day, I called another medical man in. After examining her, he agreed with me as to the intussusception. I then proposed an incision into the mass, thinking the hardness was caused by feces, and that, by opening up a passage, relief might be obtained. He consented, and I did so; nothing but a little blood came away. I then proposed the formation of an artificial anus. To this the friends would not agree; so we left the child suffering as much as ever.

A few days after this, I was fettered in a great hurry, and found that the child had succeeded, in one of her straining efforts, in expelling the tumour outside the anus. On carefully examining it, it looked like a lump of flesh covered with mucous membrane in a state of ulceration and congestion. After oiling it well, I replaced it, but to no purpose; it immediately came outside again. I then tried a plug bandaged tightly round the waist and between the thighs; but even this was forced on one side by the expulsive efforts of the child to get rid of the tumour. All at once the patient became easy; but soon collapse, with marked symptoms of peritonitis, came on, and the child quickly sank, never having passed a motion since the commencement of the illness.

AUTOPSY, twenty-four hours after death. On cutting into the peritoneal sac, a quantity of clay-coloured feces mixed up with the intestines came into view. The stench was so great, that a minute examination was impossible. I succeeded, however, in finding the ruptured piece of gut, situated just above the sigmoid flexure. The intestines

* See "Researches on Magnetism, Electricity, Heat, Light, Crystallisation, and Chemical Affinity, in their Relations to the Vital Force." By Baron von Reichenbach. Translated by Wm. Gregory, M.D., etc., Professor of Chemistry in the University of Edinburgh. London: 1850.