

**A letter addressed to the Earl of Rosse, President-Elect of the Royal Society / by Marshall Hall.**

**Contributors**

Hall, Marshall, 1790-1857.

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PRIVATE.

A LETTER

ADDRESSED TO

THE EARL OF ROSSE.

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HALL, M.

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A LETTER

ADDRESSED TO

THE EARL OF ROSSE,

PRESIDENT-ELECT OF THE ROYAL SOCIETY ;

BY

MARSHALL HALL, M.D. F.R.S.

&c. &c. &c.

LONDON :

OCTOBER, MDCCCXLVIII.



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It will be said hereafter—" In the early part of the nineteenth century, *the Physiology of the Spinal Marrow* was first detected. The discovery was laid before the Royal Society, and—rejected!"—*p. 7.*

The *idea* of an Incident Nerve, in *essential* relation, through the Spinal Marrow, with certain Reflex Muscular Nerves, did not previously exist in *Anatomy*.—*p. 10.*

The *idea* of a Reflex *Function* of the Spinal Marrow ; the *idea* of a Reflex *physiological act* did not exist in *Physiology*.—*p. 10.*



## A LETTER,

&c. &c.

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MY LORD,

I REJOICE in your Lordship's accession to the Presidential chair of the Royal Society, in the confident hope that you will not permit such wrongs as are described in the following appeal to be persisted in, in that hall of British science; an appeal which I pen entirely on a principle of duty, keenly regretting the necessity for speaking at all of myself and of my own labours.

I put aside all further prefatory observations, and proceed at once to lay before your Lordship a plain and simple detail of facts, as briefly as possible.

But there is one preliminary circumstance which I must mention. In 1831, I read a paper before the Royal Society, entitled "On the Anatomy and Physiology of the Minute and Capillary Vessels." This paper contained the discovery and description of the intermediate blood-channels situated between the minute arteries and minute veins, both in the pneumatic and systemic circulations. It was rejected, and replaced by what I venture to designate, after the lapse of seventeen years, as utter error, by *a member of the Council!*

It was during the investigation of that subject that I was struck, in 1831, with an extraordinary phenomenon. I observed that if the tip of the separated tail of the *Triton* be irritated by the point of a probe, that severed portion of the animal *moved*, assuming various curved and contorted forms.



The questions—What is the nature of these movements?—What their motor power? &c. &c.—immediately flashed across my mind.

From that day to this, from the year 1831 to the year 1848, that is, during eighteen years, I have not ceased to prosecute the inquiry which these questions suggested. My labours have, I hesitate not to say, issued in a discovery, the extent and value of which is without a parallel in modern physiological science.

But I proceed with my plain and simple history of facts.

In 1833, I read a paper before the Royal Society, the first fruits of my new labours, entitled “On the Reflex Function of the Medulla Oblongata and Medulla Spinalis.” That paper was printed in the *Philosophical Transactions*, and immediately reprinted in the *Archiv für Physiologie* of Prof. Müller of Berlin.

Thus encouraged, I pursued my career of investigation, and I presented a *second* paper to the Royal Society on the same subject, after *four* additional years of labour, in 1837, entitled “On the True Spinal Marrow, and the Excito-motor System of Nerves.”

But now I had been informed, in confidence, that Dr. Roget, the Senior Secretary of the Society, had expressed his intention of procuring the rejection of this paper!

I resolved not to be unprepared. I therefore requested Mr. Lawrence to be present at the meeting of the Council at which the fate of my paper was to be determined. This Mr. Lawrence promised; not believing, however, as he expressed himself, that I needed any advocate there.

Unfortunately Mr. Lawrence was summoned away. *My paper was rejected accordingly!* Or, in milder terms, Mr. Children, the Junior Secretary, wrote to me to advise me to withdraw it! *The rumoured threat was accomplished.*

Under the impression that Dr. Roget purposed to cause the rejection of my paper, I freely communicated with Mr. Lawrence. I explained to that gentleman, that, in my view of my subject, it was of such importance as to induce me to offer to the Royal Society the sacrifice of my professional



career for five years, in order that I might pursue my inquiry without interruption, and that he was at liberty to make that statement to the Council.

When had the Royal Society of London for the *promotion* of natural knowledge, such an offer—before or since?

After the receipt of Mr. Children's note, I addressed the Council in a printed letter, couched in the most appropriate terms I could devise,—in vain. I received a cold-hearted, or rather a cold-blooded, reply.

It will be said hereafter—"In the early part of the nineteenth century, *the Function of the Spinal Marrow*, previously confounded with *sensation*, or "sensory impressions," in a word, with that of the *Brain and the intra-spinal chord of cerebral nerves*, was first detected. The discovery was presented to the Royal Society, and—rejected!"

I must not omit to state that Dr. Roget, even in drawing up his official Abstract of my paper, could not refrain from misrepresentation and insult. He states—"The author begins by observing that a former memoir of his, entitled 'On the Reflex Function of the Medulla Oblongata and Medulla Spinalis,' published in the Philosophical Transactions for 1833, has been translated into German, and favorably spoken of by Professor Müller, of Berlin." I never began in any such manner, or wrote any such thing. What I did say was this, and very different:

"My former Memoir '*On the Reflex Function of the Medulla Oblongata and Medulla Spinalis*,' published in the Transactions of the Royal Society for 1833, was speedily translated into the German language and inserted in the '*Archiv für Anatomie und Physiologie*' of Professor Müller of Berlin (for 1834, p. 347). Professor Müller has also given a peculiarly interesting notice of the subject, in his '*Handbuch der Physiologie*.' This eminent physiologist was led, indeed, altogether independently of me, to form similar views founded upon similar facts and observations; he assigns to them the same designation, and speaks of them as 'new, and constituting a decided step in our science.'"

"This remarkable corroboration of my views by the independent observation and unbiassed testimony of so high an authority as that of Prof. Müller, is highly satisfactory and gratifying to me. Prof. Müller observes in his '*Handbuch*,' vol. i, p. 688—"The phenomena now to be noticed were observed nearly at the same time by Dr. Marshall Hall and myself. The greater part of the '*Nervenphysik*,' as it is here given, and the present chapter on the reflected motions after sensations, were composed several years ago. That this is true,



is obvious from the first part of this work, which appeared in the spring of 1833, and in which (p. 333—335) the fundamental principles of this doctrine were briefly given. It is remarkable that the same views, with the same instances and observations on narcotised animals, were propounded in the very same year by Dr. Marshall Hall, in a Memoir published in the Philosophical Transactions (of 1833).’

“I had, however, published a short sketch of the subject of my investigation, in a communication made to the Zoological Society, which was inserted in the Proceedings of that Society, in 1832.”

Dr. Roget proceeds—“He states that his object in the present paper is to unfold *what he calls* (!) a great principle in physiology.”!

He concludes—“Reference is made, in the course of this paper, to several drawings, which, however, have not been supplied.” This was untrue. The drawings were in the hands of Mr. Children, the Junior Secretary, at the time!

This latter gentleman, in his turn, wrote on my manuscript, after reading a most interesting experiment on the tortoise, “*Will they move after they are made soup of?*” Alas, for Science!

I now published my two papers together, under the title of “Memoirs on the Nervous System.” I send this volume for your Lordship’s examination. It is before the world, and has been translated into every continental language.

I still pursued my career of inquiry, now under every kind of discouragement. Thinking they might safely follow in the wake of the Royal Society of London, a host of critics arose, and Whytt and Prochaska were put to the torture to induce them to give false evidence against me—in vain! Dr. Todd, a member of the Anatomical and Physiological Committee, and *one of my judges* (!), at the Royal Society, has recently distinguished himself in this way.

In 1843, after other *seven* years of labour, I added a “New Memoir,” of course without presenting it to the Royal Society. This I also forward to your Lordship. It is entitled “On the True Spinal Marrow, and its Anatomy, Physiology, Pathology, and Therapeutics.”

I do not hesitate to proclaim here, that, had the value of my first paper been duly and justly appreciated at the Royal



Society, the Royal Medal for Physiology for 1833, would have been adjudged to me.

Nine years afterwards, in 1842, the Committee of Anatomy and Physiology, under the presidency of Sir B. C. Brodie, *did* recommend that the Copley Medal should be awarded to me.

Sir B. Brodie kindly informed me himself of this vote of the Committee. I replied—"Dr. Roget feels towards me like a man who has injured another; and whilst he remains on the Council, no justice, far less any honor, will be awarded me at the Royal Society." *My prediction was fulfilled!* The recommendation of the Committee of Anatomy and Physiology even was put aside!

Need I add that, though I have been a Fellow of the Royal Society during nearly seventeen years, I have never been placed on Council, or Committee, or appointed to read a Croonian Lecture?

But I proceed with my detail. I will now state what my discovery really is. If I prove it to be great, proportionally great will be the dishonour to the Royal Society of such proceedings as I have described. On the other hand, has either Secretary detected a new principle, or one new fact in science? I leave the adjudication of these questions to the Fellows of the Royal Society at large, to the Members of my own Profession, to Foreigners, and to another day.

I propose to measure the importance of my discovery by its novelty, its extent, and its value to the art of Medicine and to mankind. I will venture to assert that nothing comparable to it, in all these points of view, is to be found in the pages of the *Philosophical Transactions*, or even in the *History of Physiology*. I assert this advisedly, for it is *truth*.

I shall take my account from my published works. It *ought* to be compiled from the volumes whose title I have just quoted.

There has long existed, in works on physiology, the notice and detail of certain experimental facts:

First—if the denuded spinal marrow, or a denuded muscular nerve, be irritated by a probe or the forceps, the muscles



supplied by these are seen to contract. This property in the spinal marrow or nerve has been designated the *vis nervosa* ;

Secondly—if, as in the case of irritation of the tip of the severed tail of the triton already mentioned, any part of the cutaneous surface of a decapitated animal be irritated, certain movements are again observed.

The names of Haller and of Whytt are chiefly attached to these *experiments*. They remained as mere sterile, unconnected, unapplied *facts*. No physiologist had ever imagined that the property called into action in *both* cases was *one and the same*. Far less had any physiologist pointed out any application of *either* to *physiology*—to the *functions* of life.

In addition to these experimental facts, it was also known to all, that if certain surfaces of the animal body were irritated, certain movements follow : irritation of the nostril, the fauces, the larynx, induces sneezing, vomiting, coughing, respectively. Still no physiologist had imagined that the motor principle in these *pathological actions* had any application to *physiology*, or any part in any other than these very obvious “sympathetic actions,” as they have been called.

Any one of these facts *might* have suggested *The Theory of the Spinal System* ; but they never *did* suggest that *Theory* or that *System*.

It was asserted by Haller, and *all* other physiologists, to Prof. Müller inclusive, that the *vis nervosa* only acts in *one* direction,—that *from* the point irritated *towards* the muscles. So long as this opinion prevailed, to identify this motor power with that in the experimental facts, and that in the sympathetic actions to which I have adverted, was impossible.

I first detected the important *Law of Reflex Action* of the *vis nervosa*. From this moment the application of this motor power to explain the experimental facts, the sympathetic actions,—and a certain Class of the vital functions, became practicable ! I consequently consider this detection of the reflex mode of action of the *vis nervosa* as a most important link in the chain of my discovery.

So much for the *Dynamic*, or motor principle ; I now come to the *Anatomy* of the spinal system.



The *idea* of an Incident Nerve, in *essential* relation through the Spinal Marrow with certain Reflex muscular Nerves, did not exist in Anatomy.

The *idea* that the pneumogastric nerve exists in this essential relation through the medulla oblongata with the diaphragmatic and the intercostal nerves—the *idea*, and the fact—are equally new in Anatomical Science

The *idea* that there is a *System* of such incident nerves in essential relation through the spinal marrow with muscular nerves, is, I repeat, new in Anatomy.

Yet the detection of all this has resulted from my investigation! May I not ask—Is there any thing like this in the pages of the *Philosophical Transactions*, or in the history of anatomy?

I now come to *Physiology*. The *idea* of a Reflex Function of the Spinal Marrow; the *idea* of a Reflex *physiological* act did not exist in Physiology!

Yet the *whole Class* of the physiological acts, or of the *functions*, of Ingestion and of Egestion, in the animal œconomy is of this character!

The act of deglutition is a Reflex act, accomplished through the medium of an incident nerve in essential relation with the spinal marrow and with certain muscular nerves, by the agency of the *vis nervosa*, according to its Reflex law of action! Motor power, anatomy, physiology, *all* have resulted from my investigation.

The same remarks may be made in regard to the acts of *Respiration*, which are now understood *for the first time*.

The same remarks relate to *all* those acts which conduce to the preservation of the individual and the perpetuation of the race.

1. *A Reflex action of the vis nervosa;*
2. *A Reflex Anatomy consisting of*
  1. *Incident, Excitor,*
  2. *Reflex, Motor, Nerves, in essential relation with*
  3. *The Spinal Centre;*



3. *A Reflex Function of the*1. *Medulla Oblongata, and*2. *Medulla Spinalis ;*4. *A Reflex Spinal Physiology in all the*1. *Acts*1. *Of Ingestion, and*2. *Of Egestion, and especially in*2. *The Acts of Respiration ; &c. &c.*

These, as far as the Royal Society is concerned, are the results of my discovery and labours.

Again then I ask—Is there any thing so original and so extensive, so vast, in every sense of the word, in the *Transactions* of the Royal Society, or in physiological history ?

Then surely I may also ask—What is the meaning of the conduct of the Royal Society towards one of its Fellows, who has worked so perseveringly, so laboriously, and so successfully ?

My Lord, I ask you—Is such conduct as this to continue for ever ? Allow me to call your Lordship's attention to the Plates contained in the "New Memoir," which I herewith transmit to you, and to ask—Is there any thing like them in *Physiological and Pathological drawings* ?

I must however proceed. I beg your Lordship to ask your own physician, whether I have not done much for the *Pathology* and for the *Therapeutics* of this department of medicine,—pointed out the way for the study of *Obstetrics*,—and placed in the hands of the Physician and the Surgeon, the means of the *DIAGNOSIS* of the Diseases of the Nervous System. In one thing, indeed, I have been fortunate. I have not only made an important scientific discovery, but I have lived to show its utility in the art of Medicine, and therefore in the mitigation of human suffering ; events which rarely happen to the same individual.

But I should write a volume if I were to treat of all these questions—such is the extent of my subject ; and I fear lest I should exhaust your Lordship's patience. Yet I have one fact to add. Believing that "envy, hatred, and malice" in



the Royal Society had worn themselves out, during the long term of ten years between 1837 and 1847, I again presented a paper in the latter year. It was a paper containing the detail of many experiments of the utmost difficulty and nicety; experiments relating to the influence of galvanism, first, on muscular nerves; secondly, on the spinal marrow; and thirdly, on incident nerves; experiments, two thirds of which, I believe, no one *could* perform but myself, and which no one certainly ever had performed; experiments of great value to pathology, but of which my judges do and can know nothing; experiments, finally, of which no one could judge without witnessing them. Yet were these experiments rejected *unwitnessed*!

One referee and reporter has since confessed that he misunderstood my paper, and this even as to its principal term, electrogenic. One fact in particular I withhold with the utmost difficulty; but I do and will withhold it, except from your Lordship's private ear.

My task is finished. The honour of the Royal Society, of your Lordship, of the Council, of the Fellows at large, of those who *have* received its honours especially, is deeply involved in the statements I have made.

The wrong done to me is also a wrong done to Anatomy and Physiology; and rumours are now afloat of an attempt to remove this exalted science from the Royal Society altogether! This discredit, this injury, this meditated treason, we owe to such deeds and such doers of them as I have exposed in this letter. The original founders of the Royal Society were chiefly physicians, who have ever been ardent in the pursuit of "natural knowledge." It is only recently that they have, in the Royal Society, brought discredit on their profession, their science, and their name.

I can only conclude with one expression, that of my deep and sincere regret that I *ever* submitted my labours for adjudication by the Council of the Royal Society of London!

Finally—Why do I address your Lordship? Is it that I expect or even wish for any reparation of these wrongs in my own case? No! But I wish to leave these facts on *record*, to



deter future Secretaries and future Councils and Committees of the Royal Society from such evil doings. And if I had not compounded for all my payments at the Royal Society, this letter should close with my resignation of its Fellowship.

I will occupy your Lordship's time with one word more. I do not complain that I am *without* my reward; for that would be unjust. I am most amply rewarded in the manner in which every physician likes to be rewarded. But I complain that my reward has not flowed from the Royal Society; for I can say, in the words of an eloquent predecessor of your Lordship in the chair of that Society—"To me there never has been a higher source of honour or distinction, than that connected with advances in science\*."

Your Lordship will readily imagine how different my position and career would have been at the Royal Society, and elsewhere, had honour and justice prevailed there; and that the necessity which I *feel*, after so many years of labour, for writing such a letter as this, full as it necessarily must be of egotism, is, of itself, no slight injury.

With many apologies for this intrusion,

I have the honour to be,

MY LORD,

Your Lordship's obedient Servant,

MARSHALL HALL.

14, Manchester Square,  
Oct. 28, 1848.

\* The Last Days of a Philosopher, ed. 1, p. 225.

## POSTSCRIPT.

It has occurred to me as proper to point out, in this Postscript, one or two changes which I deem essential for the restoration of peace and honour at the Royal Society.

1. In the first place, I believe there will be no peace at the Royal Society, whilst there are *secret Reports* on papers presented to it;

2. In the second place, I believe, there will be no justice, and consequently no satisfaction, whilst papers containing the description of dissections, and, still more, the detail of novel and delicate experiments, are judged without those dissections and those experiments being most carefully, and perhaps repeatedly, *witnessed*;

3. There can be no peace, confidence, or honour at the Royal Society, whilst those innovations and foci of cliqueism, termed Committees, exist, and prey, as they do, like cancerous growths, upon its frame and constitution.

4. One thing is most important; that, in any case of supposed wrong, the President, and the Council, of the Royal Society be accessible to the complainant; and that he be treated with civility and consideration.





