invariably enfeebled, but not always, also retarded; it was generally quick and weak.

The peculiarities of this case, which was also seen by my respected friend, Mr. Tager, surgeon to *Mercers' Hospital, &c.*, and which eventually recovered, I shall now conclude by a summary of interesting deductions from, as to the action of mercury and the symptoms of erethism.

First. Mercury may agree with the disease, that is, may speedily extirpate it, and yet disagree with the constitution, though seeming to agree with the latter *so long as the syphilitic virus is inherent*, making thus, as it were, a choice of antagonists. A common phrase, therefore, "to give mercury in syphilis, if it agree with the constitution," must be very inaccurate, since it may not choose the constitution as its *first* antagonist, and, therefore, cannot be known to agree till the syphilitic virus be removed, that is, till it be fully tried.

Secondly. Mercury acts specifically on the syphilitic virus in the constitution, and not on this virus through the medium of the constitution; for, in the case above related, it most speedily checked the venereal virus, and yet, immediately after, proved a poison to the constitution, by agreeing with which it cannot, therefore, be supposed to establish it against the venereal poison; although, by disagreeing with it originally, that is, by taking the constitution as its *first* antagonist, as it does when causing erythema, febrile symptoms, furred tongue, and diarrhoea, it may prevent us giving mercury farther to destroy the syphilis; or, if continued, may act as fatally on the constitution, taken as its first antagonist, indicated by erythema, fever, &c., as when chosen as its second, during or after its direct action on the venereal poison, indicated by the symptoms of erethism.

Thirdly. The probability of mercury acting as a poison, is not to be considered inversely as the apparent strength of constitution, although the impropriety of administering it be *DIRECTLY* as the disordered condition of the health; for mercury is with perfect safety administered to thousands with constitutions apparently inferior to that of the patient alluded to.

Fourthly. By considering mercury and syphilis, as we are induced, mutually to act, and, perhaps, react, *DIRECTLY on each other*, we can account for effects of mercury when given for syphilis, which *do not result* from its use in *other affections*.

Fifthly. Intolerable and obstinate headache is sometimes a symptom of mercurial erethism.

Sixthly. The pulse is *not* invariably quick, weak, and intermitting.

Seventhly. Nor, although enfeebled, is it always retarded in sleep.

C. EDWARDS, A.B., Surgeon.
7, Portland-st., Cheltenham. Feb. 3, 1838.

REPLY TO

MR. NEWPORT'S INSINUATIONS

RESPECTING THE WRITINGS OF

DR. MARSHALL HALL AND DR. GRANT.

To the Editor of THE LANCET.

SIR:—In the last Number of *THE LANCET* (Feb. 10, page 715) Mr. Newport, a former pupil of mine, who has composed some papers on insects, has thought proper to indulge in animadversions and insinuations reflecting on the character of Dr. Marshall Hall, in consequence of the latter gentleman having republished the well-known fact of my having demonstrated (to my class, in 1832,) the existence and the function of the true motor nerves and columns of articulated animals,—more than twelve months prior to the appearance of any observations or conjectures of Mr. Newport on that subject. This gentleman (Mr. Newport) in the above communication, appears now "anxious to make the public believe," not only that this statement of Dr. Hall is incorrect, but that the merit of this discovery is due to himself; and he considers it would be unjust and inconsistent in me to disturb his supposed claims. The truth, however, of Dr. Hall's statement, which has been so long and frequently published by myself, and others, was also well known to Mr. Newport himself, as attested by his own printed papers, by the evidence of his co-pupils, and by his having attended and taken copious notes at more than a hundred and twenty of my lectures on Comparative Anatomy, in the session of 1832-33, when the motor nerves and columns of articulated animals were demonstrated and discussed. But however anxious he may now feel to suppress the fact, and to arrogate the discovery to himself, he has distinctly, though no doubt inadvertently, announced it in his own printed paper, where he acknowledges "that I first informed him that these nerves," in the columns of articulated animals, as in those of vertebrata, "pass over" the surface, and not through the substance of "the ganglia," (*Phil. Trans.* 1834, p. 402,) and the period is now past for even the most ignorant to be blinded or complimented by designating such nerves, in whole or in part, *respiratory*, in order to avoid indicating their *motor function*. My views on these subjects, which were frequently expounded to Mr. Newport, in public and in private, when my pupil in 1832, and which he is now desirous to claim for himself, were totally at variance with the statements and conjectures which he published in that year, when he imagined and described, and figured, the principal part of my motor system of insects as presenting large ganglia in their course, a mistake which even Lyonet, in 1760, had taught

anatomists to avoid. It is also well known to Mr. Newport, although he affects to deny the fact, that these views respecting the motor function of the columns which pass over the ganglia in the articulated classes, were fully explained and published in the pages of this Journal, in November, 1833, before the public had been favoured with a single observation, or suggestion of any kind, from him, either on the existence or the function of motor nerves in articulatæ; and, indeed, they had been demonstrated to him, in my class, a year (according to his own confession) before he first dreamt of looking for such nerves in these animals. Had I been entirely ignorant of the difference of function which had been established between nerves which *pass into ganglia*, and those which are *continued over their surface*, in the nervous axis of animals, and studious to conceal the fact that Lyonet had traced nerves of the latter character, into the muscles of every segment of the insect, and distinctly figured them passing over the last ganglia of the columns,—had I not known that Straus had often distinctly traced such columns over the upper part of every ganglion of the nervous axis, in many insects, or that Treviranus had seen and figured such a tract, superadded to the ordinary sensitive columns, in the *alvi-thorax* of the scorpion; and that Müller had distinctly traced and delineated this free motor column over the surface of every ganglion of the same animal,—I might have considered my announcement, “that I had found this to be a structure common to the articulated classes, and that I regarded the upper columns which pass over the surface of the ganglia, in these classes, as their *motor columns*,” as a discovery of nerves never before imagined to exist in articulated animals, and so have arrogated to myself the merit which is due to those who preceded me in this inquiry. In the notes of a pupil, which were written while sitting beside Mr. Newport, in my class, in 1832, about twelve months before Mr. Newport began to think of looking for motor nerves in the articulated classes, I find the following expression in reference to these animals:—“and we may consider those (nerves) which do not pass through the ganglia, as NERVES OF MOTION.” As I had uniformly found that my predecessors were correct in tracing the interganglionic nerves, in all these classes, to the *upper*, and not to the *sensitive* columns, I had no hesitation in demonstrating these interganglionic or transverse nerves, as belonging to the *motor* system, even before I had experimented upon them with galvanism, in the lobster, in presence of Mr. Bennett (one of Mr. Newport’s boarders); but this fact is never acknowledged by Mr. Newport, excepting anonymously, and in expressions like the following—“and it has been *imagined* by some that these transverse nerves *may perhaps be*

analogous to the motor.”—(*Phil. Trans.*, 1834, p. 401.)

The present extraordinary and uncalled-for conduct of Mr. Newport towards his friend and benefactor, Dr. Hall, which has alone compelled me to trouble you with these comments, has strongly recalled to my mind similar dealings of this gentleman towards myself, which had almost escaped my memory. Having made himself acquainted with my yet unpublished views, during the two courses of my lectures which he attended in the University, this wily personage now shunned my sight, and became the pupil and inmate of Sir Charles Bell, whom he proposed to gratify with the rediscovery of the motor nerves of articulatæ, in recompense, no doubt, for a little sucking of his brain, or his purse,—a recompense which Dr. Hall, however, seems to have little prospect of being offered for all his past services to this ungrateful parasite. It was not till near the end of 1833, when my lectures were publishing in this Journal, that I was led to suspect Mr. Newport’s intention to plagiarise my observations on these nerves, by a manoeuvre which he practised in my class-room, with a preparation he had made of the lobster. This preparation, *which I never looked at*, was brought to me in order to afford him a plausible pretext for the misstatement which he forthwith published in his essay, that he had shown me upon that occasion his supposed discovery; while, on the contrary, *it was I who pointed out the motor columns to him, on that occasion, in my own dissected specimens of that animal*, as soon as he announced the object of his visit; and, on reminding him of the observations I had formerly demonstrated to him on this subject, in my public lectures, and of the published diagrams by which I had been then accustomed to illustrate the history of this discovery, for the sake of consistency, he affected to have forgotten everything. Although this manoeuvre failed in its object, it might have succeeded had not my pupils of the preceding year (1832), preserved an accurate record of my having *then* distinctly assigned the *motor function* to the column which pass over the ganglia in the articulatæ; and of my having *then* also demonstrated, in confirmation of my views, the figures of Treviranus and of Müller on this subject, which Mr. Newport has had the audacity to deny.—(*Phil. Trans.*, 1834, p. 408).

These are some of “the circumstances, which (to use the words of Mr. Newport’s base insinuation), if made known, would, I trust, sufficiently prove unto whom this discovery really does belong.”—(*LANCET*, Feb. 10, 1838, page 715.) My views even regarding the *apparently inverted position* which these motor and sensitive columns occupy in the articulated classes, which were published in the *THE LANCET* in 1833, have

not escaped this plagiarist, who has published them also as his own, in the "Philosophical Transactions" for 1836, p. 541; and there is scarcely a single instance in his printed papers in which his reference to my name is not accompanied with a distinct misstatement of facts or opinions. After vainly trying, for several years, to establish a difference between his pretended discovery and my first announcement of the motor columns of articulatæ, by making the interganglionic nerves of these animals at one time *gangliated*; at another, *sympathetic*; at another, *respiratory*; at another, *involuntary*; at another, *mixed*; at another, *modifications* of some of these types: he has at last ventured to *discover* (*Phil. Trans.*, 1836, p. 544) that *they contain some voluntary motor fibrils*, in the columns which they form, as I had described them to him in my public lectures in 1832.

Misstatements of Mr. Newport, on matters of more importance than those I have above cited, and respecting an eminent fellow-labourer in this department of science, to whom, of all men, he was the most beholden, have been recently detected and exposed before the *Royal Society*, which would almost lead us to believe that he had been prompted to venture for a second medal from that body. Had this most fickle and uncandid of all inquirers into science, confined his misrepresentations, on the present occasion, to myself or my opinions, I should never have trespassed on your valuable space with the slightest notice of them, relying on the numerous distinct contradictions which his arrogant pretensions have already received in my published writings, and in this Journal. As he has chosen, however, to direct his insolent insinuations against the character of his eminent and honourable patron, who has so long cherished this cunning and arrogant egotist, and who is peculiarly qualified to examine into the merits of such questions as the present, I have felt it a duty to state the above facts, that your readers may better judge of the weight to be attached to aspersions coming from such a source, and directed against a character like that of Dr. Marshall Hall. I remain, Sir, with great respect, your most obedient servant,

ROBERT E. GRANT.

10, Seymour-place North, Euston-square,
February 12, 1838.

LETTER FROM DR. MARSHALL HALL.

To the Editor of THE LANCET.

SIR:—I trust you will excuse me for addressing this note to you and your readers, instead of to your correspondent in p. 715. Assertions of "decided misrepresentations," "suppression of facts," "passages not in

strict accordance with facts," without any attempt at *proof*, should scarcely have been admitted into your pages. I cannot be supposed capable of supplanting the writer to acquaint me with *what* these gross derelictions from truth may be.

My object in this communication is not, therefore, to reply to your correspondent, but to establish what I have asserted. On referring to my lecture, p. 650, § 15, 16, I find that I have asserted *five* things. These five things I will now proceed to prove:—

1. Lyonet not only described and figured all the large interganglionic nerves of insects as originating exclusively from the upper surface of the columns (*Traité de la Chen.* 1760, pl. ix., figs. 1, 2), but has also represented the column formed by the united interganglionic nerves as passing distinctly over the upper surface of the last pair of ganglia (Pl. ix., figs. 1, 12).

2. Treviranus subsequently observed and figured this third column between the thoracic ganglia of the scorpion (*Ueber der Arachnid*, 1812, tab. 1, fig. 13); but he considered it a peculiarity of this animal.

3. Professor Müller has the merit of having first pointed out the continuity of this apparently single column over the upper surface of all the longitudinal ganglia of that animal (*Meckel's Archiv. für Anat. und Phys.*, 1828, tab. 1, fig. 5), although he did not determine its nature.

4. The *motor function*, however, was first assigned to the superior columns of insects, and to these united columns of the scorpion, by Dr. Grant, in 1832; and he also pointed out the *general occurrence* of this structure in the *articulated classes*, and the relation which the *inverted position* of these columns bears to the other viscera, and to the corresponding parts of vertebrated animals (see his published lectures of 1835, vol. i., p. 352, *et passim*).

In proof of this last fact I insert an extract from the notes of Mr. Storrar, taken during his attendance on Prof. Grant's lectures in 1832; to which it must be added, that Mr. Storrar distinctly recollects Prof. Grant's having shown Prof. Müller's plate, and read an extract from his paper.

"—— Between the ganglia a nerve is seen passing down, which divides into two—and we may consider those which do not pass through the ganglia as nerves of motion." "Dr. Müller, of Bonn, has observed in the scorpion a nerve passing down on the outside of the ganglionic chord, which tapers at the extremity, and is difficult to separate from the last ganglion, and it is thicker than both the other nervous chords put together; probably, he says, Treviranus has seen it, &c." "Now this is nothing more than we might expect, because it will be of uniform size, as no (?) nerves enter it to produce ganglia."

5. Is not my assertion, p. 650, § 16, proved