was in a state of the greatest danger, and the second, in seven hours, was past hope; whereas eleven hours had elapsed in his case before a dangerous collapse had set in. The treatment which I adopted in the successful case was that which I have long had reason to depend on, as being the most likely to ensure a recovery, provided the stage of collapse has not set in, or only in a slight degree; it was a practice first brought before the notice of the profession by Dr. Graves, of Dublin, at the time this epidemic raged in that city in the summer of 1834, at which period I was but a student, and therefore could not speak of it from my own experience; but in the winter of 1838, when in charge of a dispensary in the south of Ireland, fifteen or twenty cases of cholera broke out in the village where I resided. There was no doubt of the identity of the disease as it occurs in India, for it was at the same time raging in Carrick-on-Suir, within fifteen miles of the village; and I have reason to believe its appearance in my district was traceable to the fact of all the persons thus attacked having attended the funeral of a person who died in the above-mentioned town. All these cases I attended within from two to four hours of their being taken ill, and before collapse (at least to any serious extent) had set in; and I attribute the recovery in all these cases to the use of the acetate of lead with opium, of course accompanied with the administration of diffusible stimulants. Subsequent to this I met and treated this disease in its own country, and in every instance with success where I saw the patient early; and in most cases I had this advantage, as my practice was among sailors on shipboard, who were obliged, immediately on their being taken ill, to report themselves, and thus were immediately attended to. I must, however, add, that in many of these successful cases I had abstracted blood previously. This course I always pursued where the patient was plethoric, and only two or three hours attacked. The success which attended this practice always induced me to give this remedy the preference in such cases as I have mentioned its being applicable to. In the fatal case I have recorded, I did not employ it for the reasons alleged, but gave calomel in its place, with the hope of restoring the bile to the bowels.

I fully concur in the judicious line of treatment recommended by Dr. Basham, especially the hot air bath, but I cannot give my approval to the hot water bath, which is so much oftener employed in India. I have known patients die immediately on their being taken out of it, and I cannot say I have ever seen any benefit from its use; I believe it, on the contrary, to be most injurious, especially if the patient has taken much opium previously, which is generally the case. I remember witnessing the most painful oppression of the breathing, and the most intense blueness of the whole body, come on in the case of one of our sailors, (who, having been some hours under treatment on board, was sent to the General Hospital, Calcutta,) from immersion in the hot bath. How far this may have been the cause of the congestion I do not attempt to argue, but that immersion in hot water must tend to increase the congestion from which persons under the influence of large doses of opium suffer, I think admits of no doubt. Were I, therefore, about to use this agent in such a case, I would open a vein or two, if required, and thus relieve the circulation, already over-burthened.

As to the causes which have given rise to the disease at present, I fully agree in the opinion of Dr. Basham, that atmospheric changes are of themselves capable of producing it in this or any country, where it may not even be epidemic; that the same state of the atmosphere which produced the disease in London, was its exciting cause here, I have no doubt; the weather here, for some time previous, had been intensely, and, for this country, unnaturally hot, and then suddenly changed to extreme cold with rain. This change was accompanied, or rather fore-run, by a severe thunder-storm, and the very next day my second case occurred. How far an electric state of the atmosphere can have influenced or developed this disease, I confess my inability to answer, but a remarkable circumstance strikes me just at this moment—that when this epidemic raged in Ireland, its outbreak in the town of Sligo, where it raged with unparalleled severity, was preceded by a terrific storm of thunder and lightning: I have also a faint recollection that the same occurrence happened when it appeared in the village of Rathfarnham, or some other of the outskirts of Dublin, but the exact place I do not now remember. Whether this was only a remarkable coincidence, or the storm was really the agent which produced the disease, I am not prepared to assert. I lately read an interesting communication in *THE LANCET* of June 13th, from Dr. Walsh, describing a peculiar disease he met with in Chusan. Though not cholera, it certainly presents some of its characters, a defi-

ciency of bile being the most marked. In trying to account for the agencies which produced this disease, he makes some very useful remarks on an electric state of the atmosphere, the different effects which it produces on the body; and he relates some interesting peculiarities of disease, difference of treatment and constitution, dependent on electricity being either positive or negative. How far can this theory be applicable to our present subject? I confess I have not either ability or leisure to enter upon the discussion of such a subject; it presents a wide and instructive field of investigation, which I think might advantageously occupy the pen of able philosophers. I have no doubt in my mind, however, that we may certainly account for the appearance of the disease we are now considering, by attributing it to the sudden atmospheric change from excessive heat to an opposite state, and this at the time that the body was enervated and incapable of resisting the depressing influence which cold exerts.

Buncrana, Donegal, July, 1846.

ON THE CONTAGION OF HOOPING-COUGH,

WITH REMARKS ON THE TREATMENT OF THAT AFFECTION.

By JOHN JAMES M'GREGOR, M.D.,

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Few subjects in the wide range of medical science command more interest than epidemical and contagious influence, and few diseases in the nosological category have excited more curious inquiry than whooping-cough; nor is this to be wondered at, for it is, unlike any other lesion as yet recognised by nosologists,—forming as it does a kind of link between the pyrexia and the neuroses,—a compound disease, so to speak, presenting mixed yet distinct phenomena; even in the same stage requiring a peculiar modification of treatment, upon which, there can be little doubt, in a great measure depends its successful termination. In the present state of our knowledge of “essences” and fever, it would be labour in vain to strive to penetrate the veil which conceals from our perception the remote cause of pertussis, the peculiar, subtle poison, which, obeying certain laws, inappreciable to sense, and pervading at certain times peculiar localities, is as unknown to us as the miasm of cholera, typhus fever, or any other epidemic disease; and in this respect, it must be allowed, even by the most enthusiastic disciples in chemistry, that their favourite science has effected little in solving the difficulty, all our knowledge on the subject being purely hypothetical. Still, although a knowledge of its essential cause be beyond our capacity, some of the phenomena are not so impalpable, and a correct observation of these, with their presiding laws, must ever be useful to the practical physician.

That whooping-cough is produced by epidemic influence is believed by all medical men; and that it may also be imparted by contagion is as generally acknowledged; yet, when both doctrines can be proved clearly, by well-authenticated cases in practice, it appears to me desirable they should be noticed. The cases I am about to adduce also illustrate another interesting fact,—that after a certain time the contagious influence ceases to operate, and although the disease may continue in all its virulence, and even prove fatal, all danger of contagion, after the phase I allude to, is over. To determine what that exact period is, would, if it is possible to do so by observation, be of great importance, as it could be then distinctly laid down and acted upon.

E. and A. P——, two young ladies, sisters, the former aged ten years, the latter seven, took whooping-cough in London, in January, 1846, during the first stage of which they were sent to school in the suburbs. Two other young ladies, R. M—— and A. G——, returned to the same school at the same time, perfectly free from cough, but in about ten days exhibited the usual symptoms, which terminated in the second stage, the peculiar phenomena of which serve to designate the disease. At this period, every child in the school, who previously had it not, took it, clearly proving that contagious, and not epidemic influence, was in operation. At the end of six weeks from the beginning of the first stage, and a month after the hoop commenced, M. M——, aged seven years, who never had had the disease, with two other children similarly circumstanced, came to school, but not one of them took it, the poison being exhausted, thus proving the very limited nature of its existence.

It would appear that, on an average, about two months form the limit of its contagious properties, after which there is little or no danger.

Although my chief object, in making these few observations, had chiefly reference to the etiology of whooping-cough, still, I trust, a word about the treatment of this fatal disease will not prove irrelevant. Physicians of all countries agree that the first stage should be treated mildly, with antiphlogistics, daily emetics of ipecacuanha, &c.

There is one point, however, upon which authors have not, in my opinion, sufficiently dwelt, and which, I feel convinced from observation, is the most important of all others,—a strict confinement to the house, and, if possible, to one room, except during the summer months.

In almost every case where this plan has been strictly enforced, in my experience, the result has been favourable, and the duration of the disease comparatively brief. Change of air, or the open air, is only to be recommended in the second stage; and even here, with much caution, a violent exacerbation of all the symptoms being a frequent result of such practice.

It is, as might be expected, in the second stage, or "stadium convulsivum" of Dr. Aberle, that the greatest difference of treatment exists; and it is not without some diffidence I venture to offer my opinion on so difficult a subject. In a paper published by me in the May number of the *Dublin Medical Journal* for 1839, I have advocated a mild course of mercury where age and other circumstances did not prevent it—an opinion I have had no reason since to change. I must also bear my testimony to the value of the treatment recommended by the late Dr. Beatty, as well as his son, Dr. Thomas Beatty, of Dublin, which, in several recent cases, I decidedly found to cut short the convulsive paroxysm, in some instances in a week or ten days.

The formula recommended by Dr. Groves in his work on "Clinical Medicine," was that I employed; it is this:—Tincture of cantharides; compound tincture of opium; of each, half an ounce; compound tincture of cinchona, five ounces and a half. A teaspoonful to be taken three times a day in a little boiling water, and if no strangury is produced, the dose is to be increased.

The modus operandi of the mixture is too obvious to require comment.

Wilmington-square, London, June, 1846.

ON THE

ANATOMY OF THE EXCITO-MOTOR SYSTEM.

By MARSHALL HALL, M.D., F.R.S. L. & E., &c.

THE history of the protracted disputes on this topic would be full of instruction, but it is not my present intention to write it. My object is, to lay before the readers of *THE LANCET*, in a few words, (I am always afraid of occupying their time and my own needlessly,) the argument, or rather, the plain and simple proof, of the distinct anatomy of the excito-motor system.

Does any one doubt the distinct anatomy of the system of cerebral nerves—of the nerves of sensation and volition?

The *very same* proof which exists of this part of the nervous system, exists in regard to the excito-motor system. It exists in the pneumogastric nerve, or, as it may be better designated, the pneumogastric system of nerves.

If the pneumogastric be sentient at all, it is the least sentient of all incident nerves. What is it then? It is excito-motor! It is, emphatically, the internal, excito-motor nerve!

Why go to complex structures, when a simple one exists? Why go to the lower orders of animals, when the mammalia, and even the human being, afford us the proof we require?

The superior and the inferior laryngeals are the associated excitor and motor nerves of the larynx.

The bronchials are associated excitor and motor nerves of the bronchia.

The pharyngeals and œsophageals are the associated excitor and motor nerves of the pharynx, of the œsophagus, and of the cardia.

Lastly, and most strikingly, the pulmonic part of the pneumogastric nerve is, as the associate of the diaphragmatic and intercostals, the internal excitor of respiration.

There is, in short, as I have said, the *same* proof of the distinctness of the excito-motor system of nerves, as of the sentient and voluntary, and it is both idle and ridiculous to dispute the fact any longer, or to appeal to other parts of the nervous system than the grand pneumogastric, or to other tribes of animals than the mammalia, for proofs not needed. As confirmations of a truth already established, these researches are, of course, interesting enough. I am myself preparing a paper on the pneumogastric system in animals of limited and of diffused respiration—in the mammalia; and in birds and insects. In birds, the spinal nerves are, doubtless,

in their distribution to the diffused breathing cells, analogous to the pulmonic branches of the pneumogastric. In insects, *each segment* with its spiracles, (analogues of the larynx, trachea, and bronchia,) is endowed with a nervous system entirely analogous to the laryngeals, and to the pulmonic branches of the pneumogastric, and the diaphragmatic or intercostals! Then we have to inquire into the nature and office of the lateral nerve in fishes. As in birds, the respiratory nerves are, probably, equally for flight and for respiration; so in fishes, the lateral nerve is, probably, for swimming and for respiration.

But to return to my topic. The proof of the distinct anatomy of the excito-motor system, is afforded by the pneumogastric—the internal, purely, or almost purely, excito-motor nerve.

If, however, we would examine other and more complicated tissues, the proof lies, not, I fear, in the dissection and tracing of fibres, but in physiological experiment: the cerebral system is, so tested, *in-excitor* throughout—in its centre, in the nerves of special sense; the excito-motor system is, in its centre, and in its incident and reflex relations, what its designation implies.

It is pitiable that there should any longer be any dispute on the subject, or that detraction should still attempt to wrest the credit of adducing the proof, in any degree, from myself, or from physiology.

Amongst other attempts of this kind, one has been to propose a change in the designation which I had given to the nerves of the reflex arc—and a most unfortunate change too. The terms incident and reflex *imply* some very definite association, or *law-relation*, between the two—a real phenomenon of the most remarkable kind. But the terms *afferent* and *efferent* are, in this respect, utterly insignificant; whilst the meaning which these words do convey, of something borne to and from, is probably altogether erroneous.

The ray of light, which is now incident and immediately afterwards reflected, is the *same* ray, modified, directed, and returned by the reflector, whether it consist in loco-moving particles, or in vibration. The same idea is attempted to be conveyed by the terms incident and reflex nerve. There is, in these nerves, and in their connexion through the spinal marrow, some extraordinary recondite connexion, so that, for example, the excitation of the superior laryngeal sends forth some mysterious messenger to the medulla oblongata, whilst this returns it in the just channel, the inferior laryngeal, so as to effect the closure of the larynx; whilst the excitation of the pulmonic branches of the pneumogastric excites, through the diaphragmatic and intercostal nerves, the contraction of the muscles of inspiration, precisely, definitely, and no other.

The ordinary reflexion of a ray of light, or the polarization of a ray of light, is not more definite.

The effect produced is obviously *designed*, not by the animal, —for its brain may be removed without interfering with this process,—but by an omniprovident Creator. This obvious design has misled many to think that there are feeling and volition in the spinal marrow.

The terms incident and reflex are therefore full of meaning; whilst the terms afferent and efferent either convey no meaning at all, or an erroneous one. In this suggestion, the *Law of association* of the effects of excitement, its incident course, its modification and direction by the spinal marrow, its reflex course and destination, were unperceived.

How much, then, is conveyed or implied in that one word, *REFLEX*,—incidence, reflexion, appropriate combination, and destination! And how devoid of all meaning are the words afferent and efferent, not very modestly attempted to be substituted for it!

I beg my reader to study and compare the physiological movements in the acts of inspiration, with their pathological forms in asphyxia: the first are reflex, normal, and beautifully appropriate; the second are, in every respect, abnormal and deranged.

My opponents are much disposed to speak of the class of reflex actions, in general terms, as known to Redi, Whytt, &c. This is another ill-chosen but deceptive phrase. The *reflex actions*, as I have *always* said, were spoken of by many previous physiologists; but the phrase I have adopted from the *very* beginning,—for the very title of my first paper,—was *reflex function*; and this expression, with its fulness of meaning, as applied to all the acts of ingestion and of egestion in the animal economy, had been used, could have been used, by no one; for as the idea of an incident excitor nerve, with its physiological relations, did not exist in anatomy, so the idea of a reflex function, with its anatomical relations, did *not* exist in physiology.

Manchester-square, July, 1846.