nerve is healthy, but that its peripheral branches, with the muscles, are disorganised, as may be seen in some cases of lead-poisoning.

If the nerve act normally to both currents while faradism, when applied directly to the muscle, excites contraction according to its bulk, but galvanism is followed by the reaction of degeneration—these signs show that the nerve is healthy in its whole extent, and that the muscle alone is diseased, which is seen in progressive muscular atrophy, pseudo-hypertrophic paralysis, etc.

If the nerve act normally to both currents, faradism applied directly to the muscle produce no result, and galvanism to the muscle be followed by normal reactions—these indicate that the trunk of the nerve and the muscles are healthy, but that the peripheral intramuscular branches are paralysed. This is seen in some forms of facial paralysis from exposure to cold.

In peripheral paralysis, all the elements of the nerve-structures are not always equally affected. If the nerve and muscles show perfectly normal electrical reactions, it is evidence that the voluntary motor fibres have been altered only in function, and not in structure, as seen in the slight forms of rheumatic facial paralysis. If the nerve exhibit great diminution or loss of electrical response, the nutritive influences to its motor fibres must be involved; and, if there be degenerative reactions of muscle, it shows that, in addition to the motor fibres, the trophic conductors to the muscles are involved; either of which may be seen in the middle form of facial paralysis. Finally, both nerve and muscle may exhibit abnormal reactions, demonstrating that the whole nerve, including the motor conductors and the trophic fibres to nerve and muscle, is degenerated, as instanced by the severe form of facial paralysis.

In many doubtful cases, electricity may decide our diagnosis before there are other symptoms sufficiently definite. For example, in an incipient case of lead-poisoning, the bulk of the muscles and their voluntary power may be apparently normal. On testing with the faradic current, diminished nerve-excitability may be found; and with the galvanic, commencing degeneration in muscles may be demonstrated—thus confirming our suspicions; for, in this disease, the histological changes precede apparent impairment of function.

In estimating the condition of sensation, electricity is of great service, and delicate shades of increased or diminished sensibility may thus be detected. The faradic current is indispensable for determining muscular sensibility, and galvanism applied to the tongue is the best method for testing the sense of taste.

Many instances might be given to show how electricity practically assists diagnosis. Of these, we shall only mention three as illustrations.

A young woman had, among other symptoms, almost complete paralysis of the left leg, from which she had suffered for many months. Electrical tests failed to elicit abnormal reactions in any part of the body. This greatly assisted in the diagnosis of cortical cerebral disease, which was confirmed after death by the discovery of a tumour in the right hemisphere, in the posterior parietal region.

A man, previously in good health, fell on a pitchfork, a prong of which passed through his upper arm. After the wound healed, the limb was weak, and, a month after the accident, had greatly lost power; and the muscles of the hand had wasted. The diagnosis of his physician was local injury to the nerves, with corresponding loss of function. Tested electrically, the nerves were found perfectly normal. With faradism, the muscles contracted according to their bulk; and those which were atrophied exhibited with galvanism the reaction of degeneration. On investigating the muscles of the apparently normal arm, precisely the same conditions were found, only not so definitely marked. Here, then, was a case, not of injury to the nerve from an accident, but an instance, probably, of commencing progressive muscular atrophy; the fall being a coincidence, and merely attracting the patient's attention to his right hand.

A house-painter entered the hospital suffering from general debility and vague dyspeptic symptoms. The question arose, Is this man suffering from lead-poisoning? There were no symptoms except those stated above; no colic; no blue line on the gums; and he had never observed any weakness or wasting of the muscles of the arms. Electricity applied to the extensors of the wrist demonstrated slight but distinct reactions of degeneration, which, under the circumstances, definitely confirmed an otherwise doubtful diagnosis.

These are three examples in which the diagnosis of a lesion respectively of the brain, spinal cord, and peripheral nerves and muscles, was practically established by electrical tests. Others similar might be multiplied, which brevity compels us to omit. It is, however, to be hoped that, in the foregoing discussion, enough has been said to show that our knowledge of electricity, although as yet imperfect, is even now sufficiently advanced to afford us valuable assistance in the diagnosis of nervous diseases.

MR SPENCE ON SURGICAL STATISTICS.

By JOSEPH LISTER, F.R.S.,

Professor of Clinical Surgery in King's College, London.

MR. SPENCE's article on Surgical Statistics, which appeared in the JOURNAL of the 24th ult., consists of two elements; one tending to disparage my character as a surgeon; the other calculated to diminish the effect of the statistics which I lately adduced in favour of antiseptic surgery. The former of these elements I shall take leave to dismiss without further notice than the remark that, as Mr. Spence never did me the honour to witness the practice which he criticises, so that his knowledge of it, beyond my brief descriptions of the cases, must have been derived entirely from hearsay, the exercise of a little charity towards a late colleague might have induced him, in every one of the points to which he refers, to accord a more generous and at the same time a more just interpretation.

But, in direct proportion as Mr. Spence succeeds in persuading the readers of the JOURNAL of his own superiority and of my inferiority as a surgeon, must his argument tell in favour of antiseptic surgery; for if, in spite of such disadvantages, I am enabled, by the application of a new principle, to obtain results superior to his, the fact must speak volumes in favour of the principle.

First, then, of amputations; Mr. Spence produces a table of excellent results from his own practice. But I have first to remark that they are not derived from the period during which mine occurred, but from a time long antecedent. What is the reason for this selection? We can hardly suppose that Mr. Spence took his illustrations from the practice of many years ago because he thought his method of treating wounds then superior to that which he employs at present. We are, therefore, forced to the conclusion that he has selected a set of cases in which good fortune came greatly in aid of good surgery.

Next, I have to observe that the cases which he compares with mine are not similar to them in kind; for he omits primary amputations, which are by far the most dangerous class, and which are included in my numbers. And the manner in which his statement is made is somewhat apt to mislead, although, I am sure, without any such intention on his part. After the mention of my general results of nine deaths in eighty major amputations, he proceeds to make what I venture to regard as a lame apology for the selection of a different period from mine, and then gives his own figures in a tabular form calculated to strike the eye; and it requires a somewhat close reading of the text to notice that these cases are only amputations for disease, and therefore not fairly comparable with mine. In order to institute a just comparison, I must deduct the primary amputations from my own list. These were sixteen in number, with four deaths, leaving sixty-four amputations for disease and secondary amputations for injury, with five deaths; and thus my percentage of mortality is at once reduced from 11.2 to 7.8. But here I must repeat the observations which I made on these cases in the discussion at St. Thomas's. Of the five deaths, two were after amputation at the hip-joint, followed by death within twenty-four hours after the operation; and, whether Mr. Spence be correct or not in his idea that the patients might perhaps have been saved by better surgery, these cases have no bearing whatsoever upon the question under discussion, which is the relative safety of different modes of dressing. Both succumbed from the immediate effects of the operation. Those two cases, therefore, should be excluded as irrelevant. But I cannot take leave of them without raising a protest against the view which Mr. Spence must be supposed from his remarks to entertain, that every case of myeloid disease of the lower end of the femur should be summarily treated by formal amputation at the hip-joint. We know how greatly these cases differ among themselves as to the degree in which the bone is implicated beyond the obvious tumour; and many a man would be deprived of life unnecessarily, and many another would lose the advantage of an useful stump, if such a doctrine were allowed to prevail.

My other deaths arose from accidental causes wholly unconnected with the operation. One took place in a woman who presented herself with a swelling of no large size under the right deltoid, so extremely soft that I was unable to satisfy myself whether it was a collection of fluid in the bursa or a solid of jelly-like consistence, without an exploratory puncture, which was made antiseptically; a malignant tumour was diagnosed, and the patient was requested to submit to amputation. This, however, she refused to do, and one day tore off the antiseptic dressings, with the result of causing putrefaction in the tumour and an aggravation of both local and general

symptoms that convinced her of the necessity of amputation. By taking the flaps chiefly from the skin, I succeeded in entirely avoiding the putrid element. The amputation-wound followed a typically aseptic course; and I regarded the patient as already safe, when, three days after the operation, bleeding took place in very large amount from a tumour of the same nature in the right thigh, and she died of this internal hæmorrhage. The *post mortem* examination showed that the tumour which had furnished the blood, though it involved the greater part of the thickness of a limited portion of the upper end of the femur, was of such very small dimensions that it could hardly have attracted the attention of a surgeon, supposing him to have examined her from top to toe for the chance of so unlikely a complication.

Of the remaining two deaths, one occurred among my amputations of the thigh, which were twenty-six in number. The amputations of the thigh constitute the main glory of Mr. Spence's table; for, though it contains twenty-two cases of amputation at the ankle-joint without a death, yet that operation, in the hands of a good surgeon, is so eminently safe, that such a successful series is not very remarkable. But his twenty-five amputations of the thigh with only two deaths are certainly fair subjects of just pride, more especially when we learn from the context that one of these cases died of "albuminuria from scrofulous kidney". We must not, however, assume, without more detailed information as to the precise nature of the renal affection, that death would be inevitable at the present day in such a case, supposing strict antiseptic treatment to be employed; for soundness of the excretory organs becomes a matter of much less essential moment when septic agencies are entirely prevented from entering the organism through the wound, and therefore do not require to be eliminated from the system. And, though Mr. Spence tells us that "the stump healed almost entirely by first intention", we must remember that there is here a mighty difference between almost and altogether, more especially as the operation was performed in the old days of septic silk ligatures.

The other cause of death in Mr. Spence's amputations of the thigh, though it almost appears as if put down under the old notion that it formed a sufficient excuse for mortality, is tragical enough—pyæmia on the twentieth day! I say, then, that in that which constitutes the main glory of Mr. Spence's table there is one melancholy blot; while, merely numerically, his twenty-five cases with two deaths are surpassed by my twenty-six cases with one death. But this one death I must claim as a recovery from the amputation; for the patient died of a totally independent cause—diphtheria—when the stump was almost entirely cicatrised, nine weeks after the operation; for the nine *months* which afford Mr. Spence occasion for such sarcasm are, like other inaccuracies to which he refers, a reporter's error.

The other death was in a boy seven years old, after amputation at the ankle. The stump certainly was slow in healing, as must, I fear, now and then be the case under any treatment where we operate through unsound textures in a feeble constitution. But the cicatrising process was, at the end of three months, almost absolutely perfected, when he died of a cause very rare at such a period of life, but confirmed by *post mortem* examination—cerebral hæmorrhage. Now, I contend that it is only strictly just to regard that case also as one of recovery from the amputation.

Thus, excluding as irrelevant the two cases of amputation at the hip-joint where death took place within twenty-four hours of the operation, the results of my amputations other than primary may be thus given in a tabular form.

	Number.	Recovered.	Died.
Thigh	26 ..	25 ..	1*
Leg	5 ..	5 ..	0
Ankle	16 ..	15 ..	1*
Shoulder	1 ..	0 ..	1†
Arm... .. .	6 ..	6 ..	0
Forearm	8 ..	8 ..	0
	62	59	3‡

It thus appears that, even in my lame hands, the application of the strict antiseptic principle has led to results surpassing those attained in a specially selected period by an excellent surgeon, who prides himself upon his success in the department of amputation, but who has not adopted the principle.

Mr. Spence asserts that "much of my practice was in the same hospital and under the same hygienic conditions" as his own. *All* my practice referred to by Mr. Spence took place in the same hospital as his; but my hygienic conditions were exceedingly inferior, and I am surprised that Mr. Spence was not aware of the fact. In visiting his

wards, I have been struck with the much greater space between the beds than prevailed in my own. When I first took charge of the clinical wards, I was so dissatisfied with what appeared to me the overcrowding of the patients, that I had the beds considerably reduced; but, finding that the step had been rendered nugatory by mattresses being put down at night for the accommodation of patients who could be up during the day, while the wards continued healthy, I allowed the beds to be replaced. Afterwards, the demand for accommodation becoming excessive, the system of putting down the mattresses at night again crept in; and, besides this, children were frequently placed two and sometimes three in a bed; so that, while my beds, quite too numerous in proportion to my space, according to prevalent hygienic ideas, were fifty in number, my patients were very rarely under sixty, and sometimes amounted to seventy. And, further, the annual cleaning of the wards, which took place, I believe, under all the other surgeons, was for three years omitted in my department, because, while the perfect healthiness of the wards rendered it unnecessary, the process entailed a risk of injury to individual cases in moving them from place to place. All this was not only notorious in Edinburgh, but it was fully described in my address on opening the Surgical Section of the meeting of the British Medical Association in that city, published in the BRITISH MEDICAL JOURNAL. (See BRITISH MEDICAL JOURNAL, December 25th, 1875.)

The truth is, therefore, that, so far from Mr. Spence's results being fairly comparable with mine on the ground of similarity of the hygienic conditions under which we worked, the converse is the case in an exceedingly remarkable degree; and the fact that the strict antiseptic principle renders the surgeon independent of such unfavourable surroundings, constitutes a signal triumph for the principle.

After describing his own successes in removal of the breast for scirrhus, and declaring that he has "never felt at liberty to test the spray-system in this class of operations", Mr. Spence inquires, "What are Mr. Lister's results in excisions of the mamma?" I feel it needful to reply to this question, because it seems to hint not obscurely that the antiseptic treatment has been followed in my hands by great failure in this important operation. Such, however, is very far from being the fact. Those who have visited my wards can testify that the general course of such cases has been one of absence of suppuration or febrile disturbance, not only after removal of the gland and dissecting out the entire contents of the axilla, but also with a degree of traction of sutures to approximate cutaneous margins, when a large amount of integument has been taken away, such as it would be out of the question to employ except under strict antiseptic management; for it is remarkable how much dragging upon the skin may be practised with impunity, if the excitement and swelling due to septic inflammation be avoided. Stitches, instead of cutting their way out quickly, as would otherwise be the case, retain their places till cicatrization is complete, the integument gradually yielding and accommodating itself to its altered circumstances.

Mr. Watson Cheyne, to whom I am much indebted for the labour he has bestowed in compiling complete statistics of my operations in the Edinburgh Infirmary from the latter part of 1871 (in which year the spray and gauze were introduced) till I left the institution in 1877, informs me that my operations for mammary tumour during that period were thirty-eight; viz., twenty-four cases in which the axilla was cleared out, besides the removal of the gland; three in which the mamma only was removed; four of recurrent scirrhus; and seven of simple tumours excised without the mammary gland. Of these thirty-eight operations, three were followed by death. Now, I freely admit that this is too high a death-rate. I mentioned in the debate at St. Thomas's that I had two deaths from blood-poisoning after this operation; and Mr. Spence has put his finger upon what, in that respect, was an unlucky bit of my practice; for the *only* two deaths from blood-poisoning which occurred during the entire period from 1871 to 1877, after operations antiseptically performed, fell upon this group.

The cause of the fatal event in one of the cases was erysipelas, which, according to my experience, like that of Volkmann of Halle and Nussbaum of Munich, is generally banished entirely by strict antiseptic treatment even in wards otherwise very liable to the disease.

In the other case, the entire contents of the axilla having been removed as well as the mammary gland, a practice which I almost invariably adopt in scirrhus of the mamma, all went well for ten days, when, having removed the drainage-tube from the axilla in dressing the patient, I was grieved to observe that the gentleman in charge of the spray had failed to direct it over the part. That very evening, she became feverish, and next morning the axillary wound was observed to be putrid; two days later she had a rigor; and other rigors occurred at varying intervals during the remaining nine days of life. Although on *post mortem* examination no pus was found in the veins of the region, nor any abscesses in the lungs or elsewhere, yet there was no doubt whatever that the

* Recovered from the amputation, but died from an independent cause.

† The cause of death was wholly independent of the operation.

‡ Mr. Spence is mistaken in supposing that there was any other fatal case besides those referred to in the table.

patient died of a form of blood-poisoning, which, according to present nomenclature, we should term septicæmia. But, when I say that this was the only instance of septicæmia among antiseptic cases during the six years, and that not a single example of pyæmia occurred during that long period among patients to whom the strict antiseptic principle was applicable, including not only 553 major operations, but also a multitude of minor ones among in-patients and out-patients, and a host of trivial injuries besides those admitted into hospital on account of their severity, such as compound fractures and wounds of joints, and this in spite of hygienic conditions which, except for the strict antiseptic principle, would have been highly unfavourable, I state a fact at which I think every unprejudiced friend of mankind must rejoice.

[To be concluded.]

PROFESSOR SPENCE ON SURGICAL STATISTICS.

By W. WATSON CHEYNE, M.B., F.R.C.S.,
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PROFESSOR SPENCE'S paper on surgical statistics, published in the number of the BRITISH MEDICAL JOURNAL for January 24th, is probably one of the most powerful replies which will be made to those produced by Mr. Lister at the recent debate on antiseptic surgery. In two ways, this attack may be called powerful; for, on the one hand, Mr. Spence is well known as a most careful surgeon—careful in his diagnosis, careful in the selection of cases for operation, and careful in avoiding all irritating applications to the wounds during the after-treatment; while, on the other hand, Mr. Spence has acted as surgeon in the Edinburgh Royal Infirmary during the period in which the results which Mr. Lister has published were obtained. His remarks seem, therefore, to call for careful criticism.

The chief object of Mr. Spence's paper is to show that at least as good results have been obtained without the adoption of any strict antiseptic measures; and, in support of this, he produces a table of amputations performed long ago, and evidently selected on account of its excellence. This selection of results for comparison must, however, be strongly protested against, and contrasts very unfavourably with Mr. Lister's complete statement of the cases occurring since the more recent improvements in antiseptic treatment have been introduced. Mr. Spence, indeed, attempts to say that Mr. Lister's is also a selected period; but in this I venture to think he is wrong. For, in judging of the usefulness of a certain method of treatment, one must look at results obtained when that method has been more or less perfected. But if the method employed by Mr. Spence in the treatment of the cases referred to were a perfected one, I would ask how it is that he has come to modify it with, as seen in his later reports, very unfavourable results.

With regard to the table itself, the results are undoubtedly excellent, and, if constant, would go far to show that strict antiseptic treatment is not so necessary for ordinary cases as has been asserted. But, on looking at it more closely, we recognise an old friend; for this same table was produced by Mr. Spence in his address on surgery at the Edinburgh meeting of the British Medical Association; and, on referring to his various publications, it will be found to be an especial favourite. Seeing, however, that the results were obtained as long ago as the years 1860, 1861, and 1862, it seems high time that a new table had been constructed.

In order to obtain a complete idea as to the entire state of the facts in this important matter, it appears to me that it will be well to examine shortly what are the results which Mr. Spence usually obtains; and with this object, I shall refer, in the first place, to those more recently published by him.

In the *Medical Times and Gazette* for March 13th, 1875, is the report of his major operations from October 1872 to October 1874. Again, in the same journal for October 28th, 1876, is the report from April 1875 to the end of September 1876. And, lastly, in the November and December numbers of the *Edinburgh Medical Journal* of last year, is that from October 1876 to the end of the winter session 1878. Thus we have a record of Mr. Spence's practice from October 1872 till April 1878, with the exception of the report for the winter session 1874-75; and, as this period corresponds in greater part to that during which Mr. Lister's results were obtained, I shall state what I find.

During the first period, there were 115 major cases treated, of which 21 died; during the second, 76 cases, with 16 deaths; and, during the

third, 140 cases, with 21 deaths; thus making a total of 331 major cases treated between October 1872 and April 1878 (with the exception of the winter session of 1874-75), of which 58 died, or a percentage mortality after major operations of 17.5, as contrasted with Mr. Lister's mortality after major operations of 5.1 per cent.*

On attempting to ascertain what is the cause of death in Mr. Spence's cases, and more especially what is the proportion of blood-poisoning, I find the task hopeless; for, in 16 out of the 58 deaths, the cause is not definitely stated, though, in several instances, we are led to surmise from the facts mentioned that blood-poisoning, in some form or other, had occurred. Thus, to give two examples, we are told, in connection with a fatal case of amputation of the upper arm, that "the prognosis was favourable, as the patient was under middle age. However, he had suffered from rheumatic fever, and was stated to have had a stroke of palsy." These are, no doubt, very interesting facts, but they do not explain why the patient died. Again, the remarks on a fatal case of amputation at the shoulder-joint are the following. "At the time of the accident, the patient was out on pass from the medical house, where he was under treatment for mitral and aortic disease with bronchitis. He had also lost much blood prior to admission." An incautious reader might, in all probability, set down the cause of death, in the first instance, to rheumatic fever or to the reported stroke of palsy; and, in the second, to mitral and aortic disease. Does Mr. Spence really mean that these were the causes of death? or did the patients die of blood-poisoning, and are these facts stated as affording some explanation of that circumstance? Of the remaining 42 cases, five died within thirty hours after the operation, and may therefore be left out of consideration. In eight, the fatal result is assigned to septic poisoning: while, in several, such indefinite causes as the following are mentioned; "irritative fever", "unhealthy action in the wound", "diarrhoea and vomiting" (in several cases), "uræmia, although without any marked symptoms", "hæmorrhage, generally in the form of uncontrollable oozing" (a well-known symptom of blood-poisoning), etc.† It will thus be evident that any attempt to gain, from these statements, some true idea as to the frequency of blood-poisoning after this method of dressing would be hopeless; and it would therefore be well, as the matter is one of the greatest importance, if Mr. Spence furnished the cause of death in those cases where it is not mentioned, the pathologist's report of the *post mortem* examinations in all cases as far as practicable, and the report of his practice during the winter session 1874-75.

Looking, for a moment, at the amputations during these years, we find that 97 major amputations were performed, of which 25 died, or a mortality after major amputations of 25.7 per cent., as contrasted with Mr. Lister's mortality of 11.25 per cent. I give Mr. Spence's results in a tabular form.

TABLE I.

	Primary.		Secondary to Injury.		For Disease.		Total.	
	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.	No. of Cases.	Deaths.
Hip-joint	0	0	0	0	3	1	3	1
Thigh	5	3	1	0	25	6	31	9
Knee-joint ..	1	0	0	0	0	0	1	0
Leg	4	0	1	1	8	1	13	2
Ankle	1	0	1	1	23	1	25	2
Partial of foot.	0	0	0	0	6	2	6	2
Shoulder-joint	1	1	1	0	2	1	4	2
Arm	2	1	2	0	5	2	9	3
Forearm	2	2	0	0	2	2	4	4
Partial of hand	0	0	0	0	1	0	1	0
	16	7	6	2	75	16	97	25
	or 43.7 per ct.		or 33.3 per ct.		or 21.3 per ct.		or 25.7 per ct.	

In constructing this table, I have followed Mr. Spence's division of his cases into "primary", "secondary", and "for disease": a division which he adopts, chiefly because the former groups are more liable to die of blood-poisoning than the latter. In this he is undoubtedly cor-

* As far as I can see, there ought only to be 114 major cases with 20 deaths; for in one of the fatal cases amputation was first performed through the leg for disease of the ankle-joint, and afterwards osteomyelitis setting in amputation of the thigh was done. The patient died of diarrhoea (?) five days after the latter operation.

† The number of cases which died of bronchitis and pneumonia is very remarkable. Were *post mortem* examinations made in all these instances?