SIR CHARLES BELL AND M. MAGENDIE

ON THE FUNCTIONS OF THE SPINAL NERVES.

BY
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DURING the meeting of the British Medical Association at Oxford in August last, a paper was read by Dr. Robert McDonnell, an abstract of which appeared in the medical journals (see *BRITISH MEDICAL JOURNAL*, August 8th and 15th, and September 19th, 1868; and *Lancet*, Aug. 8th, etc.), in which the author said that, "after carefully analysing Bell's writings, he had come to the conclusion that, previously to 1822 (when Magendie made his experiments, and published the results), Bell's written works contained no evidence that he conceived the idea that the posterior roots (*i.e.*, of the spinal nerves) were sensitive, and the anterior motor. He (Dr. McDonnell) assigned to Magendie, and to experimental physiology, the credit of having discovered this fundamental fact in physiology."

Having assisted my teachers and friends, Sir C. Bell and Mr. John Shaw, in many of their experiments on the spinal and facial nerves, and having seen and revised many of the papers of the latter (in some of which my name is mentioned) before their publication, my personal regard for these gentlemen induced me, as soon as I saw this statement, to write the following letter to Dr. McDonnell.

26, Grosvenor Street, London, August 15th, 1868.

Dear Sir,—I do not know whether the report of your paper relative to Sir Charles Bell's and Magendie's claims to the discovery of the functions of the spinal nerves, contained in the *Lancet* and *BRITISH MEDICAL JOURNAL* of last week, is correct and complete; but, if it be so, I will venture, from personal knowledge, to say, that I do not think that you have done that justice to Bell's share in these discoveries which you desire, when you assign the whole merit to Magendie.

It is not sufficient to look at Bell's opinions in 1811, when his original essay was printed; but close attention must also be paid to the result of his experiments and investigations when he again took up the subject, and especially in 1821, when I myself assisted him, and his brother-in-law Mr. John Shaw, in almost all that they were then engaged in relative to the functions of the nerves.

It is not even sufficient to study, by themselves, Sir Charles Bell's own papers in the *Philosophical Transactions* of 1821 and 1822; but it is equally, if not more, necessary to look for an exposition of Bell's opinions in a rapid succession of papers by John Shaw, in his *Manual of Anatomy* published in September 1821; in his papers in January and April 1822 in the *Journal of Science and Art*—all published before Magendie's experiments; in his paper in the twelfth volume of the *Medico-Chirurgical Transactions*, read in April 1822, before Magendie's experiments, but perhaps not published quite so soon, as one of the papers contained in the volume was not read at the Society till June 11th; and in his papers in October and December 1822, in the *Medical and Physical Journal*, just after Magendie's first brief account of his experiments published in the July number of his *Journal de Physiologie*, t. xi, p. 279.

A considerable part of Bell's and Shaw's papers was published in Magendie's *Quarterly Journal*, before Magendie's experiments were published; and Mr. John Shaw was in Paris in 1821, and there performed Bell's experiments on the fifth and seventh nerves for Magendie's satisfaction, who gives his own account of them, and of his conversations with Shaw on the subject, not only of these nerves themselves, but of their relations with the whole system of nerves, including the spinal nerves, and the great fact, so much insisted on by Bell, that different endowments are joined together by different nerves being included in one sheath.

In July or August 1822, appeared Magendie's first paper, in which he repeats Bell's previously expressed opinion, that the anterior and posterior roots of the spinal nerves have different functions; and adds, "les postérieures paraissent plus particulièrement destinées à la sensibilité, tandis que les antérieures semblent plus spécialement liées avec le mouvement." He was too cautious a physiologist to express himself with perfect confidence as to this division of their functions.

In the next number of his *Journal*, October 1822, Magendie says he supposed he had been the first to think of cutting the roots of the spinal nerves; but that he had been undeceived by a paper (that of the pre-

ceding April, read at the Medico-Chirurgical Society, vol. xii, p. 146, *et seq.*), which Mr. Shaw had sent him, and also a copy of Bell's original essay; but he still claimed to have established, in a more positive manner, the motor and sentient qualities of the two roots. If, however, like yourself, he found Bell's own expressions, which he quotes, ambiguous, so that Bell was only "bien près de découvrir les fonctions des racines spinales", he might have gathered from Shaw's paper, that Bell had clearly ascertained that the anterior roots conveyed the power of motion to the muscles; although it is doubtless true, as Shaw acknowledges in this, the latest paper before Magendie performed his experiments, that the functions of the posterior roots were still uncertain, in Bell's opinion; "from the violence necessarily used, it has been difficult to ascertain which of the filaments bestows sensibility on the part." (P. 148.)

One experiment was performed by Sir Charles Bell and Shaw, in which I assisted, in March 1821, at least fifteen months before Magendie's paper was printed; and I naturally prefer my own account of the result, written for Mr. Shaw, which was contained in his paper of October 1822, in the *Medical and Physical Journal*, vol. xlviii, p. 346.

"The spinal marrow was exposed immediately after death, while some of the muscles still had a slight convulsive motion. Upon irritating the posterior root of the spinal nerves in three or four places, no effect was produced on the neighbouring muscles; but when the anterior root singly, or the whole nerve, was pinched by the forceps, or pricked by the scissors, an evident motion was produced in the muscles, not only perceptible to the eye, but, when the third or fourth nerve was touched, the whole scapula was moved in the hands of an assistant. The motion was not communicated to the muscles, when the ganglion, which is formed on the posterior root within the sheath, was touched; neither did it follow on injury of the posterior column of the spinal marrow. Whether an injury of the anterior column of the spinal marrow would produce convulsions was not ascertained, as it was not cut upon till the irritability of the muscles appeared to be exhausted. The motion given to the muscles was not the slight tremulous motion arising from the natural irritability still remaining; but it was convulsive and spasmodic, and followed each successive prick of the scissors."

In 1856, as President of the Royal Medical and Chirurgical Society, it was my duty to notice Magendie's death within the preceding year; and I beg to refer you to my address, which was published in the *Lancet* of March 15th, 1856, p. 282, for a brief but impartial account of the respective portions of credit due to each of these distinguished physiologists. In this address, I shewed the reasons why Bell, having distinctly and clearly localised the source of motor power, had failed to ascertain with certainty the seat of sensibility; especially his theory regarding the dependence of both sensation and motion on the cerebrum, and anterior columns of the spinal marrow, and anterior roots of the spinal nerves, as connected with animal life; and the supposed influence of the cerebellum, posterior columns of the spinal marrow, and the ganglionic roots of the spinal nerves on the organic life of animals. Bell's renewed investigations about 1821 led to the further distinction in his mind of the relation of the lateral tracts of the spinal column, and the irregular nerves, to the function of respiration, which views, unfortunately, led to his earliest mistakes as to the functions of the fifth and seventh nerves of the face.

Let me, also, point out to your notice a very elaborate history of the whole subject, by a reviewer, in the ninth volume of the *British and Foreign Medical Review* for 1840; which, according to my recollection—I read the account many years ago—is, on the whole, very fairly and correctly drawn up.

I trust that the interest which you have shewn on this subject, and the importance of the truth being clearly known, will lead you to excuse the unexpected length of this letter.

I am, dear sir, very truly yours,
To Dr. Robert McDonnell, F.R.S. CÆSAR H. HAWKINS.

Having been favoured with several communications from Dr. McDonnell, I am happy to bear testimony to the great fairness and candour and gentlemanly spirit which he has shown in all of them. In a letter in the *BRITISH MEDICAL JOURNAL* of September 19th, he acknowledged that he may "have given too much weight to the testimony offered by Sir C. Bell's pamphlet of 1811," and informed the public that Bell's essay would be republished in the November number of the *Journal of Anatomy and Physiology*. This has now been done; and it has been accompanied by some very interesting extracts from Bell's early letters relating to his thoughts for many years on the subject of the nervous system, and also by comments by my friend Mr. Alexander Shaw, which, in relation to the spinal nerves, correspond in the main with my own knowledge of the subject, and in a higher degree than his letter of August 15th.

Dr. McDonnell has recently informed me that he does not now intend to publish his paper which he read at Oxford. He has been led to this determination, partly because Bell's pamphlet of 1811 is again placed before the public in such a shape that physiologists are enabled to form their own conclusions, and partly from the conviction that in his paper he did not do full justice to the share which Mr. John Shaw had in this discovery. Dr. McDonnell has, therefore, given up the intention, which he at first entertained, of publishing his own paper, together with Bell's essay and my letter to himself, and with references to various other papers relating to this subject; and he is kind enough to express an opinion that, a renewed interest being felt on this question, my letter to him, or any other statement from me, would, on account of my personal knowledge, be very acceptable and important.

I have been at all times reluctant to enter into controversy on this or other subjects; but I must confess that, when a physiologist like M. Bernard asserts that "to attribute the whole discovery of the functions of the spinal nerves to Ch. Bell is the height of injustice," he is, in my opinion, equally wrong when he says "that the honour of the demonstration of the functions of the spinal nerves belongs entirely to Magendie" (*Rapport sur les Progrès et la Marche de la Physiologie en France*, 1867, pp. 14-156). It is not improbable that M. Bernard's opinion might have been modified, if he had been better acquainted with Mr. John Shaw's writings, previously mentioned, as exponents of his relative's investigations; nor could he have implied a half-doubt whether the experiments described by Charles Bell in his essay had ever been performed (p. 156), if he had known that a description of one of them, written by myself, an eye-witness of the fact, in March 1821, had been in Mr. Shaw's possession fifteen months before Magendie's experiments were performed, although not in print till two months after Magendie's first paper on the spinal nerves was published.

To complete my history of this matter, I will venture to add a portion of what I addressed to the Royal Medical and Chirurgical Society regarding Magendie's experiments on the spinal nerves, without entering into the equally misunderstood subject of the facial nerves, which I also briefly discussed on that occasion (*Lancet*, March 15th, 1856).

"For how much, then, are we indebted to Magendie for what we know of the functions of the spinal cord and nerves? First, and above all, it is unquestionably (as between these parties) to Sir Charles Bell that the palm of originality must be assigned. It was by him that the true method of investigation was commenced; and it is not, therefore, without some justice, that the completion of the investigation is also generally called by his name. That nerves possessed different endowments, according to their several sources in the central organs, and their different roots;*—that one set were regularly double-rooted and symmetrical, and another set as constantly single-rooted and irregular, and super-added to the former;—that each of the double-rooted nerves was connected with both great divisions of the brain; with the cerebrum through its crura, the anterior columns of the spinal marrow, and anterior roots of the spinal and fifth nerves; and with the cerebellum through the crura cerebelli, posterior columns of the spinal marrow, and posterior or ganglionic roots of the spinal and fifth nerves;—that with the cerebrum and its extensions were connected the nerves of motion going outwards, and the nerves of touch and sensibility going inwards from the body or viscera;†—and that by the cerebellum and its posterior prolongations and nerves were regulated the secret operations of the bodily frame, and the connexions which unite the parts of the body into a system;‡—all this formed a grand and comprehensive theory of the most attractive kind, which was tested by experiment before 1811,§ was then nearly dormant for ten years, and revived with renewed impetus when I was myself a pupil.

"That the two roots of the spinal nerves possessed different properties, was soon ascertained by these experiments; and also that motion depended on the anterior roots; and that convulsions ensued from irritation of these roots, and not of the posterior. But the imperfect knowledge of the curious effects of reflex action, as now understood, prevented the recognition of the true significance of the facts we witnessed, and made the source of sensation obscure and doubtful, and at

first appeared to confirm the ingenious though erroneous theory of Sir Charles Bell.

"Before these doubts were solved, Magendie performed his experiments on the roots of the spinal nerves, and published the results in August of the following year (1822), and thereby clearly established that the posterior roots of the spinal nerves and posterior columns of the spinal marrow were much more connected with sensation than the anterior (*Journal de Physiologie*, t. xi, p. 279, etc.) This fact he subsequently corroborated by means of galvanism and strychnine, though he never ventured to assert, as some have said for him, that either motion or sensation belonged to either root exclusively. It is by other physiologists since his time that this has been asserted.*

"Such, then, was the partial correction of Sir Charles Bell's experiments by Magendie, who was led to their performance, doubtless, by knowing the experiments on the facial nerves already begun in this country. But there is not, as it seems to me, the least ground for the imputation of unfairness, or of his having been acquainted with Sir Charles's experiments on the spinal nerves through either private or published information. So far from this, Mr. John Shaw himself says in October 1822, two months afterwards, 'In the last number of the *Journal de Physiologie*, several very curious experiments are related by M. Magendie, which are not only important in themselves, but are interesting, as they corroborate some experiments which had been previously made in this country, but of the performance of which M. Magendie does not appear to have been aware.' (*London Medical and Physical Journal*, vol. xlviii, p. 343, October 1822, after Magendie's first paper). He then gives a translation of Magendie's paper, and adds: 'The importance of the facts discovered by these experiments must be evident to every one; and it must be gratifying to the true friends of science in this country to find that M. Magendie, whose sole object in these pursuits appears to be the promotion of physiology, has by his experiments come to the same conclusions as those which had been previously deduced by Sir Charles Bell from observations made on the brain and spinal marrow. The truth of these deductions was also by him put to the test of experiments, the results of which, though they correspond with those of M. Magendie, were not so conclusive.' (*Ibid.*, p. 344.)

"Finally, of our experiments in Great Windmill Street, he says: 'It has been difficult to ascertain which of the filaments bestows sensibility on the part. It was easily shewn that if only the posterior set was destroyed, the voluntary power over the muscles continued unimpaired; but the pain necessarily attendant upon the performance of the experiments prevented us from judging of the degree of sensibility remaining in the part.' (*Medico-Chirurgical Transactions*, vol. xii, p. 148, read April 1822, before Magendie's first paper.) I then mentioned, among the reasons for Bell's not so conclusive a result, his great dislike to inflict pain, leading him to expose three or four spinal nerves only, while Magendie exposed the whole spine to his observation.

It is evident from these expressions, that Magendie is altogether acquitted by Mr. John Shaw of plagiarism, and that in his visit to Paris in the previous year, 1821, although Mr. Shaw had made him fully acquainted with Bell's general views, especially with regard to the facial nerves, then misunderstood by all, he could not expressly have described Bell's experiments on the spinal nerves. He acknowledges the superior precision and importance of Magendie's experiments, and uses the same term after Magendie's paper, as he had done shortly before them, and as Bell had himself done in 1811; viz., that the experiments in this country were not so conclusive.

This paper, written in October 1822, while the whole of the circumstances were recent and clear in his mind, demonstrates the candour and truthfulness of my excellent teacher, in not claiming for Bell and himself greater credit than they were entitled to. It is important also to notice that John Shaw's tribute to the value of Magendie's recent paper on the Spinal Nerves, was written after the obscurity regarding the distribution and double office of the fifth nerve had been cleared up by Mayo (*Mayo's Commentaries*, vol. i, p. 107, August 1822) and by J. Shaw himself (*Medical and Physical Journal*, vol. xlviii, p. 342, Oct. 1822), and the errors thus corrected into which Sir C. Bell had previously fallen; by which corrections, a far clearer view of the whole of Bell's class of symmetrical nerves had been obtained. He makes no claim to the discovery, positively, of sensation residing in the posterior roots of the spinal nerves, at the very time when he would naturally have done so, if he desired it, nor am I aware of such a claim in precise terms by any one on behalf of Bell till 1824.

P.S.—December 22nd. A friend has this day sent me the October number of the *New York Journal of Psychological Medicine and Juris-*

* In Bell's own words: "The nerves of sense, the nerves of motion, and the vital nerves, are distinct through their whole course, though they seem sometimes united in one bundle; and they depend for their attributes on the organs of the brain, to which they are severally attached."—Bell's Essay, in *Journal of Anatomy and Physiology*, November 1808, p. 154.

† Bell's own expression is: "The cerebrum I consider as the grand organ by which the mind is united to the body. Into it all the nerves from the external organs of the senses enter, and from it all the nerves which are agents of the will pass out."—*Ibid.*, p. 163.

‡ "The nerves proceeding from the crus cerebelli go everywhere (in seeming union with those from the crus cerebri); they unite the body together, and control the actions of the bodily frame, and especially govern the operations of the viscera necessary to the continuance of life."—*Ibid.*, p. 162.

§ "Though they were not conclusive."—*Ibid.*, p. 160.

* I have already quoted Magendie's own words, in his paper of August 1822 (see page 21); and in October, he also says: "Le sentiment n'est pas exclusivement dans les racines postérieures, non plus que le mouvement dans les antérieures."—*Ibid.*, p. 368.

prudence, containing a paper by Professor Austin Flint, entitled "Historical Considerations concerning the Properties of the Roots of the Spinal Nerves", which is drawn up with great care, and with ample references to English and continental writers; and it so entirely coincides with what I have written from my own knowledge, that, if it had appeared earlier in this country, I should have taken no part in the matter.

SCARLET FEVER, AND ITS PREVENTION.

By W. BUDD, M.D. Ed.,

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MEMBERS of the profession who may happen to have read a letter by Mr. Bradley of Marlborough College, which appeared in the *Times* of December 5th, and in which he sets forth the conflicting responsibilities imposed on the master by the case of boys at school convalescent from scarlet fever, cannot have failed to sympathise deeply with that gentleman in the difficulties of his position. On the one hand, Mr. Bradley shows, in forcible terms, that a long detention in the sick-house is full of evil, moral and physical, to the boy; on the other, he is reminded that to send a scarlet-fever convalescent away through the country is not only to inflict an unwarrantable peril on the community, but to infringe the law. Happily, medical science is in a position to furnish an escape from this very painful dilemma. If, in fact, the patient can be so treated as to cease to be an active source of infection by the time he is able to travel, the difficulty is over. Now, if my own experience can be trusted, nothing is easier. Much more, indeed, can be done to limit the spread of this malignant fever than the public are at all aware of, or than the common practice of medical men generally would seem to indicate.

There is good reason to believe that not only the eruption on the skin, but everything that is shed by the body of the infected, is heavily laden with the germs or seeds by which (alone, no doubt) the disease is propagated. The discharges from the throat and nose are, I imagine, especially virulent. It is more than suspected, on grounds on which I need not here insist, that those from the bowel are scarcely less so. As the kidney is known to be affected in a very special, and often in a very severe way, by the poison, this organ probably furnishes another outlet for it. All analogy tends to indicate, indeed, that in this case the renal epithelium, which is cast off so plentifully, performs the same eliminative function as that which is cast off in still greater profusion by the outer surface of the body. As the bulk of all these excreta soon finds its way to the cesspool or sewer, the large part which sewers and cesspools are known to play in the dissemination of the fever, and which, quite lately even, has been so strangely misinterpreted, is easily understood. I could enlarge much on this topic, if I had time to do so. It must suffice for the present to say, once for all, that all that has been shown to hold of typhoid fever in regard to these relations—contamination of drinking-water included—may be applied, with little qualification, to scarlet fever also.

Taking these things as our data, the one thing to aim at, therefore, in seeking to prevent the spread of this fever, is to annihilate the germs proceeding from these various sources on their very issue from the body, and before the patient leaves the sick-room. In accordance with this view, I have long been in the habit, in all cases which fall under my own care, of enforcing the following simple precautions.

1. The room is dismantled of all needless woollen or other draperies which might possibly serve to harbour the poison.
2. A basin, charged with chloride or carbolate of lime or some other convenient disinfectant, is kept constantly on the bed, for the patient to spit into.
3. A large vessel, containing water impregnated with chlorides or with Condy's fluid, always stands in the room, for the reception of all bed and body linen immediately on its removal from the person of the patient.
4. Pocket-handkerchiefs are proscribed; and small pieces of rag are used instead for wiping the mouth and nose. Each piece, after being once used, is immediately burnt.
5. As the hands of nurses of necessity become frequently soiled by the specific excreta, a good supply of towels, and two basins, one containing water with Condy's fluid or chlorides, and another plain soap and water, are always at hand, for the immediate removal of the taint.
6. All glasses, cups, or other vessels, used by or about the patient, are scrupulously cleaned before being used by others.
7. The discharges from the bowel and kidney are received, *on their very issue from the body*, into vessels charged with disinfectants.

By these measures, the greater part of the germs which are thrown

off by internal surfaces are robbed of their power to propagate the fever. Those which are thrown off by the skin require somewhat different management. If my information do not mislead me, it is in dealing with these that the practice of medical men generally is most defective. There are, no doubt, distinguished exceptions; but, for the most part, either nothing is done, or, what is done, is done imperfectly or too late. And yet to destroy, from the first, as far as possible, the infectious power of what emanates from the skin, is, for obvious reasons, the most important object of all in the way of prevention.

In the first place, as the skin is at once the most extensive surface of the body, and is, *par excellence*, the seat of what, by a very just figure, is called the *eruption*, the crop of new poison which escapes by the skin probably far exceeds in amount that which escapes by the other surfaces. It is impossible to speak in exact figures here. We cannot count these things as we can count peas, or beans, or grains of wheat. But the case of small-pox furnishes us with a standard which cannot far mislead us. And, as we know that, in a case of confluent small-pox, enough new poison is thrown off actually to inoculate with small-pox myriads of others, so there is every reason to believe that the skin-crop in a severe case of scarlet fever is little, if at all, less prolific.

In the next place, as the process of desquamation, by which this crop is finally cast loose, is a very slow one—lasting, for the most part, over many weeks—the infection from this source is much more abiding than that from the internal sources. But what renders it still more so is the all-important fact that the poison which is liberated by the skin is liberated in the dried state. It is well known—and, indeed, the circumstance has been taken advantage of in the practice of inoculation by cow-pox and other poisons—that animal poisons, when dried at a gentle heat, retain their powers for quite indefinite periods of time. But to be dried at a gentle heat—a heat lower, in fact, than that which attended its own generation—is precisely the case of the scarlet-fever poison, as cast off by the skin.

Another danger is created by the minute and impalpable form in which the particles armed with the poison are set free. The skin peels off in part, no doubt, in flakes of palpable size, but in still greater part under the guise of dust, which floats in the air, impalpable, like motes in the sunbeam. Each of these little atoms is, potentially, the scarlet fever. While they adhere to the body, they may be readily disarmed; but, once afloat, they are in great degree beyond our power.

It is to these various circumstances—to the countless profusion of the new seed, if I may so speak, which is generated and sown broadcast by every fresh case—to the length of time during which it hangs about the sick, capable every moment of being transferred, with all its deadly power, to thing or person—to the impalpable minuteness of the organic particles in which this seed is imbedded—and, lastly, to the long retention of their properties, in virtue of being in the dried state—that we must look mainly for the true explanation of the well-known subtleness and tenacity of this particular infection. To the many striking illustrations of this subtlety and tenacity already on record, I could, if there were need, add many of my own, quite as striking, and free from all ambiguity; but it is a waste of time and space to burden the page with what is already conceded, and with what most men must be sufficiently familiar.

These same circumstances are the source of the peculiar embarrassment and perplexity which, in scarlet fever, hangs over the disposal of the convalescent, and the period, so much debated, and at present confessedly undetermined, at which he may be safely restored to society. They are the source of the dilemma, for instance, to which Mr. Bradley gives such painful expression in the letter referred to at the outset of this paper—a dilemma with which, in private life, medical men have so often to contend, but which, in public schools, if we may judge from the columns of the *Times*, is continually recurring.

Many readers, I dare say, remember the pathetic appeal to the profession which appeared in that journal some ten or twelve months ago, from the pen of a distracted father, urgent to know within what time, and by the use of what measures, his son, who, being convalescent from scarlet fever, was pining in the dreary seclusion of the sick-house of one of our great public schools, might be let out of captivity, and restored to his family. Several letters in reply—one or two, especially, bearing the signature of "A Fellow of the Royal College of Physicians"—offered some more or less sensible suggestions; but all, if my memory do not mislead me, united in the humiliating confession, that no definite time could be named at which persons who had gone through this infection could safely mix with others.

According to my own experience, these difficulties and perplexities may be entirely averted by the employment of the simplest precautions. To be successful, these precautions must be put in force early, and must be thoroughly carried out. The first thing to aim at is, to prevent the minute particles, which are the carriers of the poison, from taking wing