plication, the *spasm of the muscles of the larynx alone*, has often proved suddenly fatal."

3, North Crescent, Bedford-square,
March 26, 1838.

MR. NEWPORT'S "SECOND REPLY" TO PROFESSOR GRANT.

To the Editor of THE LANCET.

SIR:—A letter from Professor Grant, inserted in THE LANCET of the 17th instant, obliges me again to request that you will allow me space for a few comments.

In my former "Reply" to Dr. Grant I stated, as I thought distinctly, the points of difference between him and myself; but the manner in which he confuses his views with mine, renders it necessary that I should again state shortly the difference of our opinions. It appears that he would still wish it to be thought that his views and mine of the *true motor column* in articulates are identical, and that my descriptions of the motor column refer entirely to the transverse nerves. Now, it is well known that I have never claimed the discovery of the transverse nerves of Lyonet, but only the pointing out some interesting facts in the anatomy of these nerves, as to the manner in which they pass over the ganglia, which the extract I gave from Müller's "Archives" in my "reply," will prove. The assignment by me of the function of respiration to these nerves, a function suggested by their distribution, was published in my paper in the "Philosophical Transactions," in 1832, *before the time at which Dr. Grant claims to have assigned that of motion to them*. Whether my function, assigned from their anatomical distribution, or that of Dr. Grant, from analogy, be correct, remains yet to be seen, but it is questionable whether Dr. Grant can any longer call these nerves the *true motor column*, since, in those animals in which they exist, they are distributed chiefly to the trachea and muscles of respiration, and since the structure to which he also assigned the function of motion in the scorpion, and which he considered the analogue of these nerves, has been proved to belong to the *vascular*, instead of the *nervous* system.

The existence of a distinct and true motor column in the nervous centre of articulates, I do claim to have first pointed out. This structure, or column, is not, however, situated loosely on the upper surface of the nervous chord, free and widely separated from the other parts of the chord, as Dr. Grant's *presumed* motor is, but placed beneath the transverse nerves, closely united to the inferior or ganglionic part of the chord, and gives off nerves to join with those which come from this same portion of the nervous chord. Its structure is that of two longitu-

dinal bands, best observed as they pass over the ganglia, where a line may be seen indicating its separate existence. This is the structure, then, for which I claim the function of motion, and which Dr. Grant, in his letters, carefully avoids to mention, and when obliged to speak of a motor column, confuses with the transverse nerves, which, as I have shown, certainly do not constitute the true motor column.

I have thus explained, a second time, that I had *assigned a function* to the transverse nerves *before any had been assigned to those very nerves by Dr. Grant*; that those nerves do not constitute, as he has described them, the *true motor column*, because of their particular distribution to the muscles of respiration and the tracheæ; and I have elsewhere shown that what he has referred to in the scorpion, as the *motor column*, does not belong to the nervous system, and consequently he has not yet described the *true motor column* in these animals. These facts, I trust, will prove that I have not been indebted to Dr. Grant for my opinions respecting the structure and functions of the nervous system of articulates.

This brings me to another part of Dr. Grant's letter, in which I think I may justly complain of his unfairness and misstatements. Conscious of the difference of our position in science, and the greater credit that would therefore be attached to his statements, he has attempted, in consequence of a little ambiguity in Lyonet's expressions, to pervert their true meaning, in order to show that I have been anticipated by this author. Lyonet describes, and in one instance, Plate IX., figures, the transverse nerves as connected to the chords which pass from the brain, or first ganglion, to the second, by means of two portions which seem ("*semblent*") to be continuous with the fibres of these chords, and which, converging, pass over the second ganglia to unite on the third, to form the first transverse nerves, "*la bride epiniere*;" but Dr. Grant, in stating this fact, has used the words "*columns*," and "*broad white bands*," as passing over the ganglia, terms which are not to be found in the description of Lyonet. Again, the whole sentence in Dr. Grant's last letter, beginning—"In the figure of Lyonet, then exhibited and described to the class, the transverse nerves are delineated as passing into *columns*, which extend over the upper surface of the ganglia, at both ends of the nervous axis, and as coming *always from and over the upper surface* of the chords, in the intervening parts of the axis,"—is quite unsupported by Lyonet's description; for, even in the sentence from which Dr. Grant has made the extract, he has thought fit to suppress the concluding portion, which is opposed to his statement, and the words of which are after "*qui en derive*,"—"ce que ne se remarque pourtant pas dans tous les

sujets." Lyonet has, moreover,¹ *distinctly stated*, in the following words, at page 204 of his work published in 1760, that the origin of the last or tenth transverse nerves, *is not derived from the chords which connect these ganglia together, but from the posterior part of the penultimate ganglion.* "La dixieme et dernière bride ne tire pas, comme les autres, son origine du conduit de la moëlle epiniere mais de l'extrémité du penultième ganglion, d'ou passant pardessus le dernier ganglion, le nerf, qui tient à cette bride, la produit vers la 11me division." Besides this, Dr. Grant has entirely forgotten even to allude to Lyonet's general description of the origins of the transverse nerves, "brides epinieres," at p. 98, where Lyonet states that—"On voit que, de l'extrémité postérieure des ganglions, dont les conduits sont doubles, et du commencement de chaque separation de ceux, dont les conduits ne sont simplement que fourchus, descend un nerf," &c., the origin of the transverse nerves; so that, notwithstanding Dr. Grant's statements, Lyonet has described these nerves as *coming from ganglia*, as well as from the *chord*, which he nowhere states to be composed of motor and sensitive structures. Although Dr. Grant has thus attempted to show that I was anticipated by Lyonet in my descriptions of the nervous system, I think it will be seen that he does not succeed in his endeavour, since the above extracts refer entirely to the descriptions of the transverse nerves (my respiratory) and not to what I have described as *the true motor tract, or column*, which still exist beneath these converging portions of the transverse nerves, and from which these nerves derive a few filaments, as stated in my paper in the "Philosophical Transactions," 1834, page 404. But Dr. Grant has asserted more than this. He states that the drawings I have published with my paper in 1834, are a "*fac simile*" of those of Lyonet, of 1760, plate ix., fig. 1. This, as every one will perceive, is incorrect, when I state that Lyonet's drawing represents the nervous system of the *cossus*, a moth of peculiar habits and structure, while my own drawings represent that of the *sphinx*, a moth of a very different description, and also of the *common nettle butterfly*; and that, when the drawings of Lyonet and myself are compared, and minutely examined, the details in each will be seen to differ greatly, and are no more identical in these three species of insects, than are the details of the spinal chord of the hedgehog with those of the spinal chord of the mole or of the shrew.

Besides this reference to Lyonet, Dr. Grant has quoted Straus, Edwards, and Andouin, to show that I was also anticipated by them; but a perusal of their works will prove the contrary, since in none of them is there any mention made of the existence of *motor columns*, nor of the compound structure of the spinal chord; more-

over, Dr. Edwards himself, in his article on "Crustacea," in the "Cyclopædia of Anatomy," page 769, has referred to me as the first to point out distinct motor and sensitive structures in crustacea, which had been "hitherto overlooked by anatomists."

Notwithstanding the levity with which Dr. Grant, in his last letter, treats of my "juvenile," as he is pleased to call them, "productions," they merited perhaps a little more consideration, as the following fact may show. In my first paper, which Dr. Grant examined before it was read to the Royal Society, I described the visceral, or *recurrent nerve* of Lyonet, as the *vagus*, and considered my opinion original, until Dr. Grant wrote against the passage on the draft paper, which I still possess, the following remark:—"The opinion commonly received of Straus," &c., "*nerfs des organes vitaux*;" and in consequence, on the authority of Dr. Grant, I mentioned Straus in my paper, as the author who had previously described it as the *vagus*. At that time I was not acquainted with the work of Straus, but have since found that he *nowhere describes it as such*. Now, Dr. Grant had made it convenient to mention in his lecture in 1832, the *existence* of the *vagus*, as proved by his extract from Dr. Baly's notes, without once alluding to the source of his information.

After the facts I have stated, and the refutation of the unjust inferences made by Dr. Grant from the writings of Lyonet and others, and which perhaps will show with what fairness he has accused me of plagiarism, I enter on the misstatement with reference to myself. I again repeat, that I showed to Dr. Grant, in Nov. 1833, a preparation of the nervous system of the lobster, with the motor and sensitive columns, and which had previously been shown to Dr. Hall and others; and I must again assert, that the preparation which Dr. Grant showed to me on that occasion was one of the scorpion. I may add, that Sir Charles Bell first published a concise description of my preparation of the motor and sensitive columns in the lobster, in a letter dated Dec. 2, 1833, in the "Medico-Chirurgical Review," for Jan. 1, 1834. The preparation itself, dated Oct. 10, 1833, is now in the museum of the College of Surgeons, numbered 1302, A, and was sent there by Sir Charles Bell, and not by myself, as printed by mistake, in the catalogue of the Hunterian Museum, vol. iii., 1836. I am not a little surprised at Dr. Grant's statement, that a structure which he alludes to as a "*single median motor filament*," "has been shown to me" to be a ventral vein returning blood to the heart. I deny that any student ever showed to me that this structure was a vessel. If Dr. Grant can possibly refer to the *publication* of the fact, I have stated elsewhere that I was aware of

its being a vessel long before the publication of it. Dr. Grant next says, that "no person, except Mr. Newport himself, ever figured, or described or imagined, my motor nerves of articulata to proceed from ganglia, or to form ganglia in their course," &c. Now, if Dr. Grant had ever actually traced the different phases which the nervous system undergoes in the progress of development, he would then have known that *there are stages* in which the transverse nerves assume "a transitory ganglionic appearance," as I have stated in my papers in 1832 and 1834, and a trace of which frequently remains.

Towards the conclusion of his last letter Dr. Grant again accuses me of plagiarism, notwithstanding this charge has been already answered, and he afterwards uses the following words, in reference to a notice in my paper, published in the "Philosophical Transactions" for 1836, at page 542, in which I have alluded to some observations of his in a lecture in THE LANCET of July 5, 1834, page 515. He says, "to this I beg leave simply to reply, that *there is no sentence in that reference which either the most designing or the most ignorant could construe into such a meaning.*" Now, the quotation which Dr. Grant professes to give from my paper, is both incorrect and incomplete, he having terminated the extract he has given not at a period, or even at a comma, but *actually between two commas*, with an &c. The following are the words of my paper:—"It has been stated by Dr. Serres and Dr. Grant, that a series of ganglia exists on each side of the alimentary canal, which appears to be independent of the transverse nerves. I must acknowledge, however, that I have been unable to trace this series *beyond the anterior lateral ganglia, which are contained within the head, or first segment, &c.*"—(Op. cit. p. 542-3.)

To Dr. Grant's assertion, therefore, I reply simply by quoting, from his own printed lecture, the passages to which my remark refers:—

"There are *numerous* other nervous ganglia in insects, and other articulata, besides those which are connected with this great nervous column, the analogue of the spinocerebral axis of the vertebrata. *These* are seen extending from the cerebral lobes themselves, small nervous filaments, which unite into the first of a *longitudinal series of sympathetic ganglia*. The first of these *sympathetic or visceral* ganglia sends out filaments, and farther on, in the course of those filaments, another ganglion is observed; thus is the system of the great sympathetic so minutely detailed in the plates of Lyonet. *Those filaments of the sympathetic ganglia are observed to be distributed upon the principal viscera of organic or vegetative life*, and it is by the development and extent of this *ganglionic system of the viscera*, so minutely

described by Lyonet, Dufour, Straus, and many others, that *the vital connection of all the segments is chiefly established.*"

A little farther on, in the same lecture, Dr. Grant again refers to the subject, and says,—"*These numerous ganglia occupy a middle position in the body, below the level of the cerebral lobes, and above that of the abdominal ganglia.*" These, then, are the passages to which I referred, and I leave your readers to judge whether or not my statement was correct. Lyonet, and the authors named, have described the sympathetic ganglia of the head, and the existence of these is also noticed in that part of the sentence from my paper which Dr. Grant omitted to quote.

In concluding this "second reply" to the attacks of Dr. Grant, I can but again remark, that the many personal allusions he has made respecting me, both in his first and second letter, are as derogatory to him as they are unmerited by myself; and that his reference to the "humble occupation" in which I spent my youth, made, as it would seem, with a view to depreciate my claims in the consideration of your readers, but which humble occupation, I trust, no honourable mind could possibly consider a discredit to me, is more pitiable in him from the circumstance that the fact was communicated to him *in confidence*; and surely his own experience in life ought to have taught him to have been the last to scoff at the *difficulties* of others; while I hope that your readers will allow me to have possessed, in this controversy, one little superiority over my opponents, that of having sought for no weapons in my defence which science and gentlemanly courtesy alike disdain.

With these remarks I must close my present communication, as much regretting that my opponents should so far have forgotten their own dignity, as not to have confined their observations to science, as I hope not again to have occasion to intrude myself on your pages; whilst, however, the statements of interested individuals, whether made in public or in private, may, or may not, have influenced the opinions of contemporary physiologists, conscious of the justness of my own claims, I shall calmly await the decision of time, the impartial arbitrator of all controversies. I remain, Sir, your very obedient servant,

GEORGE NEWPORT.

10, University-street,
March 26, 1838.

P. S. In my second reply to Dr. Hall, printed on the 24th of March, I observe there are two misprints, which I shall feel obliged by your correcting. The first is in the date of the receipt, which is printed 1822, instead of 1832; and the other is in the date of Dr. Hall's note, printed 1835, instead of 1836.