

common to find the os uteri altered in cases of procidentia vaginæ. Sir Charles Clarke, upon this subject, makes the following observations:—"No effect in this disease is produced upon the shape of the os uteri, because the cervix of the uterus is hardly at all connected to the rectum, and the cellular membrane between the vagina and rectum is very loose, and readily admits of the vagina projecting." (Observations on those Diseases of Females which are attended by Discharges, Part I., page 145.) On applying my fingers externally I thought I could distinctly feel that the tumour contained a portion of intestine, but upon introducing my finger into the rectum, and carrying it upwards just beyond the sphincter, and forwards, it passed into the pouch, which was filled with fæces. This pouch consisted of two coats, very loosely, if at all, attached and easily moving upon each other; the anterior, or external coat, was formed by the posterior part of the vagina, whilst the internal or posterior coat consisted of the anterior part of the rectum.

A REPLY TO THE STRICTURES OF MR. WILKINSON KING

ON THE

PHYSIOLOGY OF THE SPINAL MARROW.

By TYLER SMITH, M.B.

MR. T. WILKINSON KING, lecturer on pathological anatomy at Guy's Hospital, aided by Mr. Brereton, who, as he himself says, "from the year 1836 to the present moment has lost no opportunity of testing the truth of the reflex doctrine, and during this time has written two papers in which it has been discussed,"—has recently promulgated some peculiar views respecting spinal motor action upon which I propose to make a few remarks.

I shall be as brief as possible, my sole object being to rescue the physiology of the spinal marrow from the attacks of those whose views I firmly believe to be erroneous. That this is not mere assertion on my part I shall endeavour to show, omitting all reference to those points which have no real connection with the subject.

I select the following remarks of Mr. King:—"It is said that the length of the spinal marrow is the source, or centre, or arc, of reflex functions in the adult man, but there seems to me to be the weakest grounds possible for such a supposition, and for the three or four years past, at least, that I have had this view in my mind, I have met with nothing to induce me to think otherwise. The view I entertain is, that with all kinds of actual or efficient division of the spinal marrow there is loss of *all* sense and of *all* motion below, and that in the supposed cases in which reflex signs appeared with the absence of sense and motion, the division of the spinal cord was positively incomplete or (in a very few cases) uncertain."

Mr. Brereton declares as follows:—"My conviction is that it is never developed when sensation is *fully* and *completely* destroyed. I say fully and completely, because it may be very slight, or it may be much modified, and yet reflex actions go on, but when it is so destroyed, as I have presumed, I have never witnessed reflex phenomena."

Mr. King goes on to say:—"In man there is not a single fact to show that the division of the spinal marrow at any point between its lower end and the phrenic nerve-roots has ever been attended with any reflex motion connected with its lower segment. Paraplegia has, in addition, been induced, by injury and by disease, at every point below the origin of the phrenics, and recovery has followed in due course. Partial arrest of sense or of motion, in various forms and grades, are common, and yet with all, among the countless candidates for medical and philosophic fame, it has only been amidst the last-mentioned partial cases that a few *indistinctly* excito-motory facts have been collected."

The two grand dogmas, then, of Mr. Brereton and Mr. King, are,—First, that no reflex motions take place in parts entirely devoid of sensation; secondly, that the spinal marrow has no independent motor power below

the origin of the phrenic nerve. Now these, if true, would strike at the fundamental parts of the physiology of the spinal marrow, as propounded by Dr. Marshall Hall, and they must be examined with reference, first, to *testimony*, secondly, to *anatomical fact*.

With regard to the first dogma, that sensation is necessary to the development of reflex-motor action.

1. Dr. Baly relates a case of hemiplegia which occurred in the St. Pancras Infirmary, where there was "complete loss of *sensation* and motion," in which the reflex actions of the paralysed side were very marked.

2. Dr. Macartney relates a case of paraplegia, in a letter to Sir Benjamin Brodie, in which there was violent erection of the penis on the slightest friction of the glans. "He had *no consciousness* of what was going on unless he put his hand to the part or looked at it."

3. Sir B. Brodie says that "priapism occurs when the sensibility is *entirely destroyed*, and may be induced by the mechanical irritation caused by the introduction of the catheter, when the patient is *entirely unconscious* of the operation."

4. Mr. Mayo remarks that "in some cases of privation of sense and motion in the legs through disease affecting the middle part of the spinal cord, I have seen so much independent power remain in them that pricking or tickling the foot, which yet *excited no sensation*, and was *unknown to the patient*, was nevertheless followed by retraction."

5. Mr. Barlow, a most accurate observer, and one who has bestowed great attention on this subject, gives an interesting case of paraplegia from injury of the spine, in which the lower half of the body was "*entirely devoid of sensation*," but the reflex actions were very manifest on the application of stimuli to the paralysed parts.

6. In Dr. W. B. Carpenter's case, when the catheter was passed the patient jerked his legs violently, and upon inquiry he "positively denied having experienced *any sensation*, being *not even conscious* of the presence of the instrument in the urethra."

These are some of the "candidates for medical and philosophic fame," and "casual observers," to whom Mr. King alludes, as those by whom "*a few indistinctly* excito-motory facts have been collected."

Testimony of the same kind might be multiplied, but it is scarcely necessary for my purpose to adduce more.

Let us now revert to the counter-testimony of Mr. King and Mr. Brereton.

Mr. King confines himself to generalities, and uses Mr. Brereton as the contributor of cases in support of his views. In one vaguely related case Mr. Brereton says "it was observed that on first experimenting reflex action could not be produced, but after being persisted in for some time it could, and, strange to say, it was discovered that after thus experimenting for a few minutes sensation also returned." This case *proves* nothing, either for or against, because every one knows that excito-motory actions do occur when some amount of sensation is present.

Mr. Brereton relates that he was once referred "by an eminent authority in this matter to two cases then in the *Dreadnought*, where sense was lost, but reflex action was excitable. But *subsequently* Mr. Molloy, a very intelligent gentleman, discovered sufficient signs that sensation was not annihilated in either of them." Such cases likewise prove nothing; it does not follow that because Mr. Molloy *subsequently* discovered sensation, sensation was present *previously*, when the first observations were made. It is common for paraplegic patients, entirely devoid of sensation at first, to regain some degree of sensation afterwards. If we meet with a case (and such cases are innumerable) in which sensation remains, we can draw no conclusion from it whatever,—it is the case without sensation which alone bears upon the present question.

But the *coup de grace* brought forward by Messrs. Brereton and King, is the following:—"One of the most interesting cases on this point which has come under my eye (Mr. Brereton's), was a patient of Mr. Key's, from accident, who had fracture of the tenth and eleventh dorsal vertebrae, and as he lay completely deprived of both motion and sensation for upwards of twelve months, full

opportunities were afforded of experimenting, and yet with all attempts success never followed,—not even the introduction of the catheter could produce this action. In this case autopsy proved a complete section of the marrow."

This case appears very conclusive to Mr. King and Mr. Brereton, but let them tell me what portion of the spinal marrow exists uninjured below the injury, when the tenth and eleventh dorsal vertebra are *fractured*. Not a particle, I will venture to affirm, so that Mr. Brereton looked "for upwards of twelve months" for an impossibility. There could be no reflex actions of the bladder, &c. when the lowest nodules of the spinal marrow were injured or destroyed, and when there was nothing but the cauda equina, or assemblage of nerves, left below the injury. Such is the great case of Mr. King and Mr. Brereton "on this point," and I would ask, With whom does the best testimony and anatomy lie? With these gentlemen, or with those whom they have attacked?

And now to sift the other dogma of Mr. King, namely, that the spinal marrow has no independent motor power below the roots of the phrenic nerve.

Supposing division of the spinal marrow to occur *at or above* the "phrenic nerve-roots" there would be instant, fatal asphyxia; all the respiratory actions of the diaphragm, the intercostal, and abdominal muscles would instantly cease, and a few gaspings from the irritation of the divided extremity of the spinal marrow would be all that could take place. Certainly no such case could be found in which there would be reflex actions. Mr. King quarrels with the whole subject, because he expects reflex actions to take place in a case in which death is speedy and inevitable. Mr. King makes a very wide assertion, indeed, when he says "Swords, daggers, balls, luxations, fractures, executions, &c. have left no ample records besides the known effects of gradual and limited diseases. The medulla has been divided in every possible way and *at every part*—patients have survived days, weeks, or months!"

It was unfortunate, to say the least, that Mr. King should fix on the "phrenic nerve-roots" as his salient point. I should like to know by what conjuration he did so. The fallacies it has led him into, however, contain within themselves, like most others, their own disproof.

So far from Mr. King's assertion being correct, when he says "there is not a single fact to show that the division of the spinal marrow at any point between its lower end and the phrenic nerve-roots has ever been attended with any reflex motion connected with its lower segment,"—so far, I repeat, from this being true, there never yet was a case of paraplegia with perfect loss of sensation and voluntary motion, in which the spinal marrow was *not* divided, either actually or effectively, by the progress of "gradual and limited diseases," at some point "between its lower end and the phrenic nerve-roots," and simply because, if the division or injury had been at or above the roots of the phrenic nerve, the result would have inevitably been, not paraplegia, but death. So that if Mr. King had known the real meaning of his words, he would have seen that he was not merely arguing against the independent action of the spinal marrow in its lower part, but was positively denying all past experience and declaring, on the strength of his own *dicta*, that no case of perfect paraplegia ever yet occurred. But there is no end to the dilemmas which present themselves. In all cases of paraplegia, except the whole of the spinal marrow is paralysed, there are reflex motions in the parts supplied with spinal nerves below the injury, unless, as in Mr. Brereton's case, it is seated in the last segment of the spinal marrow. In fine, if we do not see reflex actions when the spinal marrow is divided below the phrenic nerve-roots, we do not, cannot, see them *at all*.

I might pursue the subject further, as there are many minor points in which Mr. King is equally in error; for instance, he makes Dr. Marshall Hall say that "tetanus is indubitably of a reflex character, and that hydrophobia is indubitably of centric origin,—the first nerve irritation, and the latter poisoning of

brain-substance." Now what Dr. Hall really does say is, that tetanus may arise either from nerve irritation or injury of the spinal marrow, and that the spasms may be either direct or reflex; of hydrophobia Dr. Hall says it is a disease depending on poison absorbed into the circulation and acting on the spinal marrow. He makes no mention of "poisoning of brain-substance."

Mr. King remarks that "Intestinal irritations are funny things to cause coma or convulsions; arrest of bile or urine, tenesmus or strangury, just as they please." Will Mr. King deny that they do cause such morbid actions?

In conclusion, I quote the following passage from Mr. King:—"For a physician to be represented only humoral or exclusively nervous in his views seems a very sad picture, but to hold only an *imperfect nervous doctrine is still more partial and objectionable*," and will only add that Mr. King may spare himself all lamentation about exclusively nervous or humoral physicians, if such monstrosities really exist.

P. S. Having formerly written under the signature of "M. B." I take this opportunity of making a few remarks on the communication of "ANONYMOUS" to THE LANCET of August 10th. "ANONYMOUS" is much mistaken in supposing that he is "the propounder of no theory;" he has a theory, namely, that the functions termed true spinal are in some way connected with, and dependent on, the exercise of the purely cerebral functions. I contend, in opposition to this, that spinal action is independent of the cerebrum, and that injuries of the brain can only cause spinal actions when they at the same time affect the spinal marrow, or its incident nerves. Lesion of the brain may do this in three modes,—First, by irritation; second, by compression of the medulla oblongata; third, by irritation of the meninges. The proofs of these positions are that in vivisections no amount of injury of the cerebrum alone is sufficient to cause spinal motor action; that compression, which affects the medulla oblongata, or irritation of the membranes of the brain which contain excitor nerves, will cause convulsions; that the true spinal actions are present in fetuses born without any brain, and that they are also present in paraplegia, when the spinal marrow is actually severed in two, and the influence of the brain utterly and indubitably cut off from the parts manifesting spinal action.

This kind of evidence "ANONYMOUS" does not examine. He considers his own position safe because of certain cases which he quotes. I have once referred to these cases, and must briefly do so again.

"ANONYMOUS" says he "cannot help smiling when 'M. B.' gravely requires that all works prior to the appearance of Dr. Hall's first paper should be laid aside and regarded as useless rubbish. Lallemand, Andral, Rostan, Abercrombie, Martinet, and a host of other writers, are to be sacrificed at the shrine of the reflex theory. So far as regards neuropathy, at least, a revolutionary period has, it seems, occurred, and it is a medical treason to allude to any author prior to this new era. Important and interesting as Dr. Hall's discoveries are, I doubt whether they would not be dearly bought at a sacrifice so enormous."

Now I would not in the least impugn the eminence of these authors, considering the date of their writings, but "ANONYMOUS" takes his cases entirely from them, without supporting them by any clinical observations of his own; and since they wrote "important and interesting discoveries," as he allows them to be, have been made in the physiology of the nervous system. We have now new aids to diagnosis and the comprehension of the pathology of paralysis and convulsions, which were entirely wanting to Andral and the other authors in question; and I would ask the profession what they would think of an attempt to test the works of Laennec and other writers on diseases of the lungs and heart, since the invention of the stethoscope, by reference to the previous works by Bayle and Corvisart, excellent in themselves, but rendered, in a great measure, obsolete by Laennec's discovery? or what would they think of the chemist who should attack the doctrines of Berzelius and

Liebig with the formulæ of Priestly and Lavoissier? "ANONYMOUS" has done that which is analogous; the new era in neuropathy has no existence to him. I submit that on this point he is both illogical and behind his age.

But to go to the cases themselves, they are without any value as regards the point in debate. All those adduced by "ANONYMOUS," and formally numbered from I. to V., to which I refer my readers, are without any evidence of the presence of what he calls spinal action. "ANONYMOUS" will see how I have analysed Mr. King's cases; his own would admit of an equally ready explanation did I not fear to be too lengthy. He complains that the reflex actions are not always present in paralysis, and uses this as an argument in favour of their being dependent on the brain. They may not be evident in the cases of Andral and Lallemand, and simply because these observers did not look for them, and "ANONYMOUS," when he does look, may not always see those he wishes to find; but I can assure him there never was, or ever can be, a case of paralysis, short of the extinction of life, in which they were not evident. The more complete the paralysis of sense and motion the more complete the proof of their independent existence. I suppose he will not deny that perfect paralysis of sense and motion may exist and yet respiration go on. Well, then, if he reject other proofs, here is one against him, for respiration in such a case is entirely an excitomotor action.

Will "ANONYMOUS" now give up his readings of his favourite authors as regards this particular point, or is the sacrifice to truth too great? "ANONYMOUS" may depend on it there will be still more modern sacrifices than those he contemplates with such shuddering. The movement now in operation will not rest till these discoveries have penetrated into every department of a large class of diseases, and thereby rendered most of the present standard authors on such subjects, to a great extent, obsolete. It is not merely a question of the reception or negation of "the reflex theory," it is the discovery of the *physiology of the spinal marrow*, and the true extent of the fame of its author can only be measured when all the brilliant developments of which it is the central and primary idea, have been fully made. I will give "ANONYMOUS" an outline of what is at present in process of realisation.

I. In *anatomy*, the demonstration of what has been termed the *true* spinal marrow, but which may be termed *the* spinal marrow, together with a system of incident-excitor and reflex-motor nerves, to which the spinal marrow is the central organ. The separate existence of this organ has already been demonstrated in the invertebrata.

II. In *physiology*, the knowledge of the motor actions concerned in all the functions of egestion and ingestion, such as respiration, conception, parturition, deglutition, vomiting, &c., and the knowledge of the causes of these motor phenomena which is involved in the comprehension of the laws of action of the *vis nervosa* along incident and reflex nerves and the central organ.

III. In *pathology*, the knowledge of the causes and phenomena of convulsive diseases of every kind, and of paralysis of the purely spinal or reflex motions, together with the light this knowledge sheds on cerebral pathology.

IV. In *therapeutics*, the knowledge of the *modus operandi* of the sedatives and stimulants of spinal action already in use, on the spinal marrow, or its incident or reflex nerves, and the discovery of numerous other remedies possessing the same kinds of action.

V. In *metaphysics*, the explanation of many of those involuntary actions which were formerly unintelligible to metaphysicians, and which led to interminable disputes respecting the differences between instinct and reason; and a knowledge of that hitherto mysterious and unknown power which enables birds to maintain their flight uninterruptedly across continents and seas, and fishes to swim in their wonderful migrations from one ocean to another.

I conclude by inviting "ANONYMOUS" to aid in

the advance rather than the retrogression of this department of physiology and pathology. This he cannot do without first studying the works of Dr. Marshall Hall. In return, I would cheerfully study the works he refers me to, did I not feel sure that I might better employ myself with other authors and at the bed-side.

NITRATE OF UREA AS A DIURETIC.

By E. W. C. KINGDON, Esq.

KNOWING that you are ever ready to give a place in your valuable Journal to anything novel in the study of medicine, I am induced to request that you would insert the two following cases illustrative of the beneficial action of the nitrate of urea in dropsical affections:—

CASE 1.—J. J., ætat. 50: Musselburgh, May 12th. Was long affected with anasarca of the lower extremities along with slight ascites, dependent on disease of the heart. This case had, in a measure, resisted the powerful effects of a combination of squills, digitalis, and calomel. When I first saw him his legs were much swelled, breathing laborious, and urine exceedingly scanty. Let him have the following pills:—Nitrate of urea, calomel, of each twenty-four grains; conserve of roses, sufficient to form twenty-four pills. One to be taken night and morning. I had not an opportunity of seeing him again for a week. The change for the better was very obvious. His limbs were reduced to their normal size, respiration much improved, urine discharged copiously. On inquiring into the action of the pills he replied, "that after taking three or four of the pills his urine came in a 'regular gush' (his own expression), and that he continued under their use to discharge it freely." He continued to take the pills until the twenty-four were finished, when his swellings were entirely gone. I may add that in this case the calomel was used merely to gratify the caprice of the patient, who was bigoted to its use, although I have no doubt, taking into consideration the properties of calomel, that, in the case now before us, it assisted the nitrate of urea in producing diuresis.

CASE 2.—Mrs. J., Fisherrow: June 2nd. Anasarca in consequence of diseased kidney; urine very scanty, and high coloured. Let her have the following pills:—Nitrate of urea, eighteen grains; conserve of roses, sufficient to form twelve pills. One three times a day.

4. Swellings less; urine increasing.

6. Still continuing her pills; urine voided in considerable quantity; swellings greatly less.

10. Swellings entirely gone; general health good.

Remarks.—Although these two solitary cases in favour of nitrate of urea by no means fix its reputation on anything like a firm basis, yet I think they render it worthy of a more extended trial being made of its diuretic properties. The mode of action I cannot well account for, if it be not founded on the homœopathic dogma "*similia similibus curantur*," which may be made to apply here, when we consider that urea is the chief constituent of the urine, and that in dropsies the urine is always very sparingly secreted. Therefore supposing that dropsies are prevented from leaving the cavities, &c., by the languid action of the kidney, and that that languid secretion is cured by the administration of urea, and the serous fluid drained off by the excited action of the above organ, we might, I think, assign the action of the nitrate of urea to the homœopathic doctrine of "*similia similibus curantur*."

With regard to the cases, it might be said of the first case that the diuresis was occasioned by the remote action of the digitalis and calomel cumulated in the system, and not by the urea administered. I may state, however, that the digitalis, &c., had been given up for upwards of a month previous to the administration of the urea. With regard to the second case, the nitrate of urea was the primary treatment. In conclusion, I think, from the success which attended the above cases, as well as the smallness of the dose employed, that urea must be a very active diuretic.

Musselburgh, Edinburgh, August 1, 1844.