at a cursory view be imagined. Much unhappiness has its source in such a cause; and I conceive it to be the duty of any practitioner who has been so fortunate as to rescue one victim, whose case was deemed hopeless, to bring such case in some way before the notice of his professional brethren.

Prurigo.

CASE 5.—Mr. C——, aged forty-six, of a highly nervous and irritable temperament, consulted me for a most distressing pruritus around the verge of the anus, extending also along the perinæum to the base of the scrotum, but chiefly affecting the former locality. Had tried many remedies in vain, and was now quite desponding, and worn out by the unceasing irritation. The only application from which he had ever derived the least benefit (and that but temporary) was a lotion of hydrocyanic acid. Had been latterly obliged to take opiates at night, the torment being much aggravated in the nocturnal season. The integument of the affected part was inflamed, tender, and covered with minute papulæ. There was also considerable excoriation, from manual interference, resorted to in the vain hope of allaying the insupportable irritation. There was no disorder connected with the alimentary canal. He was treated with the arsenic, in the usual dose, which proved rapidly efficacious; and in seven weeks was entirely rid of his plague.

This case is strongly demonstrative of the power of arsenic over this horrid form of papular eruption, which often proves intractable by any other known remedy. Although, perhaps, the most interesting, this is by no means the only case of a parallel nature and intensity I have satisfactorily treated with arsenic.

Sycosis.

CASE 6.—G. W——, a gentleman, thirty-four years of age, consulted me some months since, having suffered severely from this distressing complaint for a period of five years. Had tried an immense number of ointments and soaps without permanent benefit. Thought he derived temporary advantage from the application of a leech under the chin occasionally. He presented a most disagreeable, not to say disgusting appearance; the chin being scabbed over. Finding his general health excellent, I prescribed him the solution of arsenite of potass, four minims to be taken in porter three times a day. One month sufficed for his cure, since which there has been no re-appearance of the pustules.

The eyes were but very slightly affected.

Acne Rosacea.

CASE 7.—Emma B——, aged twenty, waitress, came to me presenting a well-marked case of the above disease, from which she had suffered for two years and a half. The pustules were small, so as almost to induce a suspicion of a syphilitic origin, clustered, very acuminated, and having a rosy inflamed base, occupying the apex of the nose, together with the whole of the left ala, and a greater part of the right. There was also a large patch on the left cheek, but none in any other situation. She said they had been at first very irritating, but latterly had ceased to cause much annoyance. She was, however, extremely anxious to get rid of such a disfiguring "humour," as she termed it. I found on inquiry, that she had not menstruated regularly for some years, the catamenia being sometimes excessive and attended with dysmenorrhœa, but generally scanty, and with slight fluor albus. The system at large, however, did not appear to be deranged thereby, and the natural functions were well performed. But conceiving that the local inflammatory action might depend upon the uterine or ovarian derangement, I endeavoured to restore the suspended function, and in course of time succeeded so far. She now menstruated regularly and normally. Still the acne was as bad as ever. I now gave her the arsenical solution in full doses, but was quickly obliged to reduce the quantity, her system proving unusually susceptible to the influence of the drug. She persisted in its use for about nine weeks, and was perfectly cured.

I have transcribed the above case, chiefly from its being corroborative of Mr. Hunt's opinion, (expressed in his work,) that the supposition that acne of this kind, in young females, is commonly dependent on disordered menstruation, is erroneous. That the abnormal condition of the periodic function had no influence over the cutaneous disease, in this instance, at all events, may, I think, safely be inferred, from the fact of the persistence of the latter, when the former had been restored to a healthy state.

Rupia.

CASE 8.—Emma B——, aged 11 years, was brought to me with well-marked rupia, covering both legs, the anterior aspect

of both thighs, fore-arms, and wrists. The trunk was free. This poor child had suffered from the disease ever since she was two years old, (supervening on scarlatina,) and it had proved intractable in the hands of numerous surgeons. The irritation about the tubercles, which were very large in their earlier stage, was great, and as often as the scabs disappeared they were constantly succeeded by fresh eruptions. She had a universally faded, unhealthy look, and cachectic appearance, and I had strong suspicions of syphilitic taint being the foundation of her malady. I gathered, however, from her parents' account, that mercurial alteratives had been tried, again and again, to no purpose. I gave her at first iodine, with sarsaparilla, and afterwards sesquioxide of iron, and the mineral acids. Finding, after several months' trial, that I could not, by the aid of these tonics, make any impression on the complaint, or cause any apparent change in the local diseased action, I exhibited the solution of the arsenite of potass, which began to show its effect in three or four weeks. Continuing its use for five months, with the occasional use of a mild aperient, the case resulted in a complete cure. When I saw her last, there was only a slight redness over the thighs and legs.

The conjunctivæ were early and somewhat severely affected.

I deem the above case one of the most satisfactory and interesting I have as yet met with.

Kennington, 1849.

A SUPPLEMENT TO AN ESSAY ON THE NERVOUS SYSTEM, BY ALEXANDER WALKER.

THE following observations are supplementary to the five consecutive articles which appeared in the 22nd, 24th, 25th, 26th, and 27th numbers of THE LANCET for 1848, entitled, "An Essay on the Physiology of the Nervous System," by Alexander Walker, Esq., setting forth the author's claims to the discovery of the great principle, on which it is now admitted that all sound neurological investigations must be carried on.

At the beginning of the present century, physiological science, so far as regards the nervous system, was, beyond all doubt, in a state of inexplicable confusion. Physiologists were then only groping their way amidst the arcana of a very complex, but withal most important subject. Of course, there was a certain terminus *ad quem*, which all wished to reach, but the terminus *a quo*, from which all should have started, had never been ascertained, and every individual inquirer laid hold of the first leading-string that came in his way, thoughtit landed him only amidst a prodigious variety of cross-roads, which left him in the painful uncertainty whether to turn to the right hand or to the left.

In this state of uncertainty, it was suggested that there might be an action of the nervous system analogous to the circulation of the blood, the problem which Harvey solved, by proving that there was a centripetal and centrifugal flow of the vital fluid, the centre being the heart. It was asserted that there was an analogous centripetal and centrifugal action of the nerves, the centre being the brain. In short, it was asserted that the nerves which had their origin in the anterior roots and columns of the spinal marrow, and the nerves which had their origin in the posterior roots, were "sensor and motor" respectively; that the action of the former class was ascending to the brain, the action of the latter descending from it, never traversing each other, or moving in opposite direction.

It is obvious, if this were true, that this was an important discovery, that a terminus *a quo* had at least been ascertained. It is invidious and unscientific to elevate one branch of physiological investigation above another. A true theory of the circulation of the blood may be just as important as a true theory of the action of the nerves, but not more so. Yet, the elements of the problem in one case may be gross and palpable, the elements in the other case may be complex and impalpable. This is emphatically true with regard to the two theories in question.

The action of the blood is a question of pure physiology, the elements of which are gross and palpable. The action of the nerves is also, at the outset, a question of physiology, but its ultimate destination is amidst the arcana of our mental operations; it lands us in that hitherto unexplored region, the connexion between matter and mind; and, just as the latter is a higher and infinitely more delicate subject of investigation than the former, so is the true theory of the nervous system a higher and more delicate speculation than that of the circulation of the blood. It must have been on this ground that Sir W.

Hamilton, the *facile princeps* of modern philosophers, pronounced the discovery alluded to, "a prolific doctrine."

But when, where, and by whom was the doctrine first promulgated? We trust we can furnish an answer to these questions, and that, too, on evidence which must carry conviction to every unprejudiced mind.

The assertion of the duplex function of the nerves (sensor and motor) is first to be met with in two papers—one entitled, "General Physiology of the Intellectual Organs," and the other, "New Anatomy and Physiology of the Brain in Particular, and of the Nervous System in General," published in the second and third volumes of the "Archives of Universal Science," by Alexander Walker, in 1809.

These two papers, had the author never written anything more, would have established his claim to an original, a profound, a clear, and most acute thinker.

The doctrine in question was first, in print, stated in 1809. Mr. Walker again repeated his theory in "Thomson's Annals of Philosophy," in 1815. Again, in his work on the "Nervous System," in 1834; and again, in "Documents and Dates of Modern Discoveries in the Nervous System," in 1839.

So stands the question with regard to dates; and before quoting any authorities in support of them, let us see how the question of time stands in regard to Sir C. Bell.

It was not till 1811 that Sir C. Bell published a single word on the subject. Then came out his short "Essay on a New Anatomy of the Brain," under the auspices of his relative, Mr. Shaw, without a date; but, strange to say, Mr. Shaw afterwards assigned to it the date of 1809! This date was copied into the reviews and papers of the day, and was consequently quoted by Seabert and other foreign writers.

This is a painful subject. What induced the falsification of the date? What induced Sir C. Bell to allow it? That he was ignorant of it, it is impossible to believe. The mind cannot resist the painful impression that it was deliberately, intentionally, and wilfully tolerated. Else why so long in correcting it? True, the printer stated that it was printed in 1811; but Sir Charles himself never corrected it till the publication of his work in 1830—a period of nineteen years, during which he was evidently sailing under false colours, and allowing to be attributed to himself the merit due to another.

Be this as it may, the charge, grave and solemn though it be, has been publicly made again and again; and it yet remains with those who adopt Sir C. Bell's views, or who may consider themselves the guardians of his fame, to dispose of the allegation. "We see," says the *Spectator*, "Sir C. Bell tacitly countenancing, for a long time, the mis-statement of Shaw and others, by which the appearance of his Essay on the Brain was carried back from 1811 to 1809, the precise period of Mr. Walker's publications; and a paper published before Majendie's experiments has been silently altered since! So it is, 'grand anatomiste! grand voleur!'"

The following brief but decisive authorities bear on the question of time, as well as on the importance of the subject.

The first is from Professor Lizars. In a letter to Mr. Walker, dated May, 1834, he states: "I distinctly recollect that it was in 1807 you commenced those dissections of the brain in which I assisted you; and that you delivered their results in your Lectures on Anatomy and Physiology, and published them in the volume for July, 1809, of the *Archives of Universal Science*."

Burdach and Tiedemann's dates were some years after: Burdach's was 1819; and Tiedemann's, 1816.

The next is from M. Longet, in his "Système Nerveux," 1842. Speaking of the duplex function of the Nerves, he styles it, "la plus belle découverte physiologique des temps modernes." And again: "Walker est le premier physiologiste qui ait en l'ingénieuse pensée d'attribuer des fonctions distinctes à les deux sortes de racines." And again, in asserting that there was not the slightest ground for supposing that the idea had occurred to Boerhaave, he states: "Ce fut seulement en 1809 qu' Alexandre Walker émit cette idée ingénieuse."

The next is from Sir W. Hamilton. In a letter, dated 1847, Sir William states: "He (Mr. Walker) is the author of various works on physiology, all of which manifest great ingenuity; and he is (I speak after a careful investigation of the relative documents) undoubtedly the first promulgator of the prolific doctrine that the two spinal roots of the nerves correspond to the twofold variety of function (sensibility and motion) which these nerves perform."

The next is from Dr. Robert Knox, who, having been one of Mr. Walker's first pupils, ought to be a good authority on this subject.

In a letter dated 1847, after expressing his entire agreement

with everything Sir W. Hamilton had expressed, he says, "But I feel inclined to go further than either Sir William or the very candid M. Longet, who has also spoken highly in your praise: I consider you as the real discoverer of the true system of the nerves; posterity will, I feel confident, award this verdict in your favour, when party feeling shall have ceased."

Professor Sharpey, in a letter, (1840,) thus expresses himself—"I have this year, in my lectures, endeavoured to do justice to your claim of priority in assigning separate functions to the anterior and posterior roots of these nerves, without which idea, wherever it originated, not a step could have been made in the experimental inquiry."

Dr. Hunter, lecturer on anatomy, in a letter, (1847,) states—"I have also to add, after a careful investigation of the point, that there is no doubt on my mind that you have the honour of being the discoverer of the duplex function of the roots of the spinal nerves."

These dates and these authorities put the question of the true discoverer beyond a doubt. The next question is—What is the value of the discovery, if followed out to all its ultimate results? This question is partly answered in some of the authorities already quoted. Dr. Henry, in a paper read before the British Association, regards it as "doubtless the most important accession to physiological knowledge since the time of Harvey."

In addition to this, we shall afterwards see that the discovery is "prolific" of higher truths than those which occupy the attention of mere physiologists.

The nerves are the telegraphic wires of the human system, and give their report with infinitely more rapidity than even the modern telegraph. Mr. Walker was the first to discover, then, that there was an up and down train, and it is a strong presumptive proof that Sir Charles Bell was ignorant in a great measure that there were two trains at all; seeing, in following out his theory, that he starts with the down train, when he should have taken the up. He inverts Mr. Walker's doctrine, and pursues his investigation on the ground that the posterior nerves are the sensor.

The *terminus-a-quo* being ascertained, the "prolific doctrine" stated,—the "idée ingénieuse," published by Mr. Walker,—the one physiologist turns to the right hand, the other to the left.

We have proved, beyond all doubt, that Mr. Walker was the first to ascertain the true starting point. Does the inquiry, then, become so complex, delicate, and inexplicable, as to degenerate at once into a subject of doubtful controversy? It does become delicate. The inquiry, as we have already observed, lands us amidst the arcana of our mental operations. It is because Mr. Walker was aware of this that his physiological investigations have become so valuable. It was, however, valuable. "Not a step could have been taken without it, in the experimental inquiry." (Sharpey.) But it is in working out the idea that Mr. Walker's power and superiority are peculiarly felt. It is obvious that a correct physiology of the nervous system has a direct bearing on mental physiology. Mr. Walker seems to have been thoroughly aware of this. All his physiological speculations have a direct bearing on the philosophy of the mind. He is not a mere matter-of-fact physiologist; he is a profound mental philosopher; and pursuing the analogy between the two sciences, he has laid a more secure basis for our mental speculations than any of his predecessors. It has been rightly said, that every moral philosopher should be an anatomist and physiologist; and it may be asserted with equal truth, that every physiologist should be a moral philosopher. A rigid analysis of the nervous physiology is the true ground on which to commence our investigations in psychology. Both branches ought to be studied together: they are connected much in the same way as "corollaries with the propositions whence they flow."

Now from the analogous style of Nature's working in all her processes, it is obvious that in two branches so connected as those under consideration, a correct theory in one will help to a correct theory in the other; the anterior processes of the physiological department being cleared up when the ulterior results of the psychological department have been reached; while the latter, again, may have a flood of light shed over them by a clear understanding of the former.

Here, as in all her processes, Nature's style is, so to speak, "a complex simplicity," the accomplishment of a prodigious variety of ends, by the operation of a single law. It is from overlooking this great principle that so much confusion has ensued in this and in other departments of science. The phrenologists overlooked it when they proceeded from such a variety of arbitrary starting points. Instead of proceeding from the simple to the complex, they proceeded from complexity to complexity, and the result has been, so far as they are con-

cerned, that mental science has been left just in the same state of confusion in which they found it.

Mr. Walker's mode of proceeding is the reverse of this. He proceeds from the simple to the complex, knowing that sensation is the proper inlet to all our perceptions; that the senses lie in the front of the human subject, and that the four senses of taste, seeing, smelling, and hearing, may be considered only modifications of the sense of touch. He searches from the origin or roots of the nerves of this sense, which are the mere telegraphic wires of the human system, and he finds them in front of the spinal column. He asserts, here then I have laid hold of the sensitive conductors; they give their report in the cerebrum, where they are intelligibly translated, and form an intellectual perception. But here he has only solved the half of the problem, though there still remains the half—the posterior section of the human subject—as an unexplored region of physiological investigation, just as there remains in the psychological department of phenomena, volition and action to be accounted for.

The question then is,—How does perception pass onwards to volition, motion, action? Clinging to his principle, Mr. Walker proceeds in search of another class of nerves, and he finds their roots in the back of the spinal column. Here again he asserts, I have laid hold of the motor conductors; they give their report in the cerebellum, where they are intelligibly translated into volition, motion, action, passing from the purely physiological region into that of psychology.

It is obvious from this simple and beautiful theory, (and it is his from beginning to end,) that Mr. Walker was as much alive to the psychological importance of the investigation, as he was anxious to lay open at the roots, with the knife of the anatomist, the structure and physiology of the question. He is always sensitively alive to the mode of Nature's operations; he clings to the principle with all the tenacity of a man who feels himself right. He has clung to it for nearly half a century with undeviating constancy, and can look back on an inquiry which has occupied him for more than forty years, (when Sir C. Bell thought nothing of the subject,) without being able to accuse himself of a single false step in the investigation.

This, in itself, is a strong presumptive proof that his theory is right, and forms a strong *à priori* argument that it is so. The substantive proof remains to be quoted or referred to.

The theory, so far as it regards the physiology of the nerves, is complete and unbroken. With regard to its bearing on psychology, it is right also, so far as it goes. Let us hope Mr. Walker will be enabled to complete his speculations on this part of the subject. He has subscribed to, and admirably illustrated, the old adage, "*Nihil in intellectu quod non prius in sensu.*" Let him pursue his conclusions to their ultimate results, and then he will perceive the force of Leibnitz's addition, "*præter intellectum ipsum.*"

Sir Charles Bell again, for a long period, does not seem to have had any great and determinate principle to guide him. In all that he has published from 1811 till 1822, (see Wagner's "Physiology," with notes by Dr. Willis,) he seems to have been wavering from point to point, now on the right scent, now on the wrong; at one time following the true line of investigation, and at another led astray by some false trail drawn across his path.

This is a strong presumptive proof of the likelihood of his being wrong in the conclusion he came to in his work published in 1830—that the posterior nerves were the "sensor ones;" just the reverse of Mr. Walker's theory.

We have seen who was the first promulgator of the "prolific doctrine" of the duplex function of the nerves. Who, then, is right with regard to the subordinate inquiry? What are the functions which each class performs? Or, rather, is the anterior class sensor, with an action ascending towards the brain? Is the posterior class motor, with an action descending from the brain? Sir Charles Bell says, No—Mr. Walker says, Yes. The proof of this affirmative proposition would require us to repeat a great deal of the unanswerable reasoning in the papers to which the present article is supplemental. To them we must refer our readers for the great body of substantive proof. The mode on which Mr. Walker proceeds to the proof of his theory is as simple and scientific, as the theory itself is simple and beautiful. He rests his proof on the distinct grounds of anatomy, analogy, genesis, and pathology. Anatomy being the science of structure, physiology the science of functions and of powers, anatomy must decide the character and number of the elements of function—the character of the instruments of power. Any functional theory, which should assume some elements which did not exist, or overlook others which did, would be damaged at

once by the findings of anatomy. On the other hand, when the physiological theory takes nothing for granted but what the most accurate anatomical dissection warrants, then it receives the testimony of anatomy for the soundness of its foundation—this foundation Mr. Walker lays at the outset, and he illustrates his theory, as he advances, by a variety of analogous and cognate branches.

Analogy speaks distinctly in favour of the theory. There is a similarity in the style of Nature's working through all her multiplied and various processes; and it is a strong presumptive proof, that the theory of some hitherto unexplored process is correct, when it is supported by all the analogies derived from processes which have been thoroughly examined, and whose true theories have been ascertained. It is a beautiful principle that Nature loves to move in circles. We see the circulation of the sap in plants—the blood in animals—the element of water, from the ocean to the clouds—from the clouds to the hills—from the hills to the streams and rivers—that return to the ocean. And so, in Mr. Walker's theory, there is a somewhat analogous action in the system of the nerves.

But the arguments from the genesis and pathology of the system are perhaps the strongest; they are by far the most palpable, and ought of themselves to decide the question.

The argument founded on genesis is a question of development, or of the order and succession of formation. As soon as Nature has matured her materials, and is passing from the chaotic into the organic state; the anterior (sensor) columns are the first the anatomist can detect as assuming an organic form, and they are the first completed. Tiedemann's elaborate work on the Anatomy of the Brain, is a demonstration of this. The general conclusion to be drawn from the book is, "that in the cerebro-spinal system, the medullary columns, pyramids, &c., are formed before the posterior."

Now we all know what a stern economist of all her powers Nature is, and it is a striking fact, and one that bears with great force on the present question, that the anterior parts are not only first formed, but as soon as they assume an organic shape, they become agents in the completion of the posterior parts. The anterior (sensor) columns and nerves becoming sensible of irritation, find their appropriate action in pushing on the development of the posterior (motor) ones; the sensitive coming into play first, are not allowed to lie idle, but find their appropriate function in completing the formation and pushing into action the motive powers. The impulsive power lies in the sensor nerves; the first in the order of formation, and being exerted on the motor ones, forces them, if we may so speak, into action. They act, because they are put in motion from the impulsive power applied to them.

This genetic development, this gradual formation and successive growth proves decisive of Mr. Walker's theory at the outset, and that it is the true order of formation, the whole anatomical dissections and physiological investigations of Tiedemann and Burdach abundantly prove. They show, beyond a doubt, that the anterior (the sensor) are first formed, and this not only affords a sure and simple and intelligible base to Mr. Walker's theory, but harmonizes beautifully with the law of our higher mental operations. It is through sensation that the materials of our mental operations are gathered. Mr. Walker traces the conductors of these materials, till they reach the cerebrum, "the dome of thought." The conveying fibres concentrated there; and by that means, all impressions of touch, taste, smell, seeing, and hearing are compared; and not only these but all those from the vital system; so that all relations to mind and to life are compared and appreciated before the acts of the will (prepared by the cerebellum, which soon after joins them) are stamped upon them, and they obtain currency in the posterior columns, &c.

If there be a system of the nerves at all, it must be regulated by a uniform law, or two or more laws moving in harmony, analogous to the working of Nature in all her processes. And the fact that Sir C. Bell has overlooked this, is enough to damage his whole theory of nervous function. It is at least an unscientific, an unpardonable mistake. He not only reverses Mr. Walker's simple theory, and ascribes sensation to the posterior roots, contrary to the whole analogy of structure, and contrary to the analogy of our mental operations, but he ascribes action in opposite directions to the same fibrils. One and the same conductor is carrying a report up, and at the same instant carrying a report down. Now this is not Nature's mode in any of her processes. She never plays at cross purposes. There are no collisions in her trains. Like the God of Nature, whose rule is order, Nature is the mother of order.

It is unnecessary to say a single word as to the bearing of

this argument on Mr. Walker's theory. The original distinction between the two classes of roots being made, it proves decisive of the true functional theory at the outset, though in direct opposition to Sir C. Bell. But the argument is confirmed, as the investigation proceeds, by the no less decisive argument from pathology. That argument may be stated in a few words. The total, or even partial destruction of the posterior columns must be followed by one or the other of these results, according as the theory of Mr. Walker, or that of Magendie, Bell, and Müller, is adopted. If the former, then the impairment of the motor powers must be the necessary result; if the latter, then the impairment of the sensitive organs must be the necessary result. This is a problem for pathology to solve. The elements of solution are in its hand, and if it return an adverse response, Mr. Walker is willing to give up the question. What, then, does pathology say on the subject? We must appeal to cases recorded of disease of the posterior columns. When this is the case, what is the result? According to Magendie, Bell, and Müller, the loss of sensation ought to succeed.

But Todd and Bowman, both eminent physiologists, affirm that cases are on record, which prove "that perfect sensibility is compatible with total destruction of the posterior columns"—a thing which could not be if these columns were the media of sensation; and cases recorded by Dr. Webster, Mr. Stanley, Dr. Budd, and M. Rullier, &c., prove decisive of the question; they prove that the destruction of the posterior columns results in "the loss of motive power, while sensibility remains perfectly intact." A detailed account of four of these cases is given in the 26th and 27th numbers of *THE LANCET*, (December, 1848,) and to these we must refer the reader.

We have thus proved beyond a doubt, that Mr. Walker was the "first promulgator" (Sir W. Hamilton) of the theory; that he stated it, in 1809, in the *Archives*, in language as definite and precise as any that has been used in reference to it since; that for forty years he has been illustrating and proving it, on principles of rigid induction. If he be right, let him have the honour and credit of it; if wrong, let him be proved to be so, by an induction as severe and rigid as he has employed in establishing it.

Let it not be said that the theory in question was a mere suggestion on Mr. Walker's part; that those who came after him proved it, and that Paley's doctrine, "not he who states the theory, but he who proves it, is the true discoverer," is strictly applicable here.

In the first place, Mr. Walker's is not a mere suggestion; it is a plain, palpable, and positive statement of a doctrine, the proof of which has been gradually accumulating since the hour it was made.

In the second place, we may deny the correctness of Paley's assertion, whether applied to this or any other philosophical discovery. When all are in bewilderment, he who suggests the right way of deliverance is entitled to a higher credit than he who betakes himself to it as the only chance of his safety. Without the original suggestion, the subject would have remained a mystery for ever, and all the demonstrations of logic would have been useless because there was nothing to prove. The suggestion may be what original and lofty genius only could have made,—the proof may be what any ordinary mind could achieve. The original suggestion may be the offspring of genius alone,—the proof may be the result of common industry.

We have, we trust, established Mr. Walker's claim to the original statement, and have at least indicated the line he has pursued in proving his theory. Whatever difference of opinion may exist amongst physiologists respecting some of the minor details, there can be but one opinion as to the service Mr. Walker has rendered to physiological science. He pointed out the right starting-point—the true line of investigation—when all were in bewilderment. He never ceased to point onwards, in the direction his original distinction naturally indicated; and he has never swerved from his principle, which is borne out and illustrated by the state in which Nature works in all her analogous processes.

It is curious to contrast the wavering, the absolute groping, of Sir C. Bell, with this undeviating consistency,—during, at least, the whole early period of his investigation. Professor Whewell states: "Sir C. Bell appears at first to have pursued his researches with reference, *not* to this distinction, (the duplex function,) but to others. In his "Idea of a New Anatomy of the Brain," (1811,) he endeavoured to establish the opinion, that different endowments are in the same cord of the spinal nerves, and held by the same sheath; and the nature of this difference was, that both sensation and volition belonged to the anterior origin of the nerves, and another function to the

posterior origin. Yet along with this he appears to have entertained as a conjecture the doctrine now in question." In a letter, (published in Dr. Cook's work on Palsy, 1821,) he says: "The nerves of sensation and motion are bound together in the same membranes for convenience of distribution; but there is reason to conclude they are distinct throughout their whole course."

"But notwithstanding this conjecture, in his memoir of 1821 and 1822, the reference of his researches is not to this difference of sensation and motion, but to another view, resembling his original one. His main point in these memoirs is, that certain nerves are nerves of sensation and volition, and that certain others are "respiratory nerves. Thus, *Phil. Trans.* 1821, the fifth nerve, the nerve of mastication; in 1822, this (the respiratory) system of nerves is superadded to that of mere feeling and agency."

But we have done for the present with Sir Charles Bell. We have been compelled to repeat these statements, in order that justice should be done to the claims of another, whose profound and original speculations have rendered physiological science everlastingly his debtor. We have already alluded to the style and character of these investigations published in the *Archives*. But these two papers, important though they are, form only a tithe of Mr. Walker's scientific labours. There are several papers, in these volumes, of the very highest scientific value. In the departments of General Physics, the two papers, a criticism on Bacon's arrangement of the sciences, and a new arrangement proposed—the other paper entitled "General Physical Hypotheses," are the product of a fine, philosophical genius.

In the department of Metaphysics and Morals, the papers on Hume, Berkley, Godwin, and Boscovich, &c., are characterized throughout by great philosophical power.

In the departments of Criticism and Art, there are some profound and original principles laid down, and illustrated with great eloquence and force, by a reference to a series of the most distinguished writers and artists.

We have again, at the risk of repetition, alluded to these papers: they are certainly the most profound, though generally the least known, of Mr. Walker's original and multifarious speculations. Though not subscribing to all his conclusions, yet the papers, as specimens of philosophical and literary genius, would well repay their republication in a somewhat less repulsive form than that in which they are cast at present. If this were done, it would be seen what vile liberties some men have taken with what was the actual product of another's genius. Mr. Walker is one of the most likely of all men to be dealt with in this way. Many of his singularly original and bold speculations are but the first day's march on the line of investigation, perhaps sometimes not more than fixing (but fixing with severe accuracy) the point at which the investigation should commence. Of him it may truly be said, "that he has sown, and others have entered into his labours."

This is one reason why Mr. Walker, after nearly fifty years' scientific labour, is not reaping the fruits of his toil. Nor is he to blame for leaving some of his finest speculations unfinished. The pressure of external circumstances—the necessities of his condition—the burden of every day's life—have all told with a power so crushing, as to render it impossible for him to do so. That he was equal to the task is abundantly evident from the several treatises he has completed; witness his works on "Beauty," "Intermarriage," &c. And this is just the very reason why an effort should be made to relieve him, and place him in a condition that would enable him to complete some others of those speculations he has so ably begun—viz., such a work as that on Demonstrative Morality, on which he has long been labouring, but which has been brought effectively to a stand still.

A short time ago, a generous and touching appeal was made by *THE LANCET* to the medical profession on Mr. Walker's behalf. That appeal was responded to with all the ready and warm-hearted generosity which has so long characterized the profession; and one or two of its leading members, to their everlasting honour, offered to bring a statement of Mr. Walker's circumstances and claims under the notice of the Minister at the head of the Government. This is the proper course, as already suggested by *THE LANCET*.

It is a mark of the progress of society, of the advancement of science and of civilization, in a country where there is a growing tenderness and care for the unfortunate children of genius, and for those who have devoted their days, their years, their life, to scientific pursuits, especially when such may never have rewarded them, and when, but for public interference, they may be left in old age, to pine in solitude and

neglect, after a long life spent in painful and laborious investigations, which had for their sole object the amelioration, the improvement, the happiness of mankind. Genius, philosophy, literature, and science, in their various branches, are the things which shed over a people their national glory, and the nation which leaves to poverty and neglect any one of their most successful cultivators, will do that for which, afterwards, she will everlastingly reproach herself. Yet this is a thing of which all nations, at one period of their history, have been guilty. We hope this period has long gone by in the history of our own—a nation distinguished by so many noble and generous achievements, and which owes her wealth, her power, her glory, and renown, to the scientific industry and philosophic genius of her sons, will not remain deaf to the cry of one of the most distinguished of these, when that cry is raised in distress; especially a nation, at the head of whose government is a minister, himself one of the most distinguished cultivators of literature.

Seafield, Leith, 1849.

THOMAS LAING.

ON THE SUCCESSFUL APPLICATION OF A LIGATURE ON THE FEMORAL ARTERY,

IN A MAN SIXTY-SIX YEARS OF AGE, FOR A WOUND OF THE
ANTERIOR TIBIAL ARTERY.

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MR. W— was, at the period of the accident, sixty-six years of age, temperate, and of spare habit. He had for many years suffered much from large, ill-conditioned ulcers, situated a little above and behind the ankles of his left leg. On the 24th of April, 1844, he was standing on a short ladder, holding himself with the left hand, and with the right in the act of cutting off a branch from a tree with a sharp-pointed clasp-knife, when it slipped from the branch, and penetrated deeply in the soft parts at the upper and anterior part of the right leg, inflicting, in a direction from without, inwards, and backwards, a punctured wound, which bled most profusely. After arresting the flow of blood, and binding up the wound with his handkerchief, he was enabled, with much difficulty, to walk home, a distance of nearly half a mile. His wife, who had been accustomed to dress the sores on his other leg, applied some strips of adhesive plaster and a bandage, which prevented any further bleeding. I was requested to see him the following day. Finding the bandage tolerably well applied, and being informed that the edges of the wound had been brought well together, I thought it advisable not to remove the dressings, but ordered him to rest, and take opening medicine. On the 26th, the dressings being removed, a little bloody serum oozed from the centre of the wound, which was about an inch and a quarter in extent, painful, and a little inflamed; a rather severe return of the hæmorrhage the evening before had spontaneously ceased. With the intention of encouraging it, in order to ascertain its character, I made moderate pressure with the apices of the fingers in different places about the wound, but failed in inducing a return of the bleeding; strips of plaster and a thin bandage were loosely applied, and rest was enjoined. On the following day, suppuration had taken place; purulent matter and a little blood escaped from the middle of the wound, around which the skin was inflamed, puffy, and painful, conveying an indistinct sense of fluctuation. I therefore advised an extension of the opening, to let out the accumulated matter, which was forthwith made, and about two tablespoonfuls and a half of pus escaped, with some blood. A poultice was then applied; opening medicine to be taken in the morning.

On the 28th and 29th, bloody matter continued to escape, and on the 1st of May bleeding returned very freely, the blood being of a florid red colour. The purulent discharge less; the edges of the wound inflamed and swollen, and gaping a little, with general tumefaction.

On May 4th, I was hastily summoned to him, in consequence of a very copious return of bleeding from the wound; about ten or twelve ounces had been received in a basin, and were evidently arterial; considerably more than this had escaped on napkins and on the bedding; the old man seemed much exhausted, and quite unable to bear any further loss of blood. It was quite clear that some more effectual means must be resorted to in order to prevent another drain, which would most probably prove fatal in a system already so greatly reduced.

I advised him to have the opinion of Mr. Wright, the senior surgeon to the hospital; and in the course of an hour we met. Mr. Wright recommended that the femoral artery should be secured without loss of time; and having explained to the patient his precarious state, and the decision agreed upon, he at once consented; and I performed the operation at two o'clock in the afternoon, in the presence of Mr. Wright, Messrs. J. and J. N. Thompson, and others. It is not necessary to detail the different steps of the operation, farther than to state that two ligatures were applied; one corresponding to each extremity of the opening made in the sheath of the vessels, and one thread of each brought out of the wound in its centre, the rest of it being closed by strips of adhesive plaster. The patient was then put to bed, having borne the operation very well. The ligatures came away in eighteen days; and he progressed most favourably; the inflammation, pain, and swelling quickly subsided, and the excision healed up in the course of seven or eight days; in four weeks he was quite well.

The situation of the accidental wound was in the upper, anterior, and external part of the right leg, between the tibia and fibula, about two inches below and in front of the head of the latter bone, corresponding to the direction of the anterior tibial artery, immediately below where it passes forward through the interosseous ligament, and from between the bones; being covered by the extensor communis digitorum and tibialis anticus, and through the substance of the former, the knife had no doubt penetrated and pierced the artery soon after its exit from the above point.

Remarks.—It is extremely rare that the femoral artery has been tied for wounds of the anterior tibial. According to the tables of Hodgson and Lisfranc of the comparative frequency of aneurisms in the external arteries, those of the anterior tibial are extremely uncommon, not being more than three in 179. One case is related by Sir Astley Cooper, in which the femoral artery was tied; the swelling for a time subsided, but the case eventually did not succeed, sloughing of the aneurismal tumour took place. This distinguished surgeon has recommended in a case of aneurism of the anterior tibial, when situated low down in the limb, to tie the artery by opening the sac, and to secure it both above and below the aperture. In hastily glancing at the present case, it might, perhaps, have appeared reasonable in the first instance to extend the opening in the integument, dividing also the aponeurosis to the same extent, and after separating the muscles from below upwards, to secure the artery above and below the puncture. But there were several objections which might be urged against this practice. First, I may call to mind the difficulty, if not impossibility, of employing it, in consequence of the very limited space between the wound in the artery, and the point from which it issues from between the bones, increased also greatly by the very intimate connexion with it of the two accompanying veins, and the different points of attachment of the dense aponeurotic fascia in this situation. These remarks are fully corroborated by the notice of a case of wound of the anterior tibial, which was situated lower down, taken into St. Bartholomew's Hospital, and reported in the first vol. of THE LANCET for 1828-9, page 765.

Nor should I have been justified, at a later period, in opening the wound from which the hæmorrhage had ceased for thirty hours. There were doubts, also, as to whether the bleeding proceeded from a venous or arterial trunk, as I had no opportunity of seeing it, and was guided entirely by the account given by the patient or his wife, by no means satisfactory, which formed another objection to the adoption of this practice.

Had the inflammation been simply of the adhesive character, moderate pressure, with graduated compresses, and equal support to the whole limb, by bandaging from the toes upwards, with rest, might possibly have answered every purpose; but the high degree of inflammation and suppuration which ensued, not only in the aponeurotic fascia, but in the subjacent structures, prohibited its application; and to cut down upon and tie a wounded artery, during a state of inflammation, with the altered condition of the connecting tissues, would not have been attended with any beneficial result, but otherwise, as we are well aware secondary hæmorrhage would most probably have followed. My motive for applying two ligatures instead of one was, that I thought possibly in opening the sheath of the vessels, and in passing the aneurismal needle round the artery, a greater separation of the sheath might possibly have taken place than desirable, and this would render it additionally secure; in other respects one might be more satisfactory. After the operation there was but little appreciable fall in the temperature of the limb.

* Read before the Nottingham Medico-Chirurgical Society, Dec. 22, 1843.