system. There can be little doubt, however, as Dr. Murchison and others have shown, that almost similar symptoms and *post mortem* appearances may be produced in other ways—e.g., from blood-poisoning, in cases of small contracted kidney, where effete materials accumulate within the system. Dr. Murchison says, “it may fairly be inferred that the typhoid state in all febrile diseases, whether they may be due, in the first instance, to a specific poison, or to a local inflammation, admits of a similar explanation. As in the contracted kidney, the blood is contaminated with the products of tissue metamorphosis; but the cause of the contamination is different. It is now well known that, in all febrile diseases, the disintegration or combustion of the tissues is greatly increased beyond the healthy standard. To this is to be ascribed the increased temperature which is the pathognomonic symptom of the febrile state and the rapid emaciation. The products of the increased metamorphosis are mainly eliminated by the kidneys, and appear in the urine. As long as the kidneys are equal to the increased work thrown upon them, the blood is properly depurated, and the typhoid state is ward off. But, if the kidneys be unequal to the task, either from the large amount of effete material to be eliminated, from previous disease in the secreting tissue, or from congestion resulting (as it often does) from their increased work, then the blood becomes contaminated, and convulsions or the typhoid state supervene. This explains why albumen so commonly appears in the urine in the course of all acute diseases of a severe form, and why it is so justly regarded as an unfavourable symptom.” Now, to complete this picture, I would suggest that the congestion of the kidney in these acute febrile diseases may be often brought about by diffuse embolism of its small arteries—as it, doubtless, was in this case of erysipelas; and that the albuminuria thus resulting is a symptom in every way comparable, so far as its pathogenesis is concerned, to the delirium and stupor occasioned by similar lesions in another organ.

I need only hint at the important bearing which the possibility of the occurrence of minute embolisms of this kind may have in the elucidation of previously obscure forms of so-called functional disease of the nervous system, as I hope shortly to publish the details of a fatal case of Chorea, in which such embolisms led to ruptures and obliterations of small vessels throughout the corpora striata, and in the course of the middle cerebral arteries generally—this being a case of bilateral Chorea in which delirium was also present.

The therapeutical indications issuing from this view of the pathology of delirium are sufficiently obvious; but no details on this subject would be suitable or possible on the present occasion. I will only say, if we have a diseased condition of the system in which abundantly increased white blood-corpuscles are stimulated into undue activity, so that they give rise to the most wide-spread mischief throughout the body, obviously, if we cannot prevent or control the antecedent condition which determines the increase in the white corpuscles and the change in the blood, we can strive to find out medicinal agents which, when administered, may tend to blunt that undue excitability of these corpuscles, which we have seen to be the principal cause of their aggregation, and the subsequent production of wide-spread capillary embolisms throughout the brain, kidneys, and other organs.

ON THE EXTERNAL USE OF NITRATE OF SILVER IN ORCHITIS: WITH CASES.

By E. NOBLE SMITH, Senior House-Surgeon to St. Mary's Hospital,

REST being an essential part of the treatment of orchitis, it follows that, among hospital out-patients, cases are frequently met with in which the remedies usually employed are found insufficient to cure the affection, in consequence of the patients being obliged to continue their work. I tried antimony, mercury, and in some cases iron, employing at the same time lotions of spirit, or acetate of lead, and occasionally leeches, and the results were most unsatisfactory. I now use for the first few days evaporating lotions, and give a purge; I then apply, with a brush, a solution of nitrate of silver, of the strength of one drachm of the salt, to an ounce of distilled water, to the surface of the scrotum over the inflamed testicle. In all cases I have found, the following day, great relief to be experienced, and in the majority the pain to have entirely ceased; but the greater part of the swelling and hardness remained; these symptoms are removed subsequently, by attending to the general health, using lotions, and afterwards mercurial ointment—the patient all the while being able to continue his occupation, even when it is laborious. I have generally found one application sufficient; but if there be a slight return of pain, I use the solution again. I could give many cases in illustration. Every case which I have treated with nitrate of silver has ended most satisfactorily. I understand that the originator of this plan of treatment is Mr. Furneaux Jordan.

AN ADDRESS

DELIVERED AT THE

PATHOLOGICAL SOCIETY OF LONDON.

On Tuesday, January 19th, 1869.

By RICHARD QUAIN, M.D., PRESIDENT.

GENTLEMEN,—Owing to the practical character of our proceedings, and the very limited time at our disposal, it has never been the custom for those who held the office which I have now the honour to fill to address you in a lengthy or formal manner on entering upon their duties. I shall, therefore, occupy your attention but for a few minutes; the first of which shall be devoted to offering you my sincere and grateful acknowledgment for this proof of your confidence; and to the expression of a hope that I may still find myself, at the close of my period of office, in possession of that confidence which I so much value. For my guidance I have the examples of the great and distinguished men who have preceded me in this chair, and whom I follow, I say in all sincerity, with unequal steps. I feel, too, that should difficulties occur, for which it may be added there has been no precedent in our history, I can ever rely on your loyal and generous support. As I now address you, I cannot resist the impulse of memory which takes me back to a period twenty-two years ago, when this Society first came into existence; and I would wish to dwell for a moment on the gratification which I share in common with those who are still amongst us—and they happily are not few—who assisted at its origin. The idea of forming such a society on an independent basis, suggested, no doubt, by the existence of the Pathological Society of Dublin, first occurred to Dr. Bentley, a name still borne in recollection by the early members of this Society. I can never forget the pleasure derived from the first communication I received from Dr. Bentley, then a stranger to me, asking my opinion of the feasibility of carrying out such a project. It was evident that such a society could utilise the vast mass of morbid anatomy which must be found in a population of some three millions of people, and of which, at that time, but a very limited use was made. Teachers in the hospitals, and students in the medical schools, had opportunities of studying morbid anatomy, as it was then studied. But we felt then, as would be the case even now, but for the existence of this Society, that to the busy practitioner, who had little time and few opportunities for making *post mortem* examinations, such a society would supply an immense want. It would bring before him, from hospitals and other sources, specimens which he could not otherwise see; he would hear them described and discussed; he would be made familiar with the subjects against which he was daily contending; he would feel himself cleaned from that rust which is ever ready to accumulate on all of us. Need I say how fully these anticipations have been realised? The best, the busiest, the wisest, of our practitioners in every rank have come hither, never failing, as was said by one of our most learned presidents, “to learn something at every meeting.” This kind of *post mortem* examination work, in our idea at the time, constituted a large share of the benefit we expected to receive or to confer. It is true that we anticipated some useful results from bringing together so many minds more or less devoted to the study of pathology, but I question if the most sanguine amongst its founders anticipated the results which have been realised by this Society. It had been my intention to have brought before you an epitome of these results; but when I came to examine our work, when I referred to the admirable index given us by our colleague, Mr. Holmes, I felt that the accumulation of material was so vast that it would be impossible to bring an useful sketch of it within reasonable limits, whilst, in a short and meagre one, injustice must be done to the Society and to its workers. There is little cause for regretting this difficulty. The volumes of *Transactions* published by the Society, accessible and familiar to all cultivators of pathology, are themselves the records of what we have done. There is scarcely any known morbid condition which will not be found investigated, described, and illustrated more or less fully in these volumes. That they are the most valuable storehouses of pathological facts in existence, is proved by the frequency with which the abundant materials they supply are drawn upon by writers on the subject, both at home and abroad. The original members of the Society may well feel gratified at these results. The younger members will fully participate in this feeling if they will refer to the state of pathological knowledge in what, comparing the present with the past, may almost be called the prepathological period—that is to say, five-and-twenty years ago. It is true that *post mortem* examinations were there conducted by zealous and able men, that drawings and models were made,

and that specimens were collected in museums—that pathology was said to be taught and studied. I well remember the teaching of my friend, the late Sir Robert Carswell, whose pupil I had the good fortune to be, and I mention his name with the greatest respect as the most distinguished pathologist and teacher of that time, and one whose pathological drawings have never been surpassed for accuracy and beauty. Yet, if you refer to his great work, and compare the morbid anatomy and the pathology of that period with those of the present, you will be amazed at the progress made in the interval. It was not the fault of our predecessors that they are so much behind us. Physical science has advanced, and we have taken advantage of its progress. We have taken to our aid the microscope, the test-glass, and the crucible. We search into the depths of the ocean; our predecessors went but little beneath its surface. Comparing our knowledge then, I speak more especially of morbid anatomy, and now as accumulated by this Society by our fellow-labourers at home, and still more by those abroad, I do not hesitate to say that the number, the value, and the accuracy of its facts and observations will bear favourable comparison with like progress in any, as they are called, of the more exact sciences. Indeed, dealing here as we do with inanimate matter, and with the operations of physical agents upon it, we acquire, in many cases, something of that power which gives accuracy and certainty to researches in the physical sciences. Our materials are abundant; our workers able, skilled and untiring; our appliances are efficient. We work under favourable circumstances, for we are free from the disturbances which embarrass us when we have to deal with living beings and with the agencies which influence them. In morbid anatomy, studied thus, we have a safe and true basis for pathological knowledge in its widest sense. It is thus alone we can hope to learn the nature of those conditions, for the cure of which our remedial agents are to be used, and it is thus alone we can hope to get rid of the doubtful and the uncertain in the practice of our art. Encouraged by success in the past, let us strive for greater successes in the future. Let us be firmly convinced that an accurate knowledge of disease is the true key to an accurate knowledge of the means by which it can be prevented, arrested in its progress, or cured. Gentlemen, we have rejoiced in our wealth, but, like some young men who have attained their majority—as we have just done—we find ourselves embarrassed by our riches. We hear representations, I will not call them complaints, of our extravagance. We hear that there is not time to examine and discuss the subjects brought before us, and that we do not derive all the profit from our meetings that they might be made to afford us. There are grounds, no doubt, to some extent, for these representations. The difficulty must be met. But, gentlemen, let me assure you that though your Council may do something—and they have this evening appointed a committee on the subject—to mitigate these inconveniences, much of the remedy is in the hands of the members themselves. Independently of the admirable reports of our committees, which may be read for convenience in the abstract, or as summaries, by the secretary, the subjects brought before the Society may be referred to the following heads:—1st. Specimens of an ordinary character, duplicates, it may be, of specimens previously presented: 2nd. Specimens and descriptions of them of greater pathological interest: 3rd. Pathological essays or summaries of collected cases. It has been suggested that specimens included under the first head should no longer be presented to the Society. The rejection of such specimens would, for many good reasons, be very objectionable, and their interference with the more regular progress of our proceedings no doubt can be obviated by their being simply presented and sent round, with labels indicating such points of interest as belong to them. With regard to the subjects comprised under the second and third heads, it is very certain that the description of many of them may be more condensed when read before the meetings, whilst fuller reports may be reserved for the Transactions. On this subject might I quote, for our guidance, the advice of one who was himself a distinguished speaker and writer, when he says—

“Concise your diction, let your sense be clear,
Nor with the weight of words fatigue the ear.”

By such means, and by others of minor detail which your Council is likely to propose, I hope and believe that our meetings will still maintain that interest which has rendered them from their commencement continuously attractive.

In conclusion, let me say that we must not look on the object of our pursuits as specialists, nor on the specimens brought before us as objects of mere interest, however great, or of curiosity however striking or remarkable. All we do here should be done in relation, if not in absolute or indicated connexion, with the other departments of our work. When we see the remarkable objects submitted to us in such rich abundance, we may be attracted by the unique character of some, by the great bulk of others; some may claim attention as illustrating rare skill in diagnosis, others by the rapidly destructive character of

their action; some attract notice in consequence of the success with which their nature has been investigated and made clear, others by the obscurity which still surrounds them in spite of all investigation. These points are all worthy of notice; but there is something more worthy still. Be it remembered that these diseased specimens are all so many illustrations of the weakness of our art. If our means of cure were more efficient, our specimens would be fewer. It is true that there is a profound interest in studying, as morbid anatomists, these various subjects; but let us not be fascinated by our pursuit. Let us remember that the duty, the aim, the ambition of our science is to cure disease, and let us look always on these specimens to that end. We may, as we go on, with the mental eye connect them with their origin and their results; we may consider their physical conditions as well as their effects on organs and textures, and thus deduce from their clinical histories their physical signs and their symptoms. Let us thus make our morbid anatomy subservient to our diagnosis; still more so to our methods of treatment and cure. Thanks to morbid anatomy, thanks to the physical sciences, diagnosis can hold its own. Would that we could say the same in reference to the treatment of disease! This is our weakness; here we fail. But, gentlemen, there are grounds for hope that this failure will, ere long, be converted into success, and that therapeutics will keep pace with pathology and diagnosis. We “hear want of faith in physic,” too often talked of and too hastily accepted. Beyond all question, the treatment of disease on general principles has improved equally with the other departments of our science. But we aim beyond this: we seek for specifics, and when we fail to find them, we hastily express a want of faith in physic. It seems to me that such conclusions are premature; that, if we knew more of remedies and their action, and if we could bring them more in direct relation with the branch of science which we here cultivate, our success would be greater, and our faith would be stronger. On all sides we see efforts made to realise these objects; we see a powerful one in the younger sister of our Society, so recently brought into existence, and for whom we desire all prosperity; we see it in the investigations by accomplished physicians going on around us; let us wish them, too, all success. And finally, gentlemen, let us hope that the time is not far distant when our treatment of disease will be alike scientific and successful, when our profession will rank as high for its scientific character in the treatment of disease as it is now unselfish in trying to prevent by sanitary laws its occurrence and its spread.

ON LOSS OF SPEECH, OR THE POWER OF UTTERANCE, IN RESPECT TO ITS CERE- BRAL BEARINGS AND CAUSES.*

By ROBERT DUNN, F.R.C.S.

Loss of voice may result from a variety of causes, and be dependent upon differing pathological conditions, quite independent of any cerebral lesion or implication of the intellectual consciousness. For instance, aphonia is not unfrequently the consequence of disease of the larynx or vocal organs; at other times, it is occasioned by pressure upon, or change of structure in, the lingual and glosso-pharyngeal nerves, either at their origin or in their course. Dr. Schroeder van der Kolk, indeed, has been led to infer, from the close and intimate connexion between the corpora olivaria and the nuclei of the hypoglossal nerves, that the former are auxiliary ganglia to the latter in the production of certain combinations of movements in the acts of articulation and speech. “For,” says he, “speech and the articulation of words require such a multitude of peculiar motions of the tongue, and such an infinite number of varying combinations of its muscular movements, that it cannot appear strange that two auxiliary ganglia should be required for the performance of these functions.” After further remarking that “he is acquainted with no other part of the entire spinal cord or brain where the multipolar ganglionic cells are so densely accumulated as in the nuclei of the hypoglossal nerves,” and stating his conviction that all motor nerves take their origin from certain groups of multipolar ganglionic cells, he adduces pathological evidence in support of his opinion that the corpora olivaria are the organs for the articulation of the voice, though he admits they are not the organs through which the will directly acts.

The faculty of speech, however, is an *instrument of thought*, and, as such, must have its seat in the nervous apparatus of the intellectual consciousness—that is, in the great hemispherical ganglia of the brain; for, as I have elsewhere said, “language implies a train of thinking. It is

* Read in the Department of Physiology at the meeting of the British Association for the Advancement of Science, at Norwich, August 1868.