female members of the family, coupled with most of the prominent features of the case before us, and from many trifling though important incidents which from defective memory I have omitted, I am inclined to consider it one of hysteria of an aggravated character, complicated probably with a morbid condition of the brain. I entertain hopes that, provided I can sufficiently nourish the body until the uterine organs are more fully developed, my patient may continue to be "*The Dead Alive*."

I am, Sir, your obedient servant,

RICHARD BIRD NASON, M.R.C.S., L.S.A.

Bridge-street, Nuneaton, December 14th, 1858.

DIPHTHERIA.

[LETTER FROM JOSEPH STEPHENS, ESQ., M.R.C.S.]

To the Editor of THE LANCET.

SIR,—In discussing the subject of diphtheria within the limited space you can devote to its consideration, it is necessary that each of your correspondents should, when he has fairly expressed the results of his experience, retire from the field in favour of new-comers; the amassing of the opinions of the greatest possible number of observers being of paramount importance, that just conclusions may speedily be arrived at as to the nature of the disease and its appropriate treatment.

Having, however, had some four hundred diphtheritic patients under my care during the last two years, I am led again to address you, my primary reason being to correct an impression which has been, it appears, conveyed by my previously stating that I "have not been able to detect vesicles preceding the formation of the crusts," assumed to be characteristic of genuine fully-developed diphtheria.

Your correspondent in THE LANCET of the 11th inst., Mr. George, takes exception to this assertion; but on reference to the context of the passage, it will be found that I was merely questioning the accuracy of the herpetic theory of diphtheria. In short, I intended merely to convey my opinion that there is *essentially* no vesicular stage of diphtheria, and that *consequently* it cannot be a disease of *herpetic* character.

In many cases I have seen aphthæ, small and large, solitary and confluent, distributed over the pharynx, tonsils, palate, tongue, &c., and at all periods of the disease; sometimes preceding the formation of the false membrane, at others concomitant with it, and again as a sequel to its exfoliation; but never has it appeared to me that these vesicles, however minute and closely congregated they may have been, have been the parents of the crusts; indeed, in the great majority of cases which have fallen under my observation, I have been unable to detect any vesicular formation whatever.

Leaving at rest the question whether these vesicles (when they occur) are eczematous, or herpetic, or peculiar to the disease, I must reiterate my opinion, that diphtheria is *essentially* neither a vesicular disease nor a local one, but one dependent on a blood-poison, which seeks its elimination chiefly through the pharyngeal mucous membrane; and as our experience of this formidable disease enlarges, I shall not be surprised if lesions of other textures are produced by the determination of the poison to them. I have observed serious lesion of the sensitive and motor nerves which could scarcely be explained by the supposition of pressure being exerted by enlarged glands, and have read of similar cases. And, again, I have seen anasarca following diphtheria, when I have not been able to satisfy myself that it was masked scarlatina.

With the same constitutional symptoms, more or less marked, I have observed different local developments of the disease. I have noted three almost distinct sets of appearances presented by it. In the first there has been considerable enlargement of the tonsils, and tumefaction of the neighbouring parts, *without* any manifest exudation; in the second, there has been similar swelling, *with* exudation almost entirely confined to the tonsils, sometimes, however, *spreading* to the contiguous parts, and not unfrequently into the windpipe; and, in the third, a more or less continuous membrane has been exuded, so that one could scarcely say where it commenced over the whole of the interior of the mouth, pharynx, &c. I presume it is the last form which has given the name of diphtheria to the disease, for on opening the mouth of such a patient, it certainly does appear very much as if lined with chamois leather. Of this last form I will give a case:—

I was called, on the 31st of October last, to see E. W.—, a boy aged eight years, of somewhat spare habit and delicate

constitution, and found him labouring under diphtheria. He had been ailing for two or three days, having been weak and listless, with no appetite, and suffering from occasional attacks of vomiting. He had first complained of his throat on the morning of my visit. When I saw him, his general appearance and symptoms were, in short, those of adynamic fever. On inspecting the oral cavity, I found it lined throughout with a white tenacious membrane, which could be readily stripped from the subjacent textures. The tonsils were swollen, and covered in like manner. On removing the exudation, the mucous membrane presented a very red and glassy appearance, being perfectly smooth except in the tonsillar sulci. In from ten to twelve hours, however, it would be covered again as uniformly and thickly as ever.

In this case I watched narrowly the mode of formation of the crust, and could only observe that after awhile the red, glassy, mucous surface would become dimmed and gradually hidden by an opaque coating resembling that produced by the application of lunar caustic. With the naked eye I could detect nothing resembling a vesicle.

The exudation was removed twice a day by means of a sponge, and a solution of nitrate of silver (two drachms to one drachm of water) applied afterwards. The breathing frequently became croupy, but was relieved by the accession of cough and the expulsion of diphtheritic casts.

The general treatment consisted in the exhibition of chlorate of potass, hydrochloric acid, and tincture of cinchona, every three or four hours. Wine and beef-tea were given, I may say, almost constantly; and on one occasion, when the patient appeared *in extremis*, I adopted Dr. Kingsford's valuable suggestion, and administered them combined in an enema.

In imminent danger of death on the one hand by apnoea, and on the other by asthenia, the poor little fellow struggled on for nearly three weeks, when, in answer to our perseverance, signs of improvement began to appear; the tonsils, and with them the dysphagia, diminished, the exudation gradually ceased to be formed, the cough departed, appetite returned, and in about a week more I was able to hand him over to the ministrations of his mother, to whose previous indefatigability in carrying out my instructions I in great measure ascribe his recovery.

Since my last communication, two cases have occurred in my practice bearing strongly on the question of infection. In the one, a nurse, who attended a patient during only one night, at a distance of half a mile from her home, contracted the disease, and gave it to her family and neighbours; and in the other, a little girl was sent home some miles, labouring under diphtheria, and immediately the disease broke out and spread in the houses adjoining her home.

Having seen little of the literature of diphtheria but what has appeared in THE LANCET, I look forward with much expectation of profit and pleasure to the "Selected Memoirs on Diphtheria" to be issued very shortly by the New Sydenham Society; and shall be very glad, as a member, if the promised appearance of this opportune work should induce others to join our ranks.

I am, Sir, your obedient servant,

JOSEPH STEPHENS, M.R.C.S.E., &c.

Grampound, Cornwall, Dec. 1858.

P.S.—Since writing the above, I have evacuated a large abscess formed in a submaxillary gland, the result of diphtheria; the contents were chiefly fluid, holding in suspension a few of the curdy masses characteristic of glandular abscess. In this case I have resorted to nutritious enemata, the patient being unable to swallow. I fear, however, he will sink, as an abscess appears to be forming on the opposite side; and his strength, thus imperfectly recruited, will scarcely sustain the irritation and drain.

SCARBOROUGH SANATORIUM.

[LETTER FROM DR. MORLEY ROOKE.]

To the Editor of THE LANCET.

SIR,—I had indicted, as you are aware, an answer at some length to the letter of Mr. Easton in your journal of the 27th ult., and intended it for insertion last Saturday; but having received an intimation from you that the pressure on your columns is such as to necessitate shorter communications from your correspondents, I will not attempt to meet more than one or two points, and will refer anyone interested in the controversy to the answer I have elsewhere and before given to the staple objections urged by Mr. Easton.

1st. I must beg to reiterate that I *did* raise my voice more

than twelve months since against the present site; and I would also remind Mr. Easton that it is not considered the most polite behaviour to continue charges which have been already refuted.

2nd. The necessity, which Mr. Easton wishes to enforce, of having a marine sanatorium erected where the patients may be protected against the cold winds of winter and early spring, can have no existence in an institution which, as it happens, is altogether closed at those periods of the year. It is quite possible Mr. Easton was not aware of this fact, as he never had any connexion with the institution, not even that of a "small subscriber;" and from his erroneous description of the aspect of the intended site, I doubt if he knows its exact locality; for it is, in reality, open to the obnoxious winds of winter and spring, and injuriously shut in from those which chiefly prevail in summer.

3d. The assertion that "I and one or two more have taken every pains with a lady who has most liberally subscribed £2000, in order to get her to withdraw it," I can only characterize as a pure fabrication: it was certainly in Mr. Easton's letter that I first encountered the charge; the members of the Infirmary Committee, fully informed on the whole matter, deny having ever made such an imputation, which they well know to be utterly devoid of foundation. The *onus* of this calumny, therefore, rests with Mr. Easton; and I trust that, for the sake of his good name, he may be able to explain it. The lady in question was merely respectfully asked, provided she saw the matter in the same light as the objectors, to use her influence to have the site changed.

In conclusion, Sir, if what I have written appear meagre or unsatisfactory to your readers, as it probably will to those unacquainted with the whole matter in dispute, I can only remind them of the proverb that "A fool may ask a question which a wise man cannot answer;" and so an objector may, in a few words, appear to raise difficulties which require a great many for their proper confutation.

I am, Sir, obediently yours,

Scarborough, Dec. 6th, 1858. T. MORLEY ROOKE, M.D. Lond.

* * Here this controversy must terminate.—ED. L.

ELECTRIC ANÆSTHESIA IN SURGERY, &c.

To the Editor of THE LANCET.

SIR,—According to a notice in your number of the 11th inst., M. Morel Lavallée has, at the Hospital St. Antoine, Paris, been enabled to make incisions and extract teeth without *any pain* to the patient, by means of attaching the instrument employed to an induction machine.

Now, if this statement can be substantiated, it is one of the most important advances made in modern surgery. But it is not with dentists that the *onus* must rest of so substantiating the truth of such an assertion, but with surgeons; because the difficulty and uncertainty of arriving at anything like reliable evidence of such a means being the cause of a tooth's extraction occasioning less pain than usual, are very great. But it becomes a very simple matter, in many other operations, thus to test the properties of electricity, as, indeed, is said to have been done by M. Morel. Let the simplest of all operations be in this way selected: let a common needle be attached to one pole of the machine, and if the needle can then be thrust into the skin without any patient ever feeling it—that is, without causing any pain—then tooth extraction may be rendered less painful by the adoption of similar means; but even then it might be questionable whether extraction under such circumstances could be performed without any pain, as a tooth is such a bad conductor that no effect of the electric current is perceptible upon touching one with the pole of the machine, unless the tooth is partially decayed or worn down, and the pole be applied to this part, or, on the other hand, be allowed to touch the gum.

However, considering the altogether complex nature of such an operation as the evulsion of a tooth—in comparison to a mere puncture or an incision—there seems every reason to believe that what would prevent pain in the former would prevent it in the latter operations; and it also seems quite warrantable to assume that if a puncture or an incision *cannot* be thus performed, literally without *any pain*, the assertions every now and then appearing in reference to the practicability of such modes of obviating pain, are based upon most unsatisfactory evidence, or upon none at all.

I am, Sir, your obedient servant,

J. SMITH, M.D., Dentist,

December, 1858. Lecturer on Dental Surgery, Medical School, Surgeons' Hall, Edinburgh.

DRUGGIST-SURGEONS.

To the Editor of THE LANCET.

SIR,—In reference to Dr. Thorn's letter, contained in last week's LANCET, I wish to say that there are probably few who do not agree with him in the propriety of putting an end, if possible, to the evil of prescribing as carried on by druggists and unqualified persons. I would, however, submit to Dr. Thorn, or whoever takes up the question, that the root of the matter consists in the fact that some gentlemen possessing diplomas descend to retail, and thus confound themselves with druggists to an extent which prevents the poorer classes from distinguishing between them. As long as this error continues, no power can materially assist in putting an end to the improprieties of which Dr. Thorn so justly complains.

I remain, Sir, yours, &c.,

EDWARD COOPER WILLIS, M.B. CANTAB.

Addison-road, Kensington, December, 1858.

FLOW OF URINE BY THE URETHRA AFTER THE OPERATION OF LITHOTOMY.

To the Editor of THE LANCET.

SIR,—In looking over your widely-circulated journal of the 2nd of October, 1858, I observe under the head, "Clinical Records," mention made of the case of a boy who had been operated upon for stone in the bladder, in which the urine began to flow by the urethra twenty-four hours after the operation, and continued to do so without being accompanied by any bad symptom, the little patient progressing towards a speedy recovery, which was considered so remarkable and unusual as to deserve particular notice.

Now if you will have the goodness to look back thirty years to your own journal of the 19th of April, 1828, you will find an extract from the *Edinburgh Medical and Surgical Journal*, in which an intimation is made of *five cases* wherein the wound made into the urethra and prostate adhered and healed by the first intention, without undergoing the suppurative process, whereby the cure was remarkable expedited, each of them being able to get out of bed and move about the room within three and four days after the operation.

Nine years afterwards, I transmitted for insertion into the *Edinburgh Medical and Surgical Journal*, No. 132, April, 1837, the cases of *thirteen out of forty* I had operated on during that nine years' interval, wherein the prostatic wound adhered and healed by the first intention, the urine coming off by the natural passage from the very first, and every one of the thirteen able to get out of bed and move about the room within a few days after the operation. In the cases of the remainder of the forty, the chylopoietic viscera being much disordered, and the vital powers worn out and exhausted by long-continued suffering and other diseases, it was considered advisable to encourage the flow of urine by the wound until the cure was effected by the more tedious process of suppuration.

You may also read a letter addressed by me to Dr. W. Sharpey, F.R.S., University College, containing practical reflections on the operation of lithotomy, and afterwards communicated by him to the Editor of the *British and Foreign Medico-Chirurgical Review*, who gave it a place in No. 27, April, 1854, of that journal. And to this day, every case that presents to me for operation, if a subject proper for it, I encourage the union and healing of the prostatic wound by the first intention. Of these cases, I have now witnessed about thirty in my own practice, all accompanied with the same delightful results.

About three years ago, I was favoured with a communication from Dr. J. Nottingham, an eminent practitioner in Liverpool, who says he has for many years bestowed much attention upon, and taken a deep interest in, everything relative to this branch of the profession; and although not personally acquainted with him, yet from his writings I should judge him to be of very superior intellect, and more clear and rational in his ideas upon the subject than any that have come within my observation. In a part of his communication he says—"The most satisfactory result of this kind (alluding to union by the first intention) I have hitherto obtained was in a gentleman forty-four years of age. A little while after the operation, the urine passed by the urethra; not a drop was known to pass by the wound, which healed as completely and as rapidly by the first intention as it could have done had it been inflicted upon the thigh or any other ordinary fleshy part of the body. The patient was, of course, kept quiet for a time, but walked about the room with the wound healed at the end of the week; and