

allowed the rest the lecturer so ably advocates, and the extreme pains through all the movements of the wrist disappeared.

I venture to add a word or two on "sciatica," of which you give, in the same issue, an interesting and learned account from a recent German writer on that subject. Perhaps I may be pardoned for referring in this connexion to my experience, deduced from a record of nearly 500 cases of sciatica, that such a combination is so frequent as to be present in something like 1 in 6 of all cases of sciatica. Is there any real anatomical relation between the fibrous structures of the sacral region and those emerging from the pelvis in connexion with the sciatic nerve? I humbly hold that just as I have said brachial neuralgia takes its origin from the deposition of sodium urate in and about the shoulder-joint, so does sciatica start from similar deposits in and about the hip-joint. Further, so also most probably does lumbago owe its symptoms, recurrence, and too frequent persistence to the pathological deposition, temporary or otherwise, of minute particles of urates on the opposed surfaces and in the sheaths of the tendons connected with the articulations of certain of lumbar vertebrae.—I am, etc.,

Strathpeffer Spa, Jan. 17th.

WILLIAM BRUCE.

P.S.—May I add a word or two on one point referred to by Sir W. Gowers—namely, the sudden onset and sometimes the almost equally rapid disappearance of these pains? Reference to Garrod (p. 191, edition 1876) will show the possibility of washing away the deposit of urates from an affected joint *post mortem*. May not this help to explain the complete cure by absorption of the deposits of urates—"digestion" he very happily names it—in many of these cases? Another and more probable explanation is the alteration produced in the relative position of the deposits, just as takes place in floating loose cartilages in the knee, by movements of the articulations, and the effect of taking the recalcitrant muscles by surprise, as is well known occurs in dislocations, fractures, and in cases of long-standing "hysterical" joints by bonersetters and other more legitimate practitioners.

SIR.—In reading the lecture on lumbago delivered by Sir William Gowers, in the BRITISH MEDICAL JOURNAL of January 16th, I was very much surprised to find that he made no mention of a symptom of true lumbago which is always present, namely, the passage of a small amount of urine containing a large quantity of lithates. Neither did he refer to the well-recognized treatment of the complaint by full doses of belladonna and potassium bicarbonate, which I think I may say never fails to relieve the pain in the back and "clear" the urine at the same time. It is an interesting fact in connexion with lumbago, which I suppose every one has observed, that the pain in the back does not disappear until the urine becomes normal.—I am, etc.,

Minster, Ramsgate, Jan. 18th.

A. M. WATTS, M.D.

SIR.—I read Sir William Gowers's lecture on lumbago with great interest. I am not sure, however, that I did not misunderstand him when he wrote that in inflammations of the fibrous tissue of a muscle there is no indication of the formation of inflammatory products or indurations.

I think that on careful palpation of the affected muscle it is possible in almost all cases to discover distinct alterations in its structure. In acute cases the muscle feels less elastic, and has a "doughy" consistence as if infiltrated with serous exudation. In the more chronic forms one often feels distinct hardenings in the muscle, extremely tender to the touch. In lumbago these are somewhat difficult to diagnose, as the most frequently-affected muscles—erector spinae and sacro-lumbalis—are covered by the thick lumbar aponeurosis. But in cases of sciatica it is very common to find circumscribed hardenings scattered over the glutei. Other muscles frequently affected by "fibrositis" are the neck muscles and trapezius, and this is often the pathological condition found in occipital headache.

As a student I was not taught to palpate the muscles in rheumatic affections. This I have since learnt in Sweden. It requires a good deal of practice, as the condition in the muscles may easily be confounded with a very common affection of the subcutaneous tissue under the skin (the cellulites of Swedish authors). These are small tender indurations often found under the skin of the arm and abdomen, and must be distinguished from the physiological lobulation of the adipose tissue. "Cellulites" of the abdomen have not infrequently been mistaken for diseases of the visceral organs. The easiest way to discover pathological alterations in a muscle is

by having the skin over it well lubricated, and then passing the tips of the three middle fingers deeply along the muscle both longitudinally and transversely. Some superficial muscles can be grasped and kneaded between the fingers. Some Swedish authors believe that those cases of muscle inflammation caused by excessive muscular exertion are due to imperfect absorption of the "fatigue stuffs," which, remaining, act as irritants and produce the inflammation. The majority of chronic muscle indurations can be cured by deep friction-massage over the affected areas, but this treatment can of course only be given by a person capable of diagnosing the condition. In Sweden it is, therefore, frequently undertaken by medical men.—I am, etc.,

London, W., Jan. 18th.

G. B. JACOBI.

SIR.—Sir William Gowers's lecture on the nature of lumbago and its analogues cannot fail to interest many of his professional brethren, for personal reasons. Common diseases which present such obstacles to thorough pathological research demand all the more a careful study of their symptoms, a study which is more likely to bear fruit when a personal interest is added to the scientific.

Muscular "rheumatism" appears to be a good name to embrace lumbago, stiff-neck, and kindred disorders, if one regards the original meaning of the word and its reference to a humoral pathology; but a bad one if it is taken to mean any near kinship with acute rheumatism, which is probably, or certainly, a disease of microbic origin. Sir William lays stress on its connexion with the gouty diathesis—its prevalence among those whose metabolic processes are easily disordered, people who pass habitually a highly-concentrated urine which on fatigue or the use of certain articles of diet becomes loaded with lithates. We see here a link between muscular "rheumatism" and the semi-physiological stiffness following on unaccustomed exertion.

My personal experience has not extended to lumbago, but has been confined chiefly to the variety of "stiff neck" due to involvement of the upper section of the trapezius and the pre-vertebral muscles (? longus colli); and here it appeared to me that one of the symptoms pointed to a neuritis or neuralgia of the sympathetic system as the essential lesion. I refer to the dull, vague, aching pain extending down the arm of the corresponding side in the distribution of the ulnar nerve.

This recalls the shooting pain produced by thoracic aneurysm and some cardiac affections, which is generally considered to be due to mechanical or other interference with the sympathetic fibres, and is evidently of a referred character. Sir William Gowers finds the sensory nerves of the muscles at fault. Is it not possible that all the afferent nerves of normally insensitive structures—for example aponeuroses, intestine—belong to the sympathetic system? In the case of the trapezius affection the cerebro-spinal nerve supply evidently fails to furnish the clue to the wide distribution of the pain. And can we be certain that the stiffness of the affected part is really the result of a massive lesion? Is it not rather the indication of involuntary hypertonicity, such as can be observed in the abdominal wall in internal inflammations?

In the treatment of these conditions, rest is assigned the place of honour by the lecturer. It is quite certain that perfect immobility and perfect relaxation in the dorsal position will remove the pain of "stiff neck," and indeed rest has always appeared to me a much more efficient remedy for general muscular stiffness than "walking it off." This is negative treatment, of course; of positive measures, counter-irritation has proved of little use, and we have no right to expect much from salicylates in a complaint simply because we choose to call it by the same name, as one totally different.

The simplest and most satisfactory treatment I have found to be sipping a good quantity of cold water—distilled for preference—and this on the theory that the first thing to be done is removal of a toxin. The object of taking it cold and slowly is to flush the kidney without acting on the skin. Bouchard has shown that the kidney is vastly the more efficient emunctory for the majority of toxic substances.

I may just add that in those predisposed, the position in bed may have something to do with determining an attack, which, as Sir William points out, is felt so frequently after a night's sleep. From the relief experienced in the flat position I continued to sleep with only a single pillow, and since then I have had practically no recurrence. This may be a *post hoc* rather than a *propter hoc*, but Sir William's remark on the

time of incidence lead one to think that imperfect muscular relaxation, or tension during sleep, may be sufficient to determine an attack.—I am, etc.,

Cheltenham, Jan. 18th.

ERNEST CHRISTISON CARTER.

#### CANCER AND ITS ORIGIN.

SIR,—In the BRITISH MEDICAL JOURNAL of December 26th, 1903, p. 1664, there is an abstract of some observations by Messrs. Farmer, Moore, and Walker. They have been investigating karyokinetic processes in malignant tumours, a field of investigation in which I, too, worked extensively some years ago. Since my observations do not seem to be familiar to the authors, who are, I believe, botanists by profession, I should be glad if you would give me space in the JOURNAL for the following remarks.

These authors have found that in the cell development of malignant tumours the mitotic figures exhibit certain definite differences from those occurring in normal tissues. The essential alteration lies in the fact that the cells undergo a reductive division similar to that occurring in the formation of sexual cells, whereby germinal tissue is formed. The authors state explicitly that the cells do not become embryonic—in fact, differ from these through the small number of their chromosomes. In addition, the chromosomes are also changed in shape. These are all observations coinciding more or less with those described some time ago by me in various publications—for example, Virchow's *Archiv*, vol. cxix, 1890, p. 299; *Archiv f. mikroskop. Anatomie*, vol. xliii; the *Festschrift für Virchow* in 1891; and in the *Verhandlungen der anatomischen Gesellschaft*, 1891, p. 255; and elsewhere. I have given a connected account of these processes in my monograph entitled *Studien über die Spezifität, den Altruismus u. die Anaplasie der Zellen* (Berlin, 1893), and reverted to the subject in my book on *Die mikroskopische Diagnose der bösartigen Geschwülste* (2nd edition, Berlin, 1902), pp. 91 et seq.

According to my investigations, it is not a question of a reduction to exactly one-half the normal number of chromosomes, nor does the reductive division occur in the manner seen in the maturation of ova and spermatozoa. According to my observations, the reduction is effected in a twofold manner: either by asymmetrical nuclear division, or by a destruction of individual chromosomes without destruction of all the nuclear substance. The first result by either of these two processes is cells with a smaller number of chromosomes than normal. I have called these cells hypochromatic. In the further stages of development these chromosomes may be doubled, quadrupled, sextupled, and so on, so that from hypochromatic hyperchromatic cells may arise, but always with a smaller than the normal number of chromosomes of human cells.

I have sought to correlate with these observations the peculiar characters which the cells of malignant growths possess, especially their greater power of independent existence and their loss of differentiation as compared with the normal tissue cells. The combination of these two properties I have termed "anaplasia," an expression now very generally employed in the scientific world, even by those investigators who do not altogether agree with the details of my observations on the cell processes.

I have also given a detailed account of the change of form of the chromosomes, and have shown how extraordinarily manifold their form may be in malignant tumours, which in nearly all cases differ not only from normal tissues, but also amongst each other, so that not only in different cancers, but in one and the same the most varied forms of chromosomes may be found.

Since these investigations are perhaps but little known in England, I am naturally anxious, in reference to investigations of Messrs. Farmer, Moore, and Walker, to guard my priority of observation in this field.—I am, etc.,

Berlin, Jan. 15th.

Professor D. v. HANSEMANN.

#### THE TREATMENT OF PNEUMONIA.

SIR,—Since reading Dr. Lees's articles on the treatment of pneumonia, I have had practical experience of their great value. I have recently had under my care two children aged about 3 years. They were suffering from bad broncho-pneumonia after measles. They were deeply cyanosed; the pulse was very rapid and very weak, and the right auricle extended two fingerbreadths to the right of the sternum. These children were living in the vitiated atmosphere of small, overcrowded rooms, and I have never before seen children similarly circumstanced recover from the condition in which I

found them. To relieve the gorged, distended right heart I applied four leeches over the right lower ribs. When the leeches had fallen off I ordered hot water to be applied for some hours to the leech bites. In this way we got away a considerable quantity of dark venous blood. The result was to me a revelation. The children, who had been sleepless, slept all that night and late into the next morning. When at last they awoke the cyanosis was gone, the pulse was good, and the distended heart had contracted to its normal size. The children were, in fact, convalescent and recovered.

Dr. Barr, in criticising Dr. Lees, makes some remarkable statements which I can only explain by supposing that he has forgotten his anatomy. He says, "Leeches abstract blood in the course of circulation from the capillaries, and possibly in some cases from a small vein, but, from whichever source, the blood comes immediately from the left side of the heart, and not from the right. In venesection from the median basilic the bleeding is directly from the arterial tree."

Surely any medical student could tell him that the blood in the capillaries and small veins between the ribs passes into the right auricle through the intercostal veins, the azygos, and the vena cava. So also the blood in the median basilic is hurrying on towards the right auricle, and to extract 6 oz. or 8 oz. of blood from these veins is to prevent 6 oz. or 8 oz. from entering the over-distended right auricle and thus give it an opportunity to recover itself.

Dr. Lees refers to the value of belladonna, but I do not think that he lays sufficient emphasis on the great value of large doses of this drug in certain cases of bronchitis and broncho-pneumonia. The cases to which I refer are those in which there is a dangerous excess of mucus in the bronchial tubes. These patients die drowned in their own secretion. In large doses of belladonna we have a life-buoy that will save many of them. Patients taking belladonna often complain of dryness in the throat. It is this dryness, not only in the throat, but in the bronchial tubes, that we want to produce, and large doses of belladonna will do it. Of this I have ample experience. My last serious case was an old gentleman 80 years of age. He had bad bronchitis. His bronchial tubes were swamped with mucus. He was cyanosed, pulseless, and apparently sinking into stupor and death. I gave him 20 minims of the tincture of belladonna in one dose. This made him delirious, but after a little delirium the mucus disappeared, and he rapidly recovered.

I may add that alcohol, which is generally given in these cases, does nothing but mischief. Professor Sims Woodhead has proved that alcohol causes congestion and engorgement of the mucous and submucous membranes of the bronchial tubes. This effect of alcohol is the very opposite of that which is produced by belladonna, and explains why so many alcoholic subjects are also the subjects of chronic bronchitis.

I will conclude by referring again to the articles of Dr. Lees and expressing my conviction that they are the most valuable contribution ever made to the therapeutics of pneumonia.—I am, etc.,

London, W., Jan. 17th.

J. McNAMARA, M.D.

#### SIPHONAGE AND HYDRAULIC PRESSURE IN THE LARGE INTESTINE.

SIR,—While thanking your reviewer for the able manner in which he has condensed a considerable portion of my views on this subject, I should like to traverse his opinion that retraction of the anus in the young adult is due in large measure to the action of the pelvic fascia and the levator ani. There are three conditions in which retraction of the anus is emphatically absent—early infancy, the typhoid state, and death. Now, since none of these conditions can affect either the muscle or the fascia, why should retraction be absent? On the other hand, tonic contraction explains its absence perfectly. In early infancy the taeniae, like the sphincter ani, have not yet acquired tonicity; in the typhoid state there is paralysis of the musculature of the intestine, including its longitudinal fibres; in death, tonicity is of course lost. After all, retraction is only one of the many arguments in favour of tonicity. It is difficult, for instance, to explain otherwise the fact that when the abdomen is opened *post mortem* the transverse colon, averaging 20 in. in length, is found reposing peacefully and without undulations on a bed measuring 10 in. or less. The subject of my book is by no means one of pure academic interest; on the contrary, it is of the highest importance not only to our patients but to ourselves.—I am, etc.,

London, Jan. 18th.

RALPH W. LEFTWICH, M.D.