and not many days passed over before a number of wounds assumed the same appearance."

I do not mean to infer that the disease is to be solely attributed to extremes either of heat or cold. Combined with these states of the atmosphere, cognizable to our senses, there must be other conditions with which we are unacquainted.

But if some general influence were at work, patients in other hospitals should have suffered from a similar affection. Such, in truth, was the case. Mr. Cæsar Hawkins has given an account of a number of cases of hospital gangrene, which occurred about the same time in St. George's Hospital. After relating the particulars of three cases, treated by himself, he adds,—

"There have been several other cases in the hospital under the care of my colleagues—namely, one under Mr. Keate, two under Mr. Cutler, two under Mr. Tatum, and one under the care of Mr. C. Johnson—making, with my own, nine similar cases in the last two or three weeks. Of my colleague's cases, the ulceration affected two stumps after amputation; two were cases of necrosis, which have not been able to be operated on, on account of it; one patient had a strumous ulcer in the nates; and another had an abscess in the axilla, and was nearly dying of the sloughing."*

It is worthy of remark, in connexion with the above related cases, that about the beginning of July of the present year, a gentleman, operated upon by Mr. Lawrence, in the west-end of London, for strangulated femoral hernia, suffered for several days from a condition of the wound similar to that which, from having been first witnessed in large institutions, has received the name of hospital gangrene. Under proper treatment, both general and local, the sore resumed a healthy character, and the patient recovered. Taking these various facts into consideration, it appears to me that the disease, as witnessed in St. Bartholomew's Hospital, cannot be regarded as endemic, but must be attributed to some atmospheric conditions, the exact nature of which we have no means of determining. The same opinion is entertained by my colleague, Mr. Holden, who watched the progress of the cases in conjunction with myself, and who gave me most valuable assistance in investigating and in arranging the facts which have been here collected.†

Original Papers.

ON THE

LAWS OF THE NERVOUS FORCE,

AND THE FUNCTION OF THE ROOTS OF THE SPINAL NERVES.

By THOMAS WILLIAMS, M.D. Lond.,

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A SHORT time ago it was my privilege to be present at the house of Dr. Marshall Hall whilst this eminent physiologist was engaged in demonstrating to a distinguished circle his elegantly-devised experiments. Several new applications of the *method* of experimenting adopted by Dr. Hall suggested themselves to my mind on that occasion. It was not, however, until my return, a few weeks since, after a long absence on the continent, that an opportunity occurred of making these new applications. If to the rare genius of Dr. Hall the products of my humble physiological "studies" should form the entrance upon a new tract of research, at some future time they may acquire an importance with which I cannot hope myself to invest them.‡

I have repeated the experiments, with invariable success, by which Dr. M. Hall has lately proved the power of the nerves to assume an electro-genic condition. I refrain here

from giving an account of the little galvanic instrument so ingeniously contrived by Dr. Hall, by means of which it is possible to develop an inconceivable minute amount of galvanic power. It consists simply, as Dr. Hall in another place has explained, of a small strip of zinc, having soldered to one end a silver wire, which is brought back upon the zinc in the form of the letter U. The nerve of the galvanoscopic frog of Matteucci, if thrown across the arms of the machine, will excite muscular contractions if the current from the positive electrode traverse the nerve centrifugally; whilst no muscular action occurs if the zinc pole be placed distally, and the galvanic force directed therefore centripetally along the nerve.

From the conditions of this experiment, it is obvious that the galvanic current itself does not pass beyond the silver electrode on which the nerve is lying; it is returned along the silver wire towards the positive source, the nerve serving merely the function of a string to connect the active and passive poles of the apparatus. But although, as Dr. Hall has so acutely proved, the galvanic current departs from the nerve at the point where it reposes in contact with the silver electrode, the nerve, notwithstanding, throughout its whole course, as far as the extremities of the toes, and therefore *beyond* the part on which *direct* influence of the galvanic excitant is exerted, assumes a new functional state, and the muscles of the leg are thrown into persistent contraction. This excited state of the nerve Dr. Hall has called the electro-genic; that produced by strychnia, being similar, he has called the strychnogenic. Electricians would describe it as an *induced*, or secondary condition. The suggestions arising from the *principle* of these beautiful demonstrations of Dr. Hall led me to perform several experiments, which I afterwards found had already been tried by Matteucci. I exposed the two sacral nerves of the frog, and connected them, whilst the animal was still living, by the galvanoscopic frog, without inducing in the muscles of the latter the least appearance of contraction. From the analogy suggested by the electro-genic facts established by Dr. Hall, it seemed to me a probable supposition, that although the muscles of the animal body may not be excited to contraction by an electric or some allied current, conducted by the nervous ramusculi by which they are actually traversed, it might yet result that some functional excitation of these remote muscular branches occurred, after the manner of the electro-genic or *induced* condition, sufficiently energetic to determine the contraction of the muscles.

a, section of spinal cord, showing the motor root at *m*, proceeding from the anterior cornu of its grey centre, and the sensitive root, *s*, entering at its posterior cornu.—*b* and *c* indicate a dotted circle, showing the route of the nervous influence, centrifugal in *m*, the motor root, or direct; centripetal in *s*, the sensitive root, or reverse.—*d*, ganglion on posterior roots of spinal nerves.—*e*, a compound nerve emerging from the ganglion, invaginated by a common neurilemma.—*f*, Matteucci's galvanoscopic frog thrown across the two roots.

The accompanying diagram exhibits the terms of an experiment which I have performed before the Scientific Society of the Royal Institution of South Wales. In this experiment the galvanoscopic frog is made to connect the anterior and posterior roots of the spinal nerves while yet within the theca

* Medical Gazette, June 11th, 1847, p. 1024.

† In South's "Translation of Chelius," there is an account given by Mr. Arnott of several cases of hospital gangrene which occurred in one of the women's wards of the Middlesex Hospital, in 1835.

‡ I cannot forbear, in this public manner, and on this first opportunity, to present to Dr. Hall the offerings of my deeply-felt gratitude for the important services which he has just rendered to the cause of practical medicine in his splendid essays "On the Convulsive Diseases of Children, and Epilepsy." A jubilee of thanksgiving ought to have arisen from all the lovers of scientific medicine in this country when they saw that at last retributive justice had sent forth the sentence of extinction against that corrupt enemy, (the British and Foreign Review,) from whom a poisoned stream of endless opposition had so long opposed the labours of a man who, for the last thirty years, has laboured with unquenchable zeal to destroy the trammels of error and mystery in which practical medicine was involved, and to the power of whose intellect the rising race of practitioners still looks up, with anticipation, for further light to guide their footsteps in the practice of a profession, of which so great part still remains in doubt and perplexity.

vertebralis, before, therefore, they converge in the ganglion. When the experiment is skilfully executed, distinct contractions take place in the galvanoscopic frog. It will be seen that this experiment is different in principle from that in which two compound cerebro-spinal nerves—i. e., the two sciatics—are connected by means of the galvanoscopic frog. In the latter case, if there be dynamic forces in the nerves at all approaching in character to the galvanic, they cannot impress the test nerve; for in each sciatic nerve thus electrically connected, there must reside two influences acting in diametrical opposition, and tending, therefore, to neutralize each other. The galvanoscopic frog *could* not manifest the least movement. During the repetition of Dr. Hall and Matteucci's experiments, the sources of fallacy and difficulty arising from the compound character of the spinal nerves presented themselves to my mind more and more palpably. The question irresistibly arose—How are these opposed, yet contiguous currents, to be examined singly and separately? The answer was immediate,—At the roots of the spinal nerves. The experiment is surrounded by great difficulties. The animal must retain its circulation and spinal excitability, or the nervous currents cannot be excited. The roots of the nerves are placed at an inaccessible depth, and surrounded by bone. The animal should be etherized, and the required conditions are fulfilled. I have succeeded in the rabbit and the dog. I have tried without success, at the slaughter-house, on the ox and the calf immediately after death. This experiment seems to me to involve the true theory of the ganglions on the posterior roots of the spinal nerves. They subserve simply the purpose of electrically uniting the two roots, in order that the current (as shown by the dotted circle in the figure) emitted along the motor root may immediately return back to the centre along the sentient root. The motor root thus carries the direct current, and the sentient the reverse, the motor root coinciding, therefore, with the positive electrode, and the sentient the negative, the ganglion constituting only a medium of galvanic union, the solder uniting the two poles. From this explanation it is manifest that the direct current travels along the conducting motor root only to the distance of about an inch and a half from the centre, at which point it departs from the course of the direct conductor, enters the ganglion, and returns by the sentient root. Then, all that tract of the motor nerve lying to the periphery of the ganglion, or between the ganglion and the extremities of the nerves in the muscles, cannot ever be the subject of any immediate influence from the current affecting the root. The condition of the nerve, whatever be its molecular nature, must therefore be an *induced* or *secondary* state, which immediately precedes, and occasions, the contraction of the muscles. The induction of the nerve is the result of the intense current operating on the root, or the extreme of the line. Let this explanation be reversed, it will become equally applicable to the sentient nerve; the reverse current proceeding from the ganglion, back towards the centre, exciting in the root such an intense polaric condition as to affect the whole extent of the nerve, the ganglion to the skin, with that *induced* change required for the fulfilment of its function. It is impossible at present to conceive the nature of the relation, if any exist, between the muscular electric currents of Matteucci and the force affecting the nerves.

Swansea, Nov. 12th, 1847.

NOTES ON

THE STATISTICS OF PLACENTA PRÆVIA.

EXPOSITION OF THE

ERRORS OF STATEMENT OF DR. LEE.

By J. Y. SIMPSON, M.D.,

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IN his last communication (see THE LANCET for Oct. 23rd, 1847) Dr. Lee has altogether omitted to notice the principal points and subjects more immediately bearing upon the subject of discussion—viz., the general and special amount of mortality in placenta prævia; while he has discoursed at no measured length about the claims of Dr. Rigby,—my degree of acquaintance with Portal's work,—and other matters not at all directly pertaining to the question. It is a well-known fact, that when the cuttlefish wishes to escape and hide itself, it empties its sepia or ink-bag, so as to darken the surrounding water, and cover its retreat. In his last paper Dr. Lee has scattered his ink upon the pages of THE LANCET with apparently a similar object. Let us, however, gently and quietly drag him back again, for a few minutes, into clear water, in order to see him properly, and understand aright

the exact state in which he has left the questions that he himself had originally the indiscretion to raise. The task is a disagreeable one; but it is the only way in which, for future truth, it is possible to expurgate the subject from present error and misrepresentation.

It will be remembered that, against my conclusions respecting 1. The general great mortality in placenta prævia—and 2. The special high mortality attendant upon turning in this complication—Dr. Lee argued that I had mis-stated the statistics of the cases reported by himself and others; that particularly his own cases were neither so numerous nor so fatal as I had shown them to be; and that I had overstated the deaths he had met with from turning. I adduced in THE LANCET for September 11th, 1847, ample evidence to prove that on one and all of these counts his assertions were utterly valueless, because utterly unfounded; and I showed that most of the alleged misrepresentations which he brought forward were committed by him, and not by me—existed on his part, and not on mine. On the present occasion I will briefly recapitulate the principal errors of statement on the subject in which Dr. Lee has formerly indulged himself; and then answer the two or three new arguments (?) of the same kind which he has brought forward.

The three first which I shall note refer to Dr. Lee's own cases and own practice.

First error of statement of Dr. Lee.—Dr. Lee has attempted to make the number of maternal deaths that occurred in his own practice, under turning in placenta prævia, statistically less than what was the fact, and than what I had stated it to be. For this purpose he has (since 1845) altered the returns of his seventh case, by latterly reporting the mother to have died "*before*" turning was performed, when twice previously (viz., in 1842 and 1844) he had declared her death not to take place till some days "*after*" delivery. (See full proofs in THE LANCET, Oct. 9th, 1847, p. 380, and Medical Gazette for Nov. 7th, 1845, p. 1193.) This grave accusation against the literary integrity of Dr. Lee—viz., his deliberately attempting to pervert his own reports, remains *unanswered by Dr. Lee*.

Second error of statement of Dr. Lee.—Dr. Lee has represented the number of maternal deaths that occurred in his own practice, under placenta prævia, as less than what was the fact, by altogether *omitting* another fatal case of this complication which he met with, and which was terminated, not by turning, but by craniotomy. (See Case No. 119 in his "Clinical Midwifery," or Case No. 35 in table of THE LANCET for Oct. 9th, 1847, p. 380.)—*Accusation unanswered by Dr. Lee*.

Third error of statement of Dr. Lee.—Dr. Lee endeavoured to make me in error by asserting that the total number of his own placental cases was less than I had represented them. In 1845 I stated them to be forty-six; he averred they were only forty-five,* and to gain apparent proof of this, Dr. Lee, in a table which he then gave, simply "*suppressed*" one of his own previously published cases. Latterly, in the same way, he stated his whole cases to be sixty-two, while they were sixty-four in number. (See my table of his cases,† in THE LANCET for Oct. 9th, 1847, p. 380, and Medical Gazette for 1845, p. 1145.)—*Unanswered by Dr. Lee*.

The three following errors of statements of Dr. Lee refer to the cases recorded, not by himself, but by others:—

Fourth error of statement of Dr. Lee.—"Dr. Ramsbotham" (says Dr. Lee) "has related nineteen cases of placental presentation, eight of which proved fatal."—(Lectures, p. 371.) In his chapter on unavoidable hæmorrhage, Dr. Ramsbotham has related, not nineteen, but twenty-one cases of that complication.—*Unanswered by Dr. Lee*.

Fifth error of statement of Dr. Lee.—Dr. Lee accused me of misrepresenting the returns of Mauriceau's cases. I have shown that it was he who had mis-stated them, by omitting one given in Mauriceau's collection. (See THE LANCET, p. 378.)—*Admitted by Dr. Lee*.

Sixth error of statement of Dr. Lee.—Dr. Lee has latterly averred—first, that the number of Portal's recorded cases of placenta prævia is not fourteen, (as I had stated,) but eighteen; and secondly, that eight of these eighteen were cases of *complete*, and ten of them cases of *partial*, presentation of the placenta; and that he (Dr. Lee) had constructed a table proving this. I already gave references to my fourteen cases, and stated, (p. 378),—"I call upon Dr. Lee to publish this table, to show that I am in error, or to retract his remarks, or by his silence to acknowledge that the 'false

* See Medical Gazette for Nov. 7th, 1845, p. 1193.

† If there is any one error whatever in the returns in that table of Dr. Lee's sixty-four cases, let Dr. Lee point it out.