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CANADA

MEDICAL JOURNAL.

ORIGINAL COMMUNICATIONS.

Lectures on Joint Diseases.—By LOUIS BAUER, M.D., M.R.C.S., England.

III.

CLINICAL CHARACTER OF JOINT DISEASES.

(Continuation from page 440.)

The division of joint diseases into acute and chronic forms, is rather inappropriate, because artificial. It is apt to confound the character of the affection, and has no practical value in any respect. Whether the duration of the malady, or the violence of the symptoms is the principle of division we shall find neither to be tenable.

Almost every joint disease assumes a *protracted course*, and is thus essentially *chronic*. But few exceptions can be adduced to this rule. Rheumatic synovitis may be of short duration, and characterized by violent symptoms, but joints thus affected will require months to recover their normal status. On the other hand, we observe periods of acuity, in the most chronic and protracted joint diseases, which may challenge the most acute forms known.

I suggest, therefore to drop a clinical dogmatism, worthless to the experienced surgeon, and confusing to the novice.

The symptoms by which *synovitis* is characterized, materially vary, both, in duration and intensity. We need scarcely adduce the general symptoms of this disease, having already alluded to them on a prior occasion.

The chief, and pathognomonic phenomenon, is *effusion within the articular cavity*, and rapid change in the contours of the joint. From the physiological character of the structure, effusion, should, a priori, be expected, as clinical observation substantiates it.

To speak of a *dry joint* in these affections is an absurdity. The most insignificant irritation of the synovial lining, is attended with *copious secretion* of a fluid, with the peculiarities of synovia. The higher grades may not exhibit the same quantity of morbid secretion, but enough to give definite fluctuation. The liquid is of a more plastic nature, contains blood corpuscles, flakes of fibrin, fat globules and epithelium and becomes early contaminated by the organized elements of pus. To a certain extent the composition of the synovial fluid may still be recognized by the abundance of alkalis and the soapy feel.

In the highest grade of synovitis, the synovial lining, is as you are aware, converted into a pyogenic membrane, and presents the structure of granulations, as stated in the preceding section of our discourse. Under all these conditions, there is more or less morbid effusion.

The dryness of articulations cannot be denied, but it is noticed in conditions of a different character, and independent of inflammatory affections of the synovial lining. Thus, for instance, it complicates progressive deformative arthritis, which originates in the articular faces of the bones and though the synovial membrane may gradually be compromised, it is affected in such a manner as to destroy its character as a secreting structure.

In white swelling, the synovial membrane sometimes presents the peculiarity of dryness, but from anatomical changes of a pulpy character, not the result of direct inflammation.

In pure synovitis we never observe consecutive intumescence, infiltration, or hardening of the surrounding tissues, and never to such an extent as we find it in diseases of the periosteum, and the osseous structure, unless indeed the latter have become involved.

In the more active forms, there is intense pain within the whole joint, with consecutive febrile excitement; but reflex pains are moderate, and the spastic oscillations never very intense. In the lower grades of synovitis (*Hydrarthrosis*), these symptoms are entirely wanting, and the patient suffers scarcely any other inconvenience, than the effusion within the joint would naturally occasion.

The affections of the periosteum and of the epiphyses, are attended by a widely different group of symptoms. The beginning of these diseases is *very insidious*, and their development so slow as to require months to assume a noticeable form. But little pain attends the initiatory period. The whole trouble marks itself as *weakness* of the limb, dryness and *stiffness* of the joint, with inability to use the extremity in the morning. For a time the contours of the joint suffer no change; and if there be any fulness at all, it is more generally diffused, and extends beyond the

limits of the articulation. There is no discoloration of the integuments, though there is frequently that *waxy whiteness*, the result of œdema; whence the term "white swelling." The latter is often the first symptom which attracts attention. Though the patient may have the sensation of heat in the affected parts, it is not *objective* either to the hand or thermometer. The patient may gradually experience some difficulty in using the articulation to the fullest extent, feel induced to spare the extremity in locomotion, and thus favor certain positions as a source of greater comfort; malposition is superadded only at a later period.

The *advance of the disease* is marked by progressive swelling of the periarticular structures: the contours of the joint disappear, not from effusion within the articular cavity, but from infiltration of the surroundings and therefore no fluctuation can be discerned.

Contemporaneous with the enlargement of the articulation, the original feeling of soreness, increases to aching pain, being augmented by pressure and locomotion; the rest becomes disturbed by reflex pains, and the limb forced into a position over which the patient loses all control. Every attempt to alter the same is attended with aggravated suffering.

When the swelling and firmness of the soft parts still more increase, then the pain assumes a torturing character. The limb attenuates and becomes cooler, whilst the swelling shows but a moderate addition of temperature.

In viewing the affected extremity, the contrast between *the waste* of the limb, and the *general enlargement* of the articulation, with its numerous distended veins, is strongly marked, and it is this form of articular disease, which in times past was designated as *fungus articularum*, *tumor albus*, and *white swelling*. It was thought to be of malignant growth, and amputation its only remedy.

Thanks to the progress of pathological anatomy and the material aid of the microscope, this error of our ancestors has been effectually dispelled.

Now-a-days, white swelling has been recognised as an affection of the articular ends of bones, and their respective periosteum; with subsequent periarticular infiltrations of seroplastic material, with its attending organization into fibroplastic cells, fibrous structure, fat, &c. And surgery offers the means of relief as long as the pathological changes are susceptible of reduction.

The knee joint is most frequently visited with this disease, and it is there one can best study its different phases.

On a former occasion I have assigned the reasons why this malady

attacks the knee joint more frequently than any other, and likewise why the disease is more frequently observed in childhood than in adult age: and therefore need not recur to that subject.

I shall now confine my remarks to the discussion of some features that characterize the process under consideration.

One of these points is the extraordinary slow advance of the disease. Some authors think that a low grade of nutrition of the structures primarily involved, offers an acceptable explanation. On close reflection we shall find this view inadmissible, and contradictory to analogy. Nutrition in childhood is more exuberant than at any later period. In the former, maintenance is not the only object of the nutritive process; it is enhanced by growth and development, demanding more ready supply, and meeting with the most elastic condition of the vascular carriers of that supply. In these advantages the infantile skeleton participates to a higher degree than the other systems of the organism.

Hence from a physiological point of view, we have to reject the advanced theory.

In questioning analogy, we notice facts which demonstrate beyond a shadow of doubt, the prolific character of nutrition in the osseous system of children. Fractures consolidate more rapidly with them than with adults; artificial joints are scarcely ever observed during the period of evolution; if periostitis has laid bare the bone of a child, exfoliation rapidly ensues, and sequestra form much more quickly than at a later period. These facts coincide with the experiments of Flourent and Wagner, and dispose effectually of the before mentioned hypothesis.

In all those cases of white swelling, that I have had the opportunity of anatomically investigating, and they have been numerous, I have observed that there is always, in one or the other condyle, an insular disintegration of the cancellated structure, in which sometimes a small sequestrum is imbedded. Under the microscope scarcely any trace of the vanquished structure can be discerned. The chief element is fat. But in the neighbourhood of this pathological focus, hyperaemia, traces of fungoid granulations, and osteoporosis are noticed. This condition explains satisfactorily, the proximate cause of the pathological changes inconsistent with the active process of ostitis. In some rare instances, however, the healthy portion of the bone surrounds the disintegrated isle with a sclerotic capsule, by which the affected portion becomes, as it were, isolated and rendered innocuous, in a similar manner as foreign bodies in a capsule. This pathological condition may not cover all cases which pass under the name of *tumor albus*, but certainly this is the most prevalent.

There is a specimen in my collection, of the lower third of a femur of a young girl not exceeding fifteen years of age. She was admitted to the Brooklyn Medical and Surgical Institute, with all the symptoms of white swelling, comprising the articulation and peri-articular structures; the swelling however likewise involved a portion of the femur. The local disturbances were as intense as were the nocturnal pains, and the spasms of the flexor muscles. The knee was of course drawn to a right angle.

From the history of the case, and the clinical character of the disease, *circumscribed osteomyelitis*, with its termination in abscess, was diagnosed and in view of her reduced constitution, and the copious discharge of matter from the neighbourhood of the joint, amputation was deemed expedient.

The condition of the specimen fully confirmed the diagnosis. There is a large pyogenic cavity at the lower end of the femur, which opens at the posterior aspect of the bone, by an irregular aperture not less than an inch and a half in diameter; in the circumference of which, the periosteum is raised up, and its internal surface covered with new bone. The epiphysis is somewhat loosened from its attachment, and in time would have become separated.

The original focus of the disease had been obviously limited to the cancellated structure, and rather remote from the joint, but its consecutive effects had extended over the joint, and involved its soft surroundings. There may be still *other exceptions* from the anatomical prototype, but their numerical proportions scarcely affect the statistics.

The adherents of the tubercular theory, may rejoice at this pathological admission of mine, of those insular and circumscribed pathological foci, which they may claim as *bona fide* evidence of tubercular deposit.

I hold however, that pathological detritus, limited to an isolated place, cannot in the eyes of competent judges, pass as tubercle.

If the disease is permitted to spread, it eventuates in perforation of the articular cavity; the formation of external abscesses and fistulous tracts, and the more obstacles the discharge has, the more periosteum will be destroyed, and the bone corroded on its surface.

The protracted development of these phases extends over many months, and often additional injuries are required to accomplish so extensive disintegration.

A lull of all symptoms, is often observed in the like cases, to be followed by new exacerbations. A goodly number recover spontaneously, or by appropriate treatment. These recoveries happen not rarely at the period of puberty, at which time the mode of nutrition of the epiphyses becomes perfected.

In analysing the gradual development of this disease, its preceding cause, (traumatic injuries); the comparative moderate effects upon the integrity of the adjacent osseous structure; we find a more passive pathological condition, a direct necrobiosis of the affected structure, more from want of proper maintenance, than from active and progressive disease. When active symptoms subsequently set in, they are the efforts of the *vis medicatrix nature* to eliminate the detritus foreign to the integrity of the bone. Frequently the detritus becomes absorbed, or pervaded with calcareous elements, and thus recovery is attained.

This gradual change of the osseous structure and annihilation of its nervous and vascular endowments, though limited in extent, renders it intelligible why so little pain is experienced by the patient, during the first disintegrating period of the disease. The intense pain that is at a later period superinduced, is evidently connected with the peripheral and active process of osteitis arising in the circumference of the focus. The original disease has nothing to do with it.

The appearance of nocturnal pain constitutes a serious complication and indicates the commencement of suppuration.

The contraction of the biceps muscle is quite common and the result of reflected spasm. The leg is thus held in an angular position to the thigh, and most usually rotated on its longitudinal axis, with eversion of the toes. This position goes *pari passu*, with an anatomical derangement of the joint itself. The patella rides upon the external condyle of the femur, and is generally adherent; the internal condyle of the tibia projects in front, whilst the external one recedes.

The contraction of the biceps is exclusively accountable for this malposition, for at a certain angle it acts as a rotator, when not counteracted by the simultaneous contraction of the internal hamstrings.

I have but lately exhibited to the New York Pathological Society a specimen of this kind, and the action of the biceps, is so undeniably demonstrated, that there is no more room for further speculation to account for the symptoms.

For a long time the mobility of the affected joint remains, if not impeded by the contraction, but when synovitis is superinduced to the original affection, the joint may become obliterated by fibrous adhesions between the articular faces, which may still more impede the mobility, but rarely are there osteophytes passing from one bone to the other, depriving the joint of all vestige of motion. True bony ankylosis is of very rare occurrence, and much more the consequence of penetrating wounds of the joint, and high graded synovitis, than of this form of disease.

Whether the disease originates in the synovial membrane, in the crucial ligaments, in the periosteum, or the epiphysis of the joint, the symptoms appertaining to each of them respectively, will be so blended in their advanced course, as to render diagnostic discrimination almost impossible, leaving the previous history as the only guide.

The pathological conditions of joint diseases vary but little, when suppuration, burrowing of pus, has been going on, and the bones have been disintegrated for any length of time; the symptoms attending those conditions are almost uniform in all such cases. The competent and experienced surgeon may yet recognize the patho-genesis of the original disease, but novices rarely realize differences so indistinct and subtle. Thus, in caries of the joint emanating from synovitis, the articular surfaces are more generally denuded of their respective cartilaginous coverings, but the osteo-porosis does not much exceed the surface; the crucial ligaments are but partially destroyed; the semilunar cartilages partly disintegrated, discolored, and mostly detached. On moving the articulation, crepitus is discernible. If, however, the bone has been the starting point of the disease, the caries of the articular surface is generally restricted to the originally affected locality; and the cartilage is there and thereabout disintegrated. The crucial ligaments are mostly destroyed *in toto*, and crepitus is less distinct.

The clinical character of *hip disease* will now demand attention, on account of some peculiarities in its manifestations. *Morbus coxarius* is about as good a term as could be chosen and certainly more appropriate than "*coxalgia*" which applies solely to the pain of the affection.

The first stage of this lesion materially conforms with the same stage of the affections of other joints. The only symptom requiring special mention, is limping. It is most noticeable in the morning, less during the day, and least towards evening; most conspicuous after great exertion, and sometimes absent after a day of complete rest. The duration of this period is variable; repeated accidents and the continuous use of the affected extremity may shorten, and constant rest prolong it.

The so characteristic pain at the knee, may already make its appearance at this stage, but if so, there will be likewise indications of retracted muscles, with which this symptom appears conjointly. This pain has often confounded the diagnosis of the less experienced, without any need; for you may press and squeeze the knee joint as you please, without the slightest increase of that pain, whereas the pressure upon, and movement of the hip joint will aggravate it. The progress of the malady may, at this juncture be arrested, and the patient relieved from further trouble.

The second Stage is characterized by elongation, abduction, eversion and slight flexion of the affected limb at the hip, with lowering of the pelvis, flattening of the gluteal region, sinking of the gluteal fold, and an inclination of the internatal fissure, at, and towards the affected side. The mobility of the joint may either be impeded, or entirely suspended. Adduction is generally impossible.

For the purpose of locomotion, the patient brings the lumbar portion of the spine and the other hip joint into play; thereby easily deceiving the inexperienced observer. In the erect posture the spine exhibits a single curve, of which the convexity corresponds with the seat of trouble. The superior spinous process of the ilium, is depressed when compared with that of the other side, and the healthy member is adducted in proportion to the malposition of its afflicted fellow. In walking, the patient places the latter forward and outward, and drags the other limb after it in a rather diagonal direction. All these symptoms more or less complete, can be ascertained by undressing the patient; dropping a plummet line from the occipital protuberance, walking, and by careful examination in the horizontal posture. If the patient sits down in such a manner as to accommodate the affected member, both pelvis and spine assume normal relations, thus proving that the elongation of the limb does not depend on the lateral declivity of the pelvis, as *Gross asserts.

The chief or proximate cause of the entire group of symptoms rests with the immobility of the joint and the fixed adducted position of the extremity. In imitating them we produce the very same effect.

There can be no doubt that the elongation is but apparent, and not real, as the late professor Rust of Berlin, claims. Nor is there any enlargement of the head of the femur, from either tuberculosis or other causes, to which he ascribes the actual elongation. The sole source of the symptom is hydraulic pressure from existing intra-articular effusions; I was led to this view from the analogous position of the femur and the immobility of the joint produced by experimental injection. Acting on this supposition, I have succeeded in substantiating the correctness of my opinion, by paracenteses of the articular cavity. The removal of the intra-articular fluid was followed immediately by returning mobility and the correction of the malposition. This point is consequently settled by demonstrable evidence.

With the apparent elongation of the limb, the structural pain gradually increases, and the reflex symptoms rapidly rise to an intense degree. The nocturnal pains, in this period are more violent and torturing than

*Gross' "Practical Observations" Philadelphia 1859.

at any later, and for obvious reasons. Whilst the extremity is immovably fixed by hydraulic pressure, the adductor muscles are nightly agitated by reflected spasms, and kept on the stretch. The limb becomes attenuated and exhibits marked disproportion with its fellow, the constitution, rest, appetite, suffer gravely, and reduce the patient in weight and appearance. The effusion may still be of a plastic and organizable character; sero-purulent, or exclusively pus: may be free from, or contaminated with structural detritus, benign or destructive. Its composition will naturally determine the issue of the case. If the effusion be mild, plastic, benign, free from deleterious admixture, its partial absorption and final organization into fibrous structure may take place and thus terminate the malady. Or its quantity may lead to a disruption of the capsular ligament, and the escape of the intra-articular effusion into the surroundings of the joint, and there become organised and innocuous. Through similar changes the sero-purulent effusion may pass with the same result.

But if the articular contents are of a destructive character, they may, by macerating and corroding the acetabulum pass into the pelvic cavity through the cotyloid notch, or through the capsular ligament, and will invariably give rise to the formation of abscess, corresponding in locality with the place of perforation.

In the moment the perforation is effected a new series of symptoms appears, and with which the third stage of the disease is ushered in:

The third stage is distinguished by diametrically opposite symptoms. The contrast of the two stages can best be realized by placing them in juxtaposition.

Second stage.

Affected limb.

Apparently elongated.

Abducted.

Flexed at hip and knee.

Toes everted.

Foot fully on the ground

Healthy limb adducted

Pelvis lowered.

Pelvis projects forward.

Pelvis angle of inclination acute.

Nates flattened.

Gluteal fold lowered.

Internatal fissure inclined to affected side.

Spine curved on the affected side

Nocturnal pain very intense.

Third stage.

Affected limb.

Apparently shortened.

Adducted.

Flexed at hip.

Toes inverted.

Ball of toes only.

Abducted.

Tilted up.

Backward:

Almost rectangular.

Full and convex.

Elevated.

Inclined towards the opposite side.

Curved towards the other side.

Greatly diminished.

It will be seen that the third stage is characterised by unmistakeable clinical manifestations, and by so peculiar a gait of the patient, as to be recognised at a distance.

The shortening, adduction, and inversion of the limb, conjointly with the rotundity of the gluteal space, strongly convey the impression of posterior superior dislocation of the femur. This similarity of the two may have led Rust to presume their identity, and ascribe to the action of the contracted muscles the cause of *spontaneous dislocation*. The morbid enlargement of the caput femoris, said to exist (at the second stage) lent a plausible argument to this hypothesis. What was more simple and transparent, than that the head of the femur partially expelled from the acetabulum by its disproportionate size, should leave it entirely and follow the undue traction of the muscles. This hypothesis of the renowned German surgeon prevailed among the profession; spontaneous dislocation was henceforth a settled fact, against which but heterodoxy could raise its voice. Buehring, of Berlin, if I do not mistake, was the first who took issue with Rust's theory, and attempted to reduce the acknowledged similarity of symptoms to causes widely different from those propounded. In this effort, he derived material assistance from the advancement of pathological anatomy. The question once opened has received a rational solution. At this present moment there are few well informed surgeons who recognize spontaneous dislocation. Nelaton has informed us of a good method to decide the relative position of the femur to the acetabulum. In drawing a line from the anterior superior spinous process of the ilium, to the tuberosity of the ischium, it passes on its way, from one point to the other, the apex of the large trochanter, in the normal position of the femur. It crosses the trochanter more or less below the apex in dislocation.

In applying this test in the third stage of morbus coxarius, you will mostly find the normal relations, or so insignificant difference as to preclude all possibility of dislocation. Irrespective to this clinical fact the morbid condition of these points contradict the assertion of Rust in toto. It might rather be said that the acetabulum becomes dislocated, since we often find it extending up, and backward in which direction the femur follows, but true dislocations belong to the rarest occurrences. I have searched in this respect the anatomical museums, on this, and the other side of the Atlantic, without having found more than about a dozen specimens, exhibiting the conjoined evidences of hip disease and dislocation. In this statement I am borne out by other enquirers. It follows therefore, that dislocation is but a rare incident in hip disease, indeed much more so, than might be rationally expected, considering the

actual state of the joint in many instances. If dislocation is practicable in a healthy articulation, how much more predisposed must the latter be, when the acetabulum is denuded and enlarged, the round ligament totally destroyed, the head of the femur *diminished* in size, the cotyloid cartilage more or less disintegrated, the capsular ligament broken through &c.; which all tend to facilitate the displacement of the femur. It is thus evident, that the slightest appreciable injury should suffice to bring about a dislocation, but its spontaneity cannot be conceived, and must therefore be denied. On the other hand, it must be borne in mind that the joint being more or less tender, is well taken care of by the patient and protected against incidental injuries.

One of these means is the play of all muscles by voluntary effort to keep the joint at rest, and thus dislocations are prevented, which otherwise might seem inevitable. Wherever dislocations take place, there can be no doubt as to their being the result of some injury or other, however trifling. That much I can at least assure, that I never myself have had the opportunity of observing a single case of indisputable dislocation consequent upon morbus coxarius, and I have had my finger in the hip joint too often to be deceived. If you examine a patient so afflicted, with the aid of anæsthetics, extending the affected limb, whilst at the same time exercising counter extension by placing your foot against the pelvis, you will notice a certain amount of mobility of the joint; but the absolute impossibility of abducting it. In searching for the cause, a firm and unyielding contraction of the adductor muscles will be found, over which the anæsthetics seem to have no influence whatsoever. It is thus in the third as in the second stage, the malposition of the limb is produced by a single cause, and the rest of the symptoms follow as physical necessities. Now, for instance, let us presume the femur held in undue position of adduction and flexion, and the patient attempt to walk, he would yield the pelvis as much as possible for the purpose of relieving the tension of the contracted muscles. The first thing he does is to rotate the pelvis in its transverse diameter, thus approximating the anterior superior spinous process of the ilium, to the insertion of the tensor vaginae femoris. This accounts for the enhanced angle of inclination with the horizon. By turning the pelvis on its axis at the lumbar articulations, the patient favors the former object. If the pelvis remained quite horizontal and the extremity of the healthy side rectangular to the former, the affected limb would necessarily cross its fellow, and locomotion would thus be rendered impracticable. Hence the affected side of the pelvis is tilted up in proportion to the adduction of the affected extremity, the healthy member is thrown out, (abducted) and paral-

lelism is thus achieved. If the pelvis is thus out of position, the spine and shoulders have to adapt themselves to the static changes.

In compounding the effects of these changes in the position of pelvis and femur, we can almost to a nicety, ascertain the amount of apparent shortening, without regard to the so called spontaneous dislocation. The longitudinal rotation of the pelvis will raise the extremity as much as an inch, the flexion of the femur upon the pelvis, another inch, and the obliquity of the pelvis from one to three inches. Thus the limb may be shortened in the aggregate, from three to five inches, an amount never to be produced by traumatic dislocation of the femur upon the ilium.

Most cases of morbus coxarius terminate with the third stage; but comparatively a few advance to the fourth and last stage of the disease, which is a combination of the symptoms of the third, with those of caries, abscesses, fistulous openings and tracts, in the neighbourhood of the joint, local pain, arising from such sources, and hectic fever.

Thus it will be seen that hip disease is characterized more than any other, by a certain immutable regularity and chronological succession of symptoms, which, in themselves, furnish the strongest ground for differential diagnosis. Though the first stage may escape the vigilance of the professional attendant, the second will inevitably decide his appreciation of the growing trouble. The third stage is invariably preceded by the second, and the fourth by the former stages. This, at least, has been my observation in a large number of cases, and I entertain no doubt that it is substantially the same with other accurate observers. The exceptions that may be adduced appertain to cases partly not hip disease at all, partly hip disease of a consecutive nature, and consequently blended with other pathological conditions.

Periostitis in the neighbourhood of the hip joint often produces similarities of hip disease of a most striking character. We may find in connection with it all the symptoms enumerated under the third stage of morbus coxarius, but this difference will always be manifest: that the symptoms of the second stage never preceded that condition. If the joint is not secondarily implicated in those cases there will be a freer mobility of the same, and no crepitus; whilst on the other hand, the femur is enlarged and tender.

Sometimes we meet with malposition of the femur in consequence of Potts' disease, and periostitis of the spine, which may give rise to an erroneous diagnosis. The history of morbus coxarius and affections of the spine is so differentially marked that the mistake may be easily corrected. Eventually, the application of chloroform will suffice to overcome the muscular retractions of the latter, and prove the hip joint to be intact.

We owe to Erichsen's careful investigations, our knowledge of the suppurative affection of the sacro-iliac junction, but the symptoms ad-duced by that author are so widely different from those of hip disease, that they hardly can be confounded. Eventually the careful examina-tion of the corresponding hip joint must necessarily settle all doubts.

Chemical Selections. By E. S. BLACKWELL, Esquire, Montreal.

A NEW HYDROCARBON, (C=12). C. FRIEDEL.

The existence of silicium ethyl, containing one atom of silicium and four molecules of ethyl, and having the functions of a saturated hydro-carbon, pointed to an analogous compound in which silicium should be replaced by carbon, which carbon would therefore be saturated by carbon alone. In the same manner as primary, secondary, and tertiary alcohols contain each of them one carbon atom, the four bonds of which are par-tly saturated respectively by one, two, and three bonds of other carbon atoms, and as these different alcohols may be referred to hydrocarbons of analogous constitution. So it may be presumed that a quaternary hy-drocarbon may exist, in which one carbon atom will be saturated exclu-sively by other four carbon atoms.

In methylchloracetol, resulting from the action of phosphoric penta-chloride on acetone, it is most probable that one carbon atom is united with two carbon atoms and two chlorine atoms; if, therefore, the two chlorine atoms be replaced by two hydrocarbon radicals, the quarternary hydrocarbon sought for will be obtained.

Zinc-ethyl, heated with methyl chloracetol, gives rise to enormous quantities of gas; probably propylen and ethylen. The greater part of the residue, when freed from excess of zinc-ethyl and further purified, boils between 86° and 90°, and is hydride of heptyl C₇ H₁₆, or as it may be called from the mode of its formation, carbod-imethyldiethyl. (Bull. Soc. Cheini Paris, 1867, 65.)

Pseudomorphine. (C=6, O=8). O. HESSE.

Pelletier who discovered pseudomorphine, 30 years ago, from the small quantity he obtained, was unable to give precise directions for its prepa-ration, and naturally enough, doubt fell on its identity. Hesse finds that it accompanies morphine in Gregory's method, and may be separated from that body by adding excess of ammonia to the alcoholic solution of both alkaloids; the morphine is precipitated, the other remains in solu-tion. Pseudo-morphine is tasteless, insoluble in water, alcohol, ether

chloroform, carbonic bisulphide, and dilute sulphuric acid; easily soluble in potash, soda, or lime solutions, and in alcoholic solution of ammonia, sparingly so in aqueous solution of ammonia; it does not neutralise the acid reaction of even the smallest quantity of chlorhydric acid; it dissolves in concentrated sulphuric acid with an olive green; in concentrated nitric acid with an intense orange red; in ferric chloride with a blue colour. At 120° it loses 2 eq. water of crystallization; at higher temperatures it turns yellow and decomposes without melting. Its formula is $C_{34}H_{19}NO_3$ containing therefore O_2 more than morphine, but it does not result from an oxidation undergone by morphine in its preparations; it may, however, be identical with Schützenberger's oxymorphine obtained by acting on morphine with potassic nitrite. Platino and auro-pseudo morphinic chlorides are amorphous; the sulphate is very like calcic sulphate. The oxalate, tartrate, nitrate, chromate, chloride, and iodide are described; they are sparingly soluble; the crystalline precipitate with mercuric chloride is very little soluble in chlorhydric acid. (Ann. Chem. Pharm. cxli. 87).

REVIEWS AND NOTICES OF BOOKS.

The Laryngoscope in Diseases of the Throat, with a chapter on Rhinoscopy. A Manual for the Student and Practitioner. By GEORGE DUNCAN GIBB, M.D., LL.D., M.R.C.P., Assistant Physician and Lecturer on Forensic Medicine, Westminster Hospital, &c., &c. Second Edition. Re-written and Enlarged, 8vo. pp. 158. London: John Churchill & Sons, New Burlington Street, 1867.

It will be perceived that this is the second edition of a work from the pen of Dr. Gibb, considerably enlarged and with the addition of numerous wood cuts. It evinces steady persevering research, and is a most valuable addition to the literature on this important subject. Dr. Gibb, more recently, Sir G. D. Gibb, Bart., has been for years an authority on diseases of the throat, and occupies deservedly a world-wide reputation; his experience has been very great, and in this work the author gives to the world the result of that experience. This work consists of twelve chapters.

The first chapter comprises a most interesting historical sketch of the laryngoscope, from which we learn that attempts had been made by the earlier writers on medicine to obtain a view of the internal cavities of the body by various mechanical contrivances. About the commencement of the present century, Philip Bozzini, of Weimar, produced a work upon

an apparatus for conducting light to the internal cavities and spaces of the human body.

“ His speculum, consisted of a polished metal tube of silver or tin for the various canals of the body, a certain size being reserved for the throat. This tube was divided by a vertical septum or partition; and at its curved extremity were two small mirrors, directed upwards or downwards according to the situation of the part to be examined. When introduced, the light was reflected into the mouth by means of a tin lantern, in the form of a vase-shaped box, in the opposite sides of which were circular openings—a larger and a smaller—to the larger one of which was fastened the speculum, and the smaller an eye-piece. In the centre of the lamp was a receptacle for a candle, which, when lit, allowed the flame just to reach the level of the two openings in the lamp, parallel to one another and in a line with the tube. On applying the eye to the eye-piece, the reflected image was seen in one of the mirrors at the end of the speculum, and the other mirror conveyed the light, both passing through the different channels divided by the vertical septum.”

This apparatus suggested to Dr. Cruise, of Dublin, the principles of the endoscope which may be regarded as an improvement on the instrument of Bozzini.

But it was Dr. Benjamin Guy Babington, in 1827, who first conceived the idea of using a mirror, very similar to the instrument of the present day, as we read in the report of the Hunterian society published in the *Medical Gazette*, 28 March, 1829.

“ Dr. Benjamin Babington submitted to the meeting an ingenious instrument for the examination of parts within the fauces not admitting of inspection by the unaided sight. It consisted of an oblong piece of looking-glass set in silver wire, with a long shank. The reflecting portion is placed against the palate, whilst the tongue is held down by a spatula, when the epiglottis and upper part of the larynx become visible in the glass. A strong light is required, and the instrument should be dipped in water so as to have a film of the fluid upon it when used, or the halitus of the breath renders it cloudy. The doctor proposed to call it the glottiscope.”

Other workers in the field continued from this date to use mirrors of various shapes, the author himself had been in the habit of employing a steel mirror for years before the present instrument was introduced by Czermak about the year 1860. Chapters two, three, and four, are interesting and instructive, the subjects being a description of the laryngoscope, the method of using it and the philosophy of the instrument. These chapters contain many useful hints of great practical value. In

chapter five we have the "revelations of the laryngoscope," we have also chapters on "auto-laryngoscopy;" aids to the laryngoscope; topical medication of the larynx through the laryngoscope; galvanism applied to the larynx; operation on the larynx; useful hints and general remarks on the laryngoscope, &c. There is also a chapter on rhynoscropy, its history, application, &c., together with a short statement of a few cases which have come under the observation of the author. The work is illustrated throughout, and here will be found representations of instruments which the author recommends, and which he is in the habit of using in suitable cases. This little work is eminently practical in its bearing, and all who desire to follow this department of the healing art should become familiar with its teaching.

PERISCOPIIC DEPARTMENT.

Surgery.

IDIOPATHIC GLOSSITIS.

Proceedings of Surgical Society of Ireland, February 15.

Mr. Croly stated—At the meeting of the Surgical Society held on the 18th of January, I had the honour of reading a communication on acute glossitis, illustrated by seven cases, which occurred in my own practice. By a curious coincidence, I have had (since bringing the subject under the Society's notice) another case of idiopathic inflammation of the tongue under my care in the City of Dublin Hospital. The patient was seen by most of my colleagues, and also by several medical friends. I shall, as briefly as possible, detail the history of the case, which was one of unusual severity:

About eight o'clock on the night on the 23rd January, I received a letter from Mr. David Hadden (one of the resident pupils in the hospital), requesting me to visit, as quickly as possible, a girl just admitted, who was suffering from urgent dyspnoea and symptoms of glossitis.

On arriving at the hospital I found a girl sitting up in bed, suffering from alarming dyspnoea. Her tongue filled the entire cavity of her mouth, and its convexity almost touched the palate. Saliva poured copiously from her mouth; her head was thrown back; her countenance was anxious, and she breathed entirely through the nostrils; pulse 120 in the minute. She could not speak. I ascertained, on examination, that the region of the tonsils of each side was free from swelling, and

not tender on pressure. The submaxillary region, however, was enlarged, and very sensitive to the touch. The girl could not bear any pressure on the apex of her tongue, which protruded between the teeth, and was covered with a white exudation. The breath was foetid. There was no tenderness of the gums, and the patient had not taken mercury or any other medicine lately.

The girl's mother states she was in good health until the 17th of January, when she caught cold by taking off her boots and walking in the snow. She complained of *shiverings, headache, thirst, and soreness of the tongue*. She was menstruating at the time, and the discharge was checked.

It was to me quite evident that she was suffering from acute glossitis of a very severe form, and from the aggravated symptoms I felt most anxious about her. I lost no time in introducing a sharp-pointed curved bistoury into the mouth, keeping its blade flat towards the tongue until its point reached the base of the organ, and then, having, turned the edge of the knife towards the tongue, made a rapid, deep longitudinal incision between the raphe and edge, and parallel to the septum. I quickly changed the bistoury to the other side, and made a similar free incision. The hæmorrhage was copious.

The relief to the breathing was almost immediate. I directed three leeches to be applied to each submaxillary region, and a hot poultice put on when the leeches came off. The patient tried to swallow some milk, but could not succeed. She took ice in small pieces, and enjoyed it very much. A turpentine enema was administered, as the bowels had not acted since the commencement of the attack.

24th. Mr. Hadden's note says: Patient had a restless night; she swallowed a little wine-and-water with difficulty, and continued the ice; bowels were freely moved; countenance less distressed, and the breathing not so difficult.

Six P.M. On visiting the patient I found her symptoms not as much relieved as I had anticipated; and as the tongue was still very much inflamed, I made incisions again into the organ in the same situation as the previous ones. A large quantity of blood escaped, and as she could not swallow, I ordered nutritive injections of beef-tea and whisky to be administered every third hour.

Eleven P.M. Breathing much less difficult; the girl could speak a little; her voice, heard for the first time since admission, was peculiar, and the speech "thick" and indicative of disease. Pulse 112, and stronger.

25th. Patient slept tolerably well last night; she can swallow a little, and still enjoys the ice; nutritive injections continued.

26th. Pulse 100; saliva still flowing freely from the mouth.

27th. Tongue still much enlarged, and protrudes beyond the teeth; the patient cannot swallow enough of nourishment to keep up her strength. Ordered nutritive injections every hour.

28th. Pulse stronger; patient swallows a little iced milk; white exudation is separating from the tongue.

29th. The incisions, so far as they can be seen, are mere lines; the edges of the tongue are deeply indented from the teeth; the *hardness and enlargement consequent on the effusion of the lymph still remains.*

Mr. Croly proceeded to say that, on the last occasion when this subject was under discussion, he mentioned that the first case described by him coincided with those published by Dr. Fleming, where the disease commenced under the chin. In the next case half the tongue only was affected; the third case involved the whole tongue; the fourth case only half the tongue: the fifth case the entire tongue; and the last case arose from erysipelas. He (Mr. Croly) considered that the case which he brought before the Society that evening was a typical case of glossitis; and he was certain that if the tongue had not been cut freely and deeply, the trachea should have been opened to save the girl's life.

LECTURE ON THE ANATOMY AND SURGERY OF THE HUMAN FOOT.

Delivered at the Royal College of Surgeons of England in June, 1867,

By HENRY HANCOCK, Esq., F.R.C.S., Surgeon to Charing-Cross Hospital, and
Professor of Surgery in the Royal College of Surgeons.

MR. PRESIDENT AND GENTLEMEN,—Interesting and important as were the matters treated of in my last course of lectures, they formed but a comparatively small section of the surgery of the part to which they appertained—indeed, so many subjects of great value still remain to be studied that, even at the risk of appearing tedious, I am induced to devote the greater portion of this my present course to their consideration; and in this way, as far as my abilities permit, to lay before the profession a complete epitome of the surgery of this region of the body.

In accordance with this object I propose to-day to treat of an operation which, whether considered in relation to the importance of the region in which it is performed or the amount of benefit it is destined to afford, has not met with that support to which it is fairly entitled. Whilst in England the shoulder, elbow, hip, knee, and other joints are freely excised, and, when offering a prospect of success, a surgeon would almost be

considered as deserving of censure who would neglect these operations for amputation, excision of the ankle-joint has for the most part been regarded with disfavour.

We may partly attribute this to the confused ideas entertained as to the exact nature of the operation by authors, who write upon that which they have never done, and which they do not understand; and who, consequently, mislead their readers by inexactness of detail, and by the heterogeneous mass of discordant cases which they have collected and described under the head of "Excision of the Ankle-joint," and which in reality have as little to do with this operation as they have with Syme's or Pirogoff's amputation.

A recent American author, for instance, professing to write upon excision of joints, includes the following operations in his table and description of excision of the ankle—viz., excision of the astragalus and of os calcis; removal of the end of the tibia; removal of the ends of the tibia and fibula with the astragalus and navicular; simple excision of the astragalus; of the ends of the tibia and fibula, the whole of the astragalus, part of the os calcis, and the three cuneiform bones; of the end of the tibia, the astragalus, scaphoid, and two cuneiform bones; excision of the astragalus and scaphoid; excision of the os calcis, astragalus, and cuboid.

Nor are these vague ideas entertained by this author only. Others who have written text-books for the guidance of students have published equally vague notions, thus perpetuating and adding to the obscurity they profess to dispel. Although Hippocrates is stated to have removed the lower ends of the tibia and fibula for compound dislocation, whilst Mr. Hey of Leeds performed the first authenticated operation of the kind as early as the year 1766, it was not until the year 1792 that the elder Moreau performed excision of the ankle-joint for disease, being the first occasion upon which this operation had been performed excepting for accident.

The case was that of the son of M. Lucot, inspector of gendarmerie, who met with a sprain, in the year 1791, which ended in extensive caries of the left ankle. After the lapse of a year there was a fistulous ulcer on each side of the joint, discharging sanious and fetid pus; and the probe being introduced through the openings, the articulating surfaces of the tibia and fibula, as well as the body of the astragalus, were felt to be bare. The foot and the lower part of the leg were swollen, and the patient could not bear his weight on the limb. On the 15th of April, 1792, Moreau excised the ankle-joint, removing the lower ends of the tibia and fibula, and the whole of the articulating surface of the astragalus, and a

great portion of its body, till he came down to what was sound. In six months the patient was able to bear his weight upon the foot. During the seventh month he used crutches; in the eighth he could walk with a stick; and by the end of the ninth he walked without any assistance whatever, and in such a way that he could do as he pleased.

Such are the particulars of this celebrated case; and it is interesting to read the description given by Moreau's son of the disfavour shown by the French surgeons, not only to this operation, but to resection of joints in general. In the year 1784 Mr. Parke's observations on cutting out the articulating ends of the bones of the elbow and knee-joints were translated and published in France by Professor Lassas, whose authority, observes Moreau, "one would have thought might have procured for them a favourable reception. They were received with astonishment; and so far were they from gaining credit, that in 1789 they had acquired so few supporters in the Academy of Surgery that some cases of a similar kind, presented to the Academy by the elder Moreau, were rejected, though they were of such a nature, and stated in a way that deserved a more favourable reception." Nothing daunted, Moreau again addressed the Academy of Surgery, supporting his memoir by many facts. His essay, however, met with a most violent opposition. The Academy, as is too often the case, found it more convenient to deny than to examine these facts; and, instead of taking the trouble to ascertain their reality, they answered in a way that forbade all future inquiry upon matters demanding the greatest attention. It is due to M. Pellaten to state that of all the then members of the Academy, he alone appears to have considered the subject worthy of attention.

The second operation on record was performed by Moreau, junr., in 1796. He preserved the lower end of the fibula, and consequently had great trouble in getting away the disease, being obliged to use the gouge freely. The recovery was not so satisfactory as could have been desired.

The third operation was by Mulder, in 1810. He at the same time removed five inches of the fibula.

In 1813 Champion is reported to have operated upon a woman, who afterwards frequently walked three leagues to be examined by Roux and others.

In England there was no example on record of excision of the *ankle-joint* for disease prior to February, 1851, when I performed the operation for the first time. Attempts have been made to deprive me of the merit of its introduction to British surgery, but I doubt not my ability to prove that if the term "excision of the ankle-joint" means the excision of those

parts only which enter into the formation of the joint, the credit is my own; but that if it means the removal of any amount of the foot, whether comprehended in the joint or not, that credit is more justly due to Liston, Wakley, Teale, and others.

The author of the work already alluded to says: "It does not appear, however, that after the above cases (those by the Moreaus) this operation was again attempted for disease until 1818, when it was undertaken by Mr. Liston, in Edinburgh. In April, 1830, it was performed in France by M. L. Champion; and in June of the same year, by M. Roux. In December, 1847, Mr. Thomas Wakley excised the os calcis and astragalus; and in March, 1850, the end of the fibula, with part of the astragalus. It appears, therefore, that the honour of first performing this operation in Great Britain belongs to Mr. Liston, and of reintroducing to Mr. Wakley rather than to Mr. Hancock, for whose excision of Feb. 17th, 1851, it has been claimed."

Moreau, as we have seen, performed his operation on the 15th of April, 1792. His proceedings were as follows:—He made a longitudinal incision, beginning at the inferior and posterior part of the malleolus externus, continuing it upwards from three to four inches. He then made another incision, transverse, which extended from the inferior end of the former incision to the edge of the tendon of the peroneus brevis. He made another longitudinal incision on the inside, which began at the inferior and posterior part of the malleolus internus, and extended from three to four inches along the internal border of the tibia. Then, by a third incision, which began at the lower end of the tibia, he cut the skin transversely till he came to the tendon of the tibialis anticus. He disengaged the fibula from the tendons, ligaments, and in general from everything by which, at its inferior extremity, it is held in its situation; he passed the handle of a scalpel under it, and with a chisel he cut it across above the ankle. Wishing to cut the tibia above the malleolus before he turned it out of the joint, he separated everything that adhered to it; and then, passing the handle of his scalpel between the posterior surface of the bone and the flesh, he introduced between the spine of the bone and the flesh before the bone the blade of a narrow saw, and cut the bone, sawing from before backward, which was a work of no small trouble. That being done, he turned the foot outward, and, making the piece of bone which he had cut off project, he detached it from the tarsus without difficulty. The astragalus being diseased, he removed the whole of its articulating surface, and a great part of its body, till he came down to what was sound. Now, contrast this with Mr. Liston's case, as described in the *Edinburgh Medical and Surgical Journal* of January, 1821. He removed the astragalus, scaphoid, and two cuneiform.

Again, contrast Moreau's operation with Mr. Wakley's original and celebrated proceeding as detailed in *THE LANCET* of April 12th, 1851, the first of those referred to, and performed on the 27th of December, 1847. I quote Mr. Wakley's own words:—"The diseased foot (the left) having been drawn forwards so as to be free of the table, I made an incision from malleolus to malleolus directly across the heel. A second incision was next carried along the edge of the sole, from the middle of the foot to a point opposite the astragalo-scaploid articulation; and another, on the opposite side of the foot, from the vertical incision to the situation of the calcaneo-cuboidal joint. These latter incisions enabled me to make a flap about two inches in length from the integument of the sole. In the next place, a circular flap of integument was formed between the two malleoli posteriorly, the lower border of the flap reaching to the insertion of the tendo Achillis. This flap being turned upwards, the tendon was cut through, and the os calcis, having been disarticulated from the astragalus and the cuboid bones, was removed, together with the integument of the heel included between the two incisions. The lateral ligaments connecting the astragalus with the tibia and fibula were now divided, and the knife was carried into the joint on each side, extreme care being observed to avoid wounding the anterior tibial artery, which was in view. The astragalus was then detached from the soft parts in front of the joint and from its articulation with the scaphoid bone, and the malleoli were removed with the bone-nippers. The only artery requiring ligature was the posterior tibial."

Take, again, Mr. Wakley's second case, thus reported in *THE LANCET* of May, 1850:—"Mr. Wakley made a crucial incision directly over the outer malleolus, and having carefully dissected back the small triangular flaps, the whole of the diseased malleolar process of the fibula was now exposed, as well as the portion of the astragalus. Both these parts were evidently in a carious, partly in a necrosed state, and were excised principally by means of a gouge."

Although undertaken with the same object as that which actuated the elder Moreau—namely, to excise the ankle-joint solely, the operation which I performed, whilst attaining the same result, differed somewhat in certain details, which will be apparent as we proceed. Instead of making two incisions at right angles on either side of the joint, I commenced the operation with an incision behind, and about two inches above the external malleolus, carrying it forwards beneath that process across the front of the joint, and terminating about two inches above and behind the inner malleolus. This incision included the skin alone, without implicating the tendons or their sheaths. The flap thus formed was dissected

up, and the peronei tendons were detached from the groove behind the fibula and cut through, as were the external lateral ligaments close to the fibula with a pair of bone-nippers. I next divided the fibula about an inch and a half above its inferior extremity, and cutting through the inferior tibio-fibular ligaments detached the external malleolus. Now, turning the leg on to its outer side I cut through the internal lateral ligament, carefully keeping my knife close to the end of the tibia to avoid the posterior tibial artery. The tendons of the tibialis posticus and flexor communis were then detached from the groove behind the internal malleolus, and taking the foot in my two hands, the late Mr. Avery, who assisted me, holding the leg, I next dislocated the foot outwards, thus bringing the end of the tibia with the internal malleolus prominently through the wound. These were removed by the common amputating saw, applied half an inch above the horizontal articulating surface of the tibia, the soft parts being protected by a spatula, and the upper articulating surface of the astragalus having also been removed by a metacarpal saw, held horizontally. The foot was then restored to its proper position, the cut surface of the astragalus being adapted to the cut surface of the tibia, and the wound having been closed by sutures, except on the outside, which was left open for the free escape of discharge, the leg was placed on its outside on a splint, having an opening corresponding to the wound, and the patient was returned to his bed.

You will doubtless have observed that the parts here cut through were the skin, the peronei tendons, the internal and external lateral and the inferior tibio-fibular ligaments, and the lower ends of the tibia and fibula. In subsequent operations I preserved the tendons entire. In no instance have the anterior or posterior tibial arteries been wounded, and in no instance has it been necessary to apply a single ligature.

Let me now demonstrate by the diagrams before you the amount of skeleton removed, and I doubt not it will then be admitted that, however honourable to their authors the operations performed by Liston, Teale, and Wakley, assuredly they were not excisions of the ankle-joint, and that consequently they in no way invalidate my claim of having been the first to introduce this operation into the arena of British surgery.

Although this operation has been performed by the late Mr. Jones of Jersey, Mr. Canniffe of Canada, the late Dr. M. S. Buchanan of Glasgow, Mr. Barwell, Mr. Canton, Sir W. Fergusson, Mr. Hessey, Mr. Holmes, and Mr. Paget, it is not regarded with that general favour to which its merits entitle it.—*Lancet*.

ON THE MANNER OF THE INHERITANCE OF CANCER, AND ITS RELATION TO QUESTIONS CONCERNING THE LOCAL OR CONSTITUTIONAL ORIGIN OF THIS DISEASE.

By W. MORRANT BAKER, F.R.C.S., Demonstrator of Anatomy and Operative-Surgery at St. Bartholomew's Hospital, etc.

The theory that cancer is, at the first, a local and not a constitutional disease, has recently being strongly upheld by an eminent writer on this subject (*The Antecedents of Cancer*, 1865, by C. H. Moore, F.R.C.S.; *A Brief Report on Cases of Cancer*, by C. H. Moore, F.R.C.S., *Brit. Med. Journal*). At the same time, he has shown how necessary for the establishment or destruction of any such theory are large statistics of cases of the disease; and it therefore seems incumbent on all to contribute, as far as they can, to the settlement of this vexed and oft-mooted question.

It is a matter of common observation, that no subject, illustrates better than cancer how much may be said on both sides of an argument. There are, indeed, but few facts which have been imported into discussions on this subject that will not serve as weapons as well for one side as the other—as evidence, as much of the local, as of the constitutional origin of the disease.

There is, however, one point in the history of cancer which has a real bearing on the question at issue, and has more weight, perhaps, than any other; namely, the manner of the inheritance of the disease. And I propose to consider this point alone, not because it is by any means the only one that should be taken into consideration, but because it seems to me sufficient by itself to settle the particular question in dispute, so far as a settlement is at present possible.

Before bringing forward any statistics of inheritance, it may be well to direct attention to the fact that two very different notions concerning that which is inherited appear to exist in the minds of different writers on this subject. For instance, the question is argued sometimes on the apparent supposition that cancer, as such, is handed down from parent to offspring; and that, therefore, the laws by which its transmission is regulated ought to be the same with those which govern, say, the appearance of syphilis in the offspring of a syphilitic parent. Thus it has been said that, had inheritance much to do with the appearance of cancer, this disease should be more frequently congenital, or, at least, a more common malady in infants and young children than, from the examination of statistics, it appears to be (*The Antecedents of Cancer*, by C. H. Moore, 1865). But this argument, which at first sight seems a strong one, be-

comes fainter when it is examined from another side. That cancer, as cancer, is handed down from one generation to another, is not, I think, generally accepted as the usual manner of its inheritance, although it may so happen sometimes. It is not necessary to suppose, in every case in which inheritance has had to do with the matter, that there has been direct transmission of the disease; else a belief in the inheritance of cancer from a grandparent, when the intervening generation escapes, would be an absurdity; for we cannot believe that, in this instance cancer could actually exist in the second generation, and even be transmitted to offspring, and yet remain undeveloped. Mr. Paget has put this fact very plainly. "That which is transmitted," he observes, "from parent to offspring is not cancer itself, but a tendency to the production of cancer at some time far future from the birth. We have no reason to believe that a cancerous material passes with the germ. To suppose such a thing, where the cancerous parent is the male, would be almost absurd. Moreover, no reason to believe that cancerous material passes from either parent is furnished by any frequency of congenital cancer." (*Lectures on Surgical Pathology*, 2nd ed., p. 774.)

So, then, in speaking of the propagation of cancer by inheritance, it is necessary to have a definite idea of what is supposed to be inherited, or, at least, to be not inherited; for, on the ordinary supposition that it is a tendency to the disease, and not the disease itself, that is transmitted from parent to child, we must be contented, necessarily, with a very vague notion, or, indeed, none at all, concerning that which is passed down from one generation to another. But the impossibility of saying how a tendency to disease, and not the actual disease, can be transmitted from parent to offspring, need not make us discard the notion of such being the manner in which cancer is inherited. At least, if we do so, we must fly in the face of all statistical records of the inheritance of cancer, and be prepared to deny altogether the possibility of arguing on this subject from the analogy afforded by many other diseases. For certainly a tendency to many others than cancer is inherited, as, for instance, gout, phthisis, brain-disease, etc.; and yet the disease, itself may never show itself at all, or may skip over a generation; and, even in the event of its appearance, it is frequently or commonly not observed until the subject of it has reached about the same age as that at which the disease showed itself in the parent. Indeed, there is no more impossibility in the transmission of a tendency to disease than there is of a tendency to the development of likeness in feature at about corresponding ages in parent and child. Both are results of the descent of a material something or other by inheritance; but just as cancer, which is not inherited, is the

result of certain material conditions which become potent for its production only at a certain period of life—it may be at an advanced age—so there is no improbability in the supposition that, in the case of inherited cancer, the inheritance is of the conditions which may or may not develop the disease, rather than of the disease itself. So far as the fact can be proved by statistical records, this is certainly the usual way in which it is handed down from generation to generation; and, if argument from analogy be admitted, this is the manner in which, from observation of the inheritance of other diseases, we should expect it to be transmitted.

In the next place, it must be observed, that the mere fact of cancer being a heritable disease has, by itself, nothing to do with the question, whether it is local in its nature or constitutional. The inheritance of this disease is sometimes mentioned as if to allow that such an occurrence is frequent, is almost to grant that the malady is a general and not a local one. Surely, this is an error. Epidermal cysts, crooked fingers or toes, hernia, etc., which may be taken as instances of what are called local diseases, are as undoubtedly heritable as cancer. And we may therefore regard the question, whether cancer is, at its beginning, local or constitutional, a distinct from the inquiry, whether, and to what extent, it is inherited.

As to the fact, that inheritance has a frequent influence in the production of cancer, there can be little doubt. The last published *résumé* of Mr. Paget's statistics (*Medico-Chirurg. Trans.*, 1862) gives 24.2 per cent., or nearly one in four, as the proportion of cancerous patients who were aware of the occurrence of the disease in other members of their families. And it must be remembered, that although some of these cases may have been only coincidences of disease; yet the deduction that should be made on this account is probably more than balanced by the increase that would have to be made, if the inheritances from patients, who die with unknown internal cancers, could be reckoned also.

It may be fairly said, too, that in a certain number of cases, a tendency to cancer, must be transmitted by inheritance; but the fact is never known on account of the death of the parent from some other cause before the disease has manifested itself. The frequent delay in the appearance of cancer until advanced age makes it the more probable that, from this cause, all statistics of the disease must give too low an estimate of the frequency of its inheritance.

It is not, however, the extent to which the inheritance is influential in the production of cancer that can settle the doubt whether the disease is, at first, only local. There are local diseases which are inherited as fre-

quently as many which are constitutional, and, in some instances, even more so. The mode, however, in which the disease is passed from one generation to another, is that which should afford a crucial test of its beginning, as a local disease or a general one.

Mr. Moore has, indeed, put the case very plainly. He remarks (*Antecedents of Cancer*, 1865, p. 20), "If it be usual for dissimilar cancers to prevail in direct inheritance,—for the children of cancerous parents to have primary cancer of various organs,—then cancer passes from parent to offspring as a general and not a local disease. It belongs indifferently to all the body. Its constitutional nature is established." And he then proceeds to say that such is not the result of his experience, although he is not in a position to state the fact numerically. In his later paper (*BRITISH MEDICAL JOURNAL*, Dec. 1, 1866), he supplies this want by a relation of twenty-four cases of cancer, which occurred in the relatives of cancerous patients who were under the care of various members of the British Medical Association. "One-half of these multiple cancers, in related persons, occupied similar, and one-half dissimilar, organs.

By the kindness of Mr. Paget, I have also lately been enabled to publish some statistics of cases of multiple cancers in families, and of the proportion of similiar to dissimilar cancers among relatives (*St. Bartholomew's Hospital Reports*, vol. ii. 1866). They have been collected from those of his cases in which any information of the kind had been recorded, and have been arranged in the order in which they occurred, without reference to any particular result. It is unnecessary to quote the whole table, which may be found in the place to which a reference has been given. It will suffice to say, that altogether I have been enabled to tabulate eighty-three cases in which the occurrence of cancer, in more than one member of a family, had been recorded, together with the situation of the cancer in the relatives affected. Examination of the cases gave the following result:—

"There were altogether forty-five instances (in forty-one families) of direct inheritance from father or mother; and of these, the disease was in the same organ in both parent and child in nineteen, and in different organs, in twenty-six instances.

"It is a curious fact that in all the cases but one, in which the disease occupied the same site in both generations, the breast was the organ affected; the exception was the uterus. As might be expected from this fact, almost all these patients were females, only one case occurring of cancer of the breast inherited by a male.

"Among the cases of direct inheritance from a parent, but in which

the disease was transmitted to a different organ in the child, nine were instances of inheritance from the father, and seventeen from the mother.

“ There were sixteen instances (in fourteen families) of inheritance from a grandparent or great-grandparent, or both; of these, eight were cases of cancer in the same, and eight in a different, organ in the two generations. As before, the cases of disease of the same site in both generations, were cases of cancer of the breast, with a doubtful exception.

“ There were forty-nine families—some of them have been included in the former statement—two or more members of which had cancer, the relationship between whom, however, was not that of parent, or grandparent, and child. Of these, twenty-eight were families in which the disease was seated in the same organ in all the relatives affected; twenty-one in which different organs were attacked.

“ There were twenty-five instances of the disease in brothers or sisters, or in brother and sister. In fourteen of these the same organ suffered in both the relatives affected; in eleven, a different one.

“ The whole of the twenty-eight instances of cancer of the same organ, in all the affected relatives, were cancer of the female breast; and fourteen of them were in sisters ” (*St. Bartol. Hosp. Rep.*, vol. ii, p. 136).

Taking Mr. Moore's statistics (twenty-four cases), therefore, and Mr. Paget's (eighty-three cases), we have altogether one hundred and seven cases wherewith to decide the question whether cancer, is, at its being, local or constitutional, so far, that is to say, as it can be determined by observation of the manner in which it is inherited.

In the sentence before quoted from Mr. Moore's paper, it is rightly said, that the occurrence of inherited cancer in other organs than those affected in the parent, would be a sufficient reason for considering the disease a constitutional one and not a local. How then does the case stand, now that a fair number of cases, having a special reference to this point, has been collected?

Taking the whole of the cases together, in which cancer occurred in relatives, one-half, almost exactly, occurred in similar, and one-half in dissimilar, organs in the affected members of the same family.

Taking only the cases of direct inheritance from a parent,—and these are, of course, the more conclusive—the number of instances of unlike cancer in parent and child is found to preponderate over those in which they were alike.

Now, it is difficult to see how any evidence, so far as it goes, can be

more conclusive. It is equally difficult to see how this evidence of cancer being not merely a local disease, can be resisted. The only way in which it can be resisted, or shown to be not conclusive, must surely be by doubting whether inheritance has anything whatever to do with the matter; or, and this is a less reasonable alternative, by assuming that the instances of cancer, similiar in the offspring to that in the parent, are cases of true inheritance; and that those of cancer, unlike in parent and child, are simply coincidences, and nothing more.

As the latter alternative, indeed, is almost a *reductio ad absurdum* in the presence of the histories of cases of multiple cancers in families, such as may be found recorded in treatises on this subject, it will be unnecessary to consider it. It may be well, however, to inquire whether there is any reason for thinking that all supposed cases of inheritance of cancer may be only coincidences of disease in the same family.

The percentage, before quoted, of cancerous patients who were aware of the occurrence of the disease in other members of their family, seems by itself almost conclusive; but the supposition of coincidence is rendered still more untenable by the observation made some time ago by Mr. Page,

Med. Times and Gazette, Aug. 22, 1857), that among a large number of patients with innocent tumours, neither cancerous nor recurring, only 6·8 per cent. were aware of any relative having cancer; while 22·4 per cent. of the cancerous had one or more relatives with the same disease.

If, then, we may believe on the present evidence, that cancer is really inherited in a large number of cases, and that in the transmission of the disease in this way, it happens, as often as not, that a different organ suffers in the offspring from that affected in the parent, how can the theory be maintained that cancer is a local malady?

The fact that, among inherited cases, cancer is so frequently in the same organ in both parent and offspring is no evidence for its merely local nature. The occurrence of tuberculous disease in the lung, and of gout in the great toe in successive generations may be fairly taken to prove for what parts these diseases have a special liking, but is scarcely a sign either of their local nature, or of their transmission by inheritance as local diseases. And it is not easy to understand why, under analogous circumstances, cancer, and not gout or phthisis, should be considered a local disease.

Moreover, on reference to the statistics, it will be seen that it is only in instances of cancer in those organs, especially the breast and uterus, for which it has a very great liking, that we see any special tendency to a repetition of disease in the same part in two members of successive gene-

rations. And that cancer of the breast or uterus, when transmitted to offspring, should be repeated in the breast or uterus respectively is surely not, on any theory, to be wondered at, when it is remembered how likely it is that, under any circumstances, cancer in the female will be found in one or other of these organs.

But again, the proportion of cases in which cancer occurs in a different organ in parent and offspring is of comparatively little moment. If it be allowed that cancer is ever transmitted to a different organ in the child from that which was affected in the parent, then it is practically allowed that cancer is not merely a local disease. For such an admission in any individual case, must of necessity be the same thing as granting that in this special instance the disease was not merely a local one. And before those which occur in the same organs in parent and offspring can be supposed to be merely local, surely some difference must be shown plainly to exist between the cancer which occurs in the two groups of cases (those in the same and those in different organs). For, while there is every reason to believe that a constitutional disease will always be found to prefer certain organs as its site before others, there is no reason for believing that a disease, not traceable to external influences, can spring up at one time as a local and at another as a constitutional disease.

If the number of cases in which there occurred unlike cases in successive generations was very small in proportion to the number of those in which the disease was, under the same circumstances, alike, there would be, of course, an increased chance that the exceptions might be only coincidences. But, if it be granted or proved that such cases as those referred to really occur, then one such case would weigh more in the decision than a hundred or a thousand on the other side, and for the reasons just given. But the case is not reduced to such straits. Under all circumstances, the occurrence in question seems to be as frequent as the reverse; and, if we except the cases of cancer in those organs towards which it always has a very strong tendency, there really seems to be comparatively little chance of an inherited cancer attacking the same organ in the child with that which it affected in the parent. At least, if we except cancer of the breast and uterus, such an event happened in Mr. Paget's forty-five cases of direct inheritance (from parent to child) not once, and in Mr. Moore's eight cases only once.—*British Medical Journal*.

FAVOURABLE TERMINATION OF A CASE OF OVARIAN DROPSY WITH- OPERATION AND SUGGESTIONS AS TO ITS TREATMENT.

By DR. GEORGE CRAWFORD.

The following case is so pregnant with instruction to the Medical Profession at large, that were I to withhold giving it publicity I should not be doing my duty as one of its members:—

Mrs. H., aged 64, the subject of the following remarks, and the mother of a large family, had always enjoyed good health till about three years ago. At that time she observed a slight swelling taking place in the lower part of her abdomen, on the right side. Having caused her no pain, the only inconvenience arose from its bulk and occasional gastric disorder; consequently she gave it no attention for two years, till I was called in about twelve months ago. The swelling had rapidly increased for some months previously, and had now become very painful. I examined her and found her very much enlarged, resembling a person about the seventh or eighth month of utero-gestation; but, as she had ceased to menstruate for twenty years, it left no doubt in my mind as to the disease being any other than ovarian. The tumour occupied very much the middle line, and fluctuation was distinctly felt. Great pain was complained of at a point about two inches to the right of umbilicus, and the skin at that place was much inflamed, and apparently adhered to the structures beneath.

As a means of relieving this pain I ordered hot linsced-meal poultices to be constantly applied, at the same time giving her a mixture of infusion of digitalis, acetate of potash, and iodide of potassium, there being a considerable amount of œdematous swelling in the lower extremities. The pain was immediately relieved, and also the swelling in the legs. The tumour did not perceptibly increase for the next three or four weeks, but as she was chiefly in bed she could not judge so well of its size as when she was walking about. At the end of this time a small opening that would admit a common probe occurred on that part, which was inflamed and adhering to the structures beneath, when a considerable quantity of yellowish fluid was discharged from it, along with a number of very minute hydatids. This fluid continued gradually to escape for a number of weeks until the tumour was almost gone; indeed, the only indication left of its existence was a thickened doughy feeling in the right iliac region, extending to the opening where the fluid escaped. After she was up and apparently as well as ever, the small opening healed over, but the swelling slowly returned. A few more poultices were applied, and the fluid began to escape as before.

I saw her a short time since, and she told me that when it stops discharging for a week or two, all she has to do is to apply more poultices, and the discharge is renewed. Several folds of cloth placed over the opening to dry up the discharge, which is now very trifling, and a bandage tightly applied to keep up a considerable degree of pressure, is all she is doing for it, and her health is now as good as ever—at least she has no inconvenience from it.

From the facts of the case narrated above, it will be seen that the opening whereby the fluid was discharged took place spontaneously, that the cyst in all likelihood was unilocular, that adhesion of its walls must, to a certain extent, have taken place, that the opening has been nearly permanent, that it gives exit to the fluid as it is secreted within the cyst, that the disease is checked, the swelling removed, and the patient able to attend to her duties as before.

Now, I am of opinion, when the distension is great and the fluctuation is more distinctly felt at one spot than at another, that if inflammation were induced, by pressure or otherwise, to insure adhesion of the abdominal wall to that of the cyst, and a small opening made, not with the view of emptying the cyst of its contents at once, but more with the intention of allowing it to drain by degrees, and at the same time a bandage be tightly applied to exert a considerable amount of pressure over the tumour, and the opening prevented from closing, many cases might be treated in this way, and the patients, if not entirely cured, might be much relieved, and be able to spend the remainder of their lives with more ease and comfort, and freed from the grave consequences of undergoing the operation of ovariectomy, which is at all times accompanied with so much hazard and danger to life, or the tumour be prevented from pressing on some vital organ, and destroying the patient in that way.

Port Glasgow, Feb. 25.—*Medical Times and Gazette*, March 23.

DEATH OF M. CIVIALE.

This eminent surgeon died, almost suddenly, on the 13th inst., in the 75th year of his age. Seldom has a name been so thoroughly identified with an operation as that of Civiale; and all over the civilized world will everyone understand, when hearing of his death, that the inventor of lithotrity, an ingenious and persevering surgeon, a skilful operator, an eminent author, a teacher of several generations of lithotritists, a conscientious man, and a benefactor of humanity in his special branch of surgery, has departed this life.

Civiale died at an advanced age, in the possession of a very ample fortune, and in the enjoyment of the most enviable honours and distinctions. He was a member of learned societies of the first order, the head and leader of lithotrity, the author of several works on subjects connected with his department of practice, and sole surgeon of the wards set apart for crushing stone at the Necker Hospital at Paris; and so fully was he in possession of his faculties up to the end as to have just completed a magnificent museum of calculi for the above-named hospital, and corrected the proofs of an important work on Lithotrity, which will soon see the light.

Seldom has a man succeeded in rendering greater services, and of a

more signal kind, to the sufferers from a most distressing complaint; and all must rejoice to find that his efforts have been duly rewarded and magnificently acknowledged, both by the public at large and by the most eminent bodies of our profession.

The general expression of regret at Civiale's demise contrasts in a striking manner with the unnoticed death of Heurteloup, who certainly worked with great activity and perseverance in the same path. And why? Civiale laid his practice, his inventions, his teachings open to the world; Heurteloup kept whatever improvement he might introduce egotistically to himself, afraid that others should reap any benefit from his mechanical skill. Civiale brought his methods of operating and his instruments with great alacrity before the learned societies of his country; Heurteloup's communications were all wrapped in mystery, full of supposed results, but silent on the means of obtaining them. Both are gone—one will be remembered with gratitude, the other forgotten.

Civiale's position among the leading members of the medical profession and the learned societies of Paris may be looked upon as quite exceptional, and the distinctions he obtained as peculiarly gratifying, because our brethren of the French capital, particularly the most eminent; have a horror of specialism. Both Ricord and Civiale have had to contend against this feeling, and they have both succeeded in a most credible manner. The setting apart of special wards at the Necker Hospital for the exclusive treatment of calculous patients by Civiale was quite an event, the like of which had not been known before in any specialty, and has not occurred since.

It is said that some remarks made by Marjolin at the Hôtel Dieu many years ago struck Civiale, then Dupuytren's pupil. After surmounting many difficulties and submitting to severe criticism and even ridicule, he succeeded in applying Marjolin's hint; and, by dint of persevering efforts, the deceased inaugurated and subsequently vastly improved a most important branch of practice.

It may here be remarked that discoveries and inventions have of late years borne principally on the improvement of our means of diagnosis, as shown by the stethoscope, laryngoscope, ophthalmoscope, sphygmograph, &c.; but therapeutics have not advanced in the same ratio. Civiale's is a splendid therapeutical discovery, which he has the glory of having made and carried, through a long series of years, to a high point of perfection.

The best known British lithotritists will certainly learn with regret: the demise of Civiale, for many, if not all, of them have been his pupils. We well recollect that one of them, Mr. Coulson, was the first to introduce the practice of lithotritry into this country. A series of English surgeons have since shed much lustre on lithotritry, and gratitude for Civiale's services must rise to a very high point, when it is recollected that his teaching spread extensively, as it included medical practitioners from the old and new world.

He was buried in the neighbourhood of Paris with the greatest simplicity, in compliance with his wishes; but the news of his death will certainly cause the deepest regret among his friends, the medical profession, and the public at large.—*London Lancet.*

Canada Medical Journal.

MONTREAL, JUNE, 1867.

We publish in this number of the journal the evidence taken in defence, at the trial of Provencher for the murder of Joutras, by poison. To the unbiased reader, this case presents all the features of a most foul premeditated murder, there is not one single extenuating circumstance, and in our opinion the culprit met the just punishment for his crime. The evidence for the defence consists exclusively of medical testimony, which is of such an extraordinary character, that it is perfectly incomprehensible, it is utterly beneath criticism, as it partakes more of the character of the testimony of a partisan, than that of a scientific investigator. A medical witness is bound to speak out his conviction truthfully, and according to the ability wherewith he has been endowed. We regard it as much to be deprecated that medical men are to be found willing and ready, apparently from a spirit of opposition, dissenting from the views of others, to rush into a court of justice and endeavour by special pleading, to mystify and puzzle a jury. Every allowance should be made, for the desire of saving an accused from the awful consequences of a chain of circumstances neatly interwoven, and which seem to be incapable of disentanglement, but this is not the province of a medical jurist he can alone hear facts testified to by other parties, and on these facts base his opinion.

We must believe that in this case the medical gentlemen for the defence are sincere in all they say. We cannot for one moment suppose that they have in any one particular stated what they do not believe to be true. Still it is not quite what we ourselves believe to be correct, in fact, we must state that their testimony in many particulars is utter nonsense, to place the very mildest construction on the rendering, it is at utter variance with the acknowledged received opinions of the day.

It is seldom in a case of the present character that an unbroken chain of evidence is brought forth. The motive was clearly made out, the fact, that of the administration of the poison on several different occasions, with intervals of days between, the dose on each occasion being sufficient to develop the characteristic symptoms of poisoning by strychnine; distinct periods of repose occurred, during which the unfortunate victim was enabled to go about his usual avocations. At length the wretched

criminal being fully impressed with his safety from detection, gave the *coup de grace*, in a larger dose which proved fatal, and then came the *dénouement*. The man Provencher never supposed that his complicity had been suspected by the physician who had observed the symptoms of deceased, as Dr. Ladouceur says in his evidence at the trial, the symptoms exhibited by deceased, differed from those of any disease of which he was acquainted: hence his suspicion of foul play. It has been asserted by one of the medical gentlemen who appeared for the defence, that testimony given by unskilled persons of the existence of certain symptoms is unreliable, and therefore not admissible in a trial of this nature, that the evidence alone of medical witnesses is to be received. This is a novel and we must say a most pernicious doctrine, and without applying any stronger term is simply absurd. Supposing a man is shot through the body and is not seen during life by a physician, is the testimony of a third party who may have stood by and observed the gradual death, say from syncope, of the victim, unreliable, simply because he is not a physician, and as a consequence incapable of judging whether the deceased died from the injury or from natural causes. We regard the evidence of the unskilled who testify to facts which fully bear out the observations of scientific research as of more than ordinary weight, for instance, in the present case, in the report of the witnesses for the crown, they all testify to the great sensitiveness of deceased under the slightest excitation, walking across the floor, touching his body, speaking loudly, in fact anything likely to excite the highly exalted condition of his nervous system at once developed the tetanic spasms. Can any man in reason associate this condition with one of rheumatism or the host of isms of which, according to the medical testimony for the defence, Joutras died. We regret exceedingly to be obliged to take even this notice of the defence, but as public journalists it becomes our duty. We regret to observe that one of the medical gentlemen who gave evidence for the defence in so bad a cause is still floundering in the mire by writing letters in a daily paper, endeavouring, with the heroism and pluck worthy of a better cause, to rid himself of the opprobrium which must attach itself to his name if he pursues so false a policy.

This cannot be regarded as a criticism on the evidence, because, as we before observed, it is utterly beneath criticism, but we are unable to pass over the evidence as published without a few remarks. It has been stated that Provencher died without leaving a confession, this we have been informed is incorrect, and we trust for the benefit of the community among whom he dwelt while living, that any statement which he may have left will in due time be made public.

The Medical evidence and an abstract of the general evidence adduced on the trial of Modeste Vilbrun alias Provencher, for the alleged wilful murder, by poison, of François Xavier Joutras. At the criminal term, held at Sorel. C.E., in March 1867, before the Honorable Mr. Justice Loranger.

EVIDENCE FOR THE DEFENCE.

DR. JOSEPH EMEBY CODERRE STATES.—I have been a practising physician since 1844.—I am a professor at the montreal school of medicine. During my period of tuition I have been professor of Materia Medica and of Therapeutics I have been physician to the Hotel Dieu since 1850 and I also practise my profession amongst the public.

I was employed as medical "expert" in a supposed case of poisoning it was the case of Lussier of St. Hyacinth. My report in this case was published. I have published my opinions in several cases of poisoning.—Venomous matter may in certain cases be found in the materials used for a Chemical Analysis, I myself found arsenic in the copper which we were going to use in an Analysis. This was in the Lussier case. I dont think strychnine could exist in the materials which are used as reagents, but accidentally there might be some.

I heard the evidence which was given in this case regarding the symptoms which preceded the death of the deceased, and I have also read the report of the post mortem examination made of his body.—I also heard and read the report of the analysis made by Drs. Provost, Bruneau, Migneault and Girdwood. My opinion is, that from the symptoms as related, and from the reports of the autopsy and analysis, it is impossible to come to any definite conclusion concerning the cause of the illness.

My opinion is that the conclusions arrived at by the doctors who have been heard on behalf of the crown is a very uncertain one. The Medico-legal "expert" should have before him a report of the autopsy to enable him to arrive at any conclusion.

Before giving my opinion about the symptoms, I will review some of the evidence of the people who were present at the last illness, and here I would remark that symptoms described by witnesses who are not Doctors, possess no importance whatever as means of arriving at a conclusion. It is only when a medical man has observed the symptoms, that medical witnesses can arrive at a more satisfactory conclusion, which even then would, in many cases, not be correct. I will first take the evidence of Michel Lemaire, in which he says that the deceased retained perfect consciousness to the last moment, and that up to the last moment his breathing was free, that his eyes were sunken in the sockets. These symptoms we cannot say are those which are met with in a case of poisoning by strychnine. The breathing, in the last minutes of a case of poisoning by strychnine, is very labored and even completely suspended. The eyes are prominent, which has caused some medical men to say that they seemed started out of the socket, and the pupil is much dilated. Some days ago, since the commencement of this trial, I tried an experiment on an animal, and I found the eye prominent and the pupil dilated. I next take Dr. Ladouceur's report in which he states that on opening the chest he found an infiltration of blood in the peri-

cardium, and an effusion of more than a pint of blood in the right pleura and a little more in the left pleura, and also that upon making an incision into the sternum there was an escape of serum from the cellular tissues; that the heart was of a dark red, of a natural size and completely dilated in its auricles and ventricles; that its cavities were empty and presented the same appearance as the exterior. The dilatation of the auricles and ventricles indicate that the heart was not in a natural state. The exterior and interior color of this organ denotes the existence of an inflammation prior to death. This inflammation of the internal membrane seldom accompanies chronic inflammatory Rheumatism. The rheumatism may exist without it, but when it becomes acute and affects the articulations the membrane of the heart is generally found much inflamed and sometimes thickened. By the same report we find that the lungs were very much congested and had a blackish appearance especially at the posterior part, that they were very friable and that upon pressure there came from them a dark brown froth. This congested state of the lungs may be the result of the difficulty of breathing especially at the last moments, and the black aspect, especially at the posterior part, is a post mortem result caused by the horizontal position of the body. The report states that there was more than a pint of black blood in the right pleura and rather more in the left. The presence of this liquid, unless from some traumatic cause or wound of the pleura, is in my opinion only a quantity of coloured serum, which had accumulated there gradually, and which constitutes what is called hydrothorax. This effusion having taken place, gradually explains to me the feeling of constraint that existed. There were, says the report, small white elevations on the back of the lung, which upon being opened, gave out a froth of the same colour. This state must have been owing to the presence of tubercles which had passed into the softening stage; an indication that the lung had been unhealthy for a long time. In another part of Dr. Ladouceur's report, I see that in speaking of the brain, he says "The brain presented a natural appearance in all its parts. The dura mater was moderately congested towards the middle of the frontal bone; between this bone and the dura mater, was found a whitish substance very friable, and about a line in thickness." This state of the dura mater shows that the presence of a foreign substance, of a line in thickness, can only be the result of inflammatory disease, and from the presence of that substance the inflammation may have existed for some time. This state necessarily deranged the functions of the nervous system, and the presence of foreign bodies, may even bring on convulsions and considerably weaken the intellect. The arachnoid was strongly congested, which confirms my opinion regarding disease of the brain. I now pass to that part of the report which speaks of the opening of the abdomen. Speaking of the liver he says "it was of ordinary size strongly congested with black blood, the right lobe was softened and friable, especially at its posterior part." This pathological state of the liver can only result from an inflammation which existed before death. Speaking of the kidneys in another part of his report he says. "The kidney was slightly swollen and was much injected with black blood." This state is an indication of an inflammatory disease of the kidney existing before death. "The spleen was," says he, "of considerable size, and like the other organs contained black blood: putrefaction was far advanced." This state is also an indication of inflammation of the spleen. In another part

of the report: speaking of the stomach he says: The internal surface was covered with a thick black mucus. This black mucus could only be the effect of inflammation of the internal membrane of this organ. This opinion is corroborated by Drs. Provost and Bruneau, who, in their report of the analysis, say that the internal membrane of the stomach was much injected, especially at the large curve, where the mucus membrane was of a dark red and also showed large blackish spots. I pass to the intestines. Dr. Ladouceur says that the duodenum was of a reddish color and congested. The internal surface was also congested. Upon scraping this surface with a scalpel, I detached a layer of reddish brown mucus. This state is also an indication of inflammation of the duodenum existing before death, Dr. Ladouceur says that the ascending and descending portion of the colon seemed slightly inflamed. The rest of the intestinal canal was in an extraordinary state of putrefaction. This state was an indication of a severe inflammation of the bowels existing before death. The state of the intestinal canal described by Dr. Ladouceur, denotes an inflammation of the whole intestinal canal, and would be more than sufficient to explain death. The state of all the organs that were examined shows me that they were all unhealthy. The pathological state of the corpse, as described by Dr. Ladouceur, might mark an illness that would cause death with the symptoms described by the witnesses as being those of deceased, without its being a case of poisoning by strychnine. The chemical analysis as described and made by Drs. Provost, Bruneau and Girdwood, of a portion of the mucus detached from the internal membrane of the stomach, of the duodenum, and gall bladder, cannot, in my opinion lead to any certain conclusion. To come to a conclusion about any poisoning, it is not sufficient to say you have found poison, but you must show it with all its characteristics. In the present case the poison is not shown in a natural state with the characteristics that belong to it. To come to a conclusion of death from poisoning, even supposing poison to have been found, if it has not been found in sufficient quantity to cause death, it is indispensable to demonstrate the effects of the poison by means of physiological tests and these tests consist in administering to live animals a portion of the substance which is supposed to have caused the poisoning. To come to any certain conclusion it is necessary, that the purity of the reagents and instruments used, should be proved by analysis. I do not consider the color test, described by the medico-legal witnesses for the crown, infallible. I consider that putrefaction might produce a substance which would make the colour test illusory. This is the opinion of writers who are authorities in toxicology Orphila is still the best author in these matters, Dr. Taylor is one of the greatest authorities on these questions, that England has produced, and I think when he said that other substances might produce the colours, that he was right, as he had agreeing with him, men who stood highest in the science, amongst which were Christison, Orphila, Casper and Bloomhart. Nearly all organic matters contain the same elements as strychnine in different proportions. There are several substances composed entirely of hydrogen, oxygen, carbon and azote—such as strychnine, protein, brucine and morphine. The fibrine of the blood is composed of the same elements, but with different equivalents. These same alkaloids take juices which unite with the acids, to form the salts with different equivalents. This difference in the equivalents and in

the proportions, is not sufficient to make them perfectly different substances. All authors do not give the same equivalents. Rigidity of the body may come immediately after death, but it is not an essential condition of poisoning by strychnine that it should come on immediately. Strychnine given in a liquid, and dissolved in sufficient quantity to cause death, would take effect almost instantaneously, especially if the stomach was empty. In the first attacks, a person dying of strychnism would be able to breathe and speak, but as these attacks increase, the difficulty of breathing and speaking increase with them so much, that in the last spasm the patient can neither speak nor breathe. The moment a spasm commences the respiration is oppressed. I have been present at the death of a good many people; I have seen some die who retained their consciousness to the last moment. At the moment of life's departing, convulsions would supervene with rigidity of the limbs, a throwing back of the head and clenching the teeth. These are symptoms which manifest themselves in a great many diseases and inflammations, in some cases of phthisis and especially in certain cases of effusion in the thorax; they are also remarked in cases of death from inflammation of the intestines, or inflammation of the brain. In poisoning by strychnine, the face is of a bluish colour and the eye prominent, and it stays in that state; I should not consider a placid face at the moment of death incompatible with poisoning by strychnine. I consider that the symptoms described by the witnesses are not necessary and characteristic symptoms of poisoning by strychnine, but they may be symptoms, of other diseases of which the post mortem examination revealed the existence; I repeat the conclusion I have come to—that from the symptoms, the autopsy and the analysis, it is impossible in this case to come to any certain conclusion of poisoning by strychnine.

Cross-examined.—The only experiment I made as to the effects of strychnine was to poison a dog which died in two hours. The first effects manifested themselves in two minutes, I cannot say whether the dog had been fed or not, I poisoned him at night. The nervous system of a dog is very like that of a man. In dissecting rooms we use sheep brains instead of dogs' brains because it is easier to get the head of a sheep than that of a dog. In Lussier's case I did not conclude arsenic had caused death, because there was none, I never made any experiments to discover strychnine. I never analysed any substance with a view to finding strychnine. I should not believe in the presence of strychnine unless I found it in crystals in the body in which I was looking for it. Having thus obtained it, I should require a successful manifestation of the series of colours which are peculiar to it and also physiological tests which should produce results affirming its presence, for the colour test alone is insufficient. If the quantity found was not sufficient to kill an animal, I should not come to a conclusion of poisoning. The effects of strychnine usually manifest themselves in pain throughout the whole system, especially in the region of the chest. The respiration becomes oppressed and laboured, after that comes the stiffness of the jaws and often a bending back of the trunk, with convulsive shaking of the limbs, prominence of the eye and suspension of respiration, generally at the third or fourth fit death supervenes, sometimes in from twenty to thirty minutes and sometimes in from an hour to an hour and a half. Generally in the last spasms the symptoms become more violent and there is loss of consciousness. Loss of consciousness is not invariable. There are cases in which consciousness remains to the

last, but the prominence of the eye and the dilatation of the pupil are constant signs and are always manifested. All authors agree in giving these symptoms as invariable. The presentiment of death is not a characteristic sign of poisoning by strychnine. One of the signs is the knowledge the sufferer has of the coming on of a spasm. But this knowledge is not peculiar to poisoning by strychnine only. I agree with the following remark made by Orphila, vol. 3, page 784. "One thing worthy of remark that is observed in poisonings by strychnine, by false angusturus and by brucia, is that the touching a part of the body, a threat or a noise generally brings on the tetanic pains." I have no personal knowledge of any case of poisoning by strychnine. I never heard of a case in Canada or even in America. In a case of acute rheumatism the heart is often found in a state of inflammation, especially the internal membrane, I recently opened the body of a man who died from inflammatory rheumatism. I found the heart considerably dilated, the cavities on the left side empty, those on the right full of black blood. I never saw a case of inflammatory rheumatism in which the patient died suddenly. This patient was better in the morning and left his bed in the course of the day. At 6 o'clock in the evening, he was administered and he died in the night. In that case I also found an effusion of serum in the pericardium. We found in the stomach traces of an old inflammation. The kidneys were congested, the whole inside was nearly in the same state as that described by Dr. Ladouceur in the report of his *post mortem* examination. This man died from inflammatory rheumatism which I consider a symptom of inflammation of the heart. Authors do not agree upon this point. This man died neither of angina pectoris, nor hydrothorax, nor trichina, nor from poisoning by mushrooms, nor by strychnine. A man that is attacked with inflammatory rheumatism always suffers in some articulation, either the arms or legs. Sometimes the whole body will be swollen, and the slightest touch causes great pain. It is one of the most painful of all diseases: the least movement in the room will sometimes bring on the pains, which are generally in the articulations, sometimes in the chest and sometimes in the stomach, I saw the patient I mentioned about eleven o'clock and he died about midnight. He had taken some food, I had ordered him a little beef-tea or light soup. He was delirious, but had no shiverings. Marie Plourde's evidence seemed to me so strange that I don't think any reasonable man could attach any importance to it. It is not likely that a man would get dragged any distance at all, by the tail and leg of his horse. Admitting Marie Plourde's evidence to be true, I could not from it come to any conclusion as to the illness of deceased on the 22nd December, because a man in convulsions could not sit on a horse. I do not remember the symptoms described by Dr. Ladouceur on the 22nd December last. I have read the symptoms described in the trial of Palmer who was accused of murdering Cook—the trial took place in London in 1856, and was presided over by Lord Chief Justice Campbell. I know what medical evidence was adduced by the prosecution. I could not come to the same conclusions as were arrived at by the medical witnesses in Palmer's case, because in my mind you cannot conclude that there has been poisoning by strychnine if the poison is not found in nature, and demonstrated as I have before said. I cannot give any particular disease as the cause of Joutras' illness. He died of chronic inflammation of the stomach and liver, which brought on the disorders observed in the system

Hydrothorax, which supervened as a symptom of inflammation, might bring on sudden death. The state of the organs as described by Dr. Ladouceur in his report of the autopsy, indicate a disease of the brain, with inflammation of the lungs, liver, kidneys, spleen, duodenum and the rest of the intestines and also of the heart. After poisoning by strychnine, as a general rule, there are no traces of inflammation. Death from asphyxia may bring on congestion of the lungs. I do not think that in cases of asphyxia, there would be any effusion into the pleura, especially of serum. In the first stage of inflammation of the lungs there is engorgement ("engorgement") and if the disease continues there is effusion. Putrefaction cannot take place in twenty-four hours; and in that time an effusion of serum in the thorax could not be owing to that cause. When decomposition was established I should not be surprised to find serum in the thorax, because, the tissues being broken, the liquid must be deposited somewhere. Death by strychnine may take place from coma, asphyxia or syncope. The effect of strychnine on the system is that all the organs become congested. Pain could not cause death by asphyxia. Strychnine acts upon the spinal marrow, compressing the respiratory organs which brings on asphyxia, the effect of which is to destroy the functions of the other organs. There is then congestion or effusion of the brain. In cases of poisoning by strychnine, some have occurred where the two ventricles of the heart were empty, in others only one was empty. In inflammatory rheumatism a man might walk an hour before his death, especially if the rheumatism was chronic. I have assisted at a great many *post mortem* examinations. I never remember finding a white substance between the frontal bone and dura mater. There are sometimes small bodies in this region, which may exist in groups. The stomach is not congested in poisoning by strychnine, but in some cases it might be. All the diseases I have mentioned cannot exist at the same time. In twenty-four or forty-eight hours there might be a commencement of putrefaction. The first proceeding in an analysis consists in separating the poison from the animal matter in which it is contained. This is done in different ways by maceration in alcohol or acids, to destroy the flesh, the residue is then filtered, sometimes ammonia is added and sometimes carbonate of potassa. The object of adding these things is to precipitate the alkaloids, which being precipitated from the solution, remain in the deposit. This deposit is subjected to the proper reagents for the discovery of the different alkaloids. The object of each process is to obtain the alkaloid in solution and separate it from all organic matter. The process of maceration by means of alcohol is called Staas' process. Chloroform is now substituted for ether in Staas' process. When the chloroform has been separated from the liquid it is drawn off by means of a syphon. After complete evaporation, if the residue remains colored, it is calcined by means of a small quantity of sulphuric acid. Ammonia is then added to precipitate the alkaloid and chloroform to dissolve it. The chloroform is then evaporated and should the residue be colorless, it is treated with sulphuric acid and bichromate of potash, which bring out the different colors already mentioned. So long as the residue remains colored it should never be tested by the reagents, but should be subjected to fresh treatment until all coloring matter is removed. Staas' process differs from the one I have just described, because alcohol is used, to separate the poison from the animal matter, to which

tartaric acid is added from preference. The first process is that of Angers and Flaudin, and the second is Staas', Staas was never preferred as being the most efficacious in removing all colouring matter, but this process is a difficult one, and even nearly impossible to those who are not in the habit of manipulating chemicals. The process of Rodgers and Girdwood mentioned by Dr. Girdwood is that of Angers and Flaudin, to which has been added sulphate of magnesia and chloroform. To Staas' process chloroform has also been added, hydrochloric acid is not used in Staas' process. This new process is that of Angers and Flaudin with the addition of sulphate of magnesia. The process adopted by Dr. Taylor, in Palmer's case, is the same as Rodgers and Girdwood, with this difference, that in the last one chloroform and sulphate of magnesia are added. In Staas' process sulphuric acid is used. Sulphuric acid destroys organic matter and precipitates alkaloids, I cannot say who first recommended sulphuric acid to destroy organic matter, but it is in Staas process. I think sulphuric acid would destroy any organic matter. When there is no organic matter left, the residue becomes colorless, and it is then that reagents are used. When the organic matter is entirely removed so that the residue becomes colorless; if the tests upon application produced the series of colors peculiar to strychnine, I should conclude there was strychnine present. The series of colors given by strychnine, are those described by the witnesses for the crown: blue, violet, purple and red. I know of no substance but strychnine, which, being separated from organic matter would produce this series of colors. If I were called as a medical Jurist, and that the chemical analysis revealed strychnine; if the symptoms remarked at death were those of poisoning by strychnine, I should conclude it was a case of poisoning by strychnine. The symptoms described by the witnesses are incompatible with those of poisoning by strychnine as characteristics, as each one of them rebuts the idea of poisoning. I swear this man did not die of strychnine, as each of the symptoms repels the idea of poisoning by strychnine. I said that according to the report of the autopsy, from the state of the heart, there were more than sufficient diseases to cause death. There was a general state of suffering. I am still of the opinion that the general state of suffering he was in, might bring on these diseases. It is impossible to be sure of the cause of this man's death, but what I can say is that he did not die of strychnine. I said that, as a general rule strychnine did not produce any alteration in the stomach. If it was administered in alcohol it might. If I had examined two hundred bodies, without finding any, I should not because I did find strychnine, conclude that it was a case of poisoning. If this man had had all the inflammations that we observed, and that strychnine were given to him, he would die of strychnism: but he did not die of strychnism because the state of the organs does not indicate strychnine. Inflammation of the liver might cause the illness of the 22nd December. The symptoms given by Madame Cajolette might exist in diseases of the liver. The "ensemble" of the symptoms are not those of poisoning by strychnine, although some of them exist in it, but a diagnosis can only be based on all the symptoms together, which points out which organ it is that is suffering. The bending back of the body is a symptom met with in strychnism, but to base a diagnosis upon it, it must be accompanied with other characteristics. This symptom is found in diseases of the liver, in epilepsy, or in hysteria, I have myself seen cases of

hysteria and epilepsy where this arching of the body existed. This might also occur in angina pectoris. Taylor says hysteria is not a common disease amongst men. Nearly all the symptoms of poisoning by strychnine, given by Taylor are met with in Idiopathic and Traumatic Tetanus, but I agree with Taylor in what he says. Amongst the symptoms given, I find this bending back of the body and the sensitiveness, I should call "tetanic convulsions" this bending back of the body spoken of by Madame Cajolette and the general state of sensitiveness that rendered him sensitive to everything, to the touch. The deceased also kept saying he was going to die, and I also remark that his jaws were locked as described by Taylor. I remark that as soon as the Doctor gave him his remedies that he got better. In cases of epilepsy consciousness is lost during the attacks, even when the patient is exhausted, once there was an effusion, convulsions might arise from it, the poison which is absorbed (I speak of strychnine) can not be found in the blood, nor in the urine, nor in any of the secretions. I do not believe in experiments made by people who are not recognized by the science. When an experiment has been made it is customary to submit it to academicians for approval of the operations, and if that approval were granted I should then believe in the authority. I do not think poison is formed in the body, I have seen no case of poisoning by strychnine and I never made an autopsy in such a case. All I know I have learned from the study of books, To obtain a correct result it is necessary to analyse the tests (reagents) glasses cannot be analysed, if I said this morning that the instruments should be analysed, I made a mistake, but the certainty should be obtained that they are clean and contain no poison. I said that strychnine could not be found to a certainty by the color test but that it should be found in crystals. If, however, being separated from all organic matter it produced the series of colours and the physiological effects, I should conclude it had been found. If there is enough strychnine to produce the series of colours, there is enough to crystallise, even if completely dissolved, strychnine can be brought back to its primitive state. Medical science is founded in great part upon experience. I know of no other substance than strychnine that will produce this series of colours. The series of colours is only fit to individualise. I do not admit that there is only one substance which produces this series of colors. I would admit that a crystallised substance producing those colours would be strychnine, and even that a substance which was separated from all organic matter and produced those colours would be strychnine. Any powder which might have been thrown on the stomach during the autopsy would localize itself, but I would remark that bile contains matter that might decompose strychnine. I do not think that supposing strychnine to have been thrown on the stomach, it could have got into the other parts. A man who had had all the symptoms remarked, and who died on the 31st., of inflammatory rheumatism, might have thrashed in a mill as described. Inflammatory rheumatism may become localized and cause sudden death. The symptoms of any disease have an infinity of variations. There are no two things alike in nature, and mens' constitutions vary in the same way. The same disease may exist in two men with different characteristics. A powder thrown on the stomach, duodenum &c., could not spread to all the parts and could not be found unless it was spread over them. Amongst the symptoms described I did not find these: "raging thirst" "prominent eye" "livid coun-

tenance" "the wish to be turned over" "loss of consciousness," after the series of colours has been obtained to make sure it is necessary to have the physiological effects. In case of a powder being thrown on as insinuated, I do not think it could have been found as it was.

Louis Joseph Moll, M.D., states: I have been practising about 27 years. I heard the symptoms given by the witnesses who were present during the illness of Frs. X. Joutras. I have read the report of the autopsy and the analytical reports in the case. From what I have heard and read I consider the symptoms given by the witnesses as very doubtful, and deceitful as indications of poisoning by strychnine.

In Dr. Ladouceur's report, commencing with the brain, he describes a strong congestion in all the membranes; he describes a little serosity in the ventricles, and the existence of a whitish friable substance about a line in thickness, which exercised a pressure on the brain. Passing on to the chest I first take the heart, the internal membrane of which was of a darker colour than natural, and the exterior appearance of this organ also presents a darker color than in a normal state. The dilatation of the auricles and ventricles of the heart denotes a strong pressure of blood, which would have increased the size of this organ. From the morbid pathological state of the heart I conclude that this organ must have suffered from some inflammatory disease, either chronic or acute, previous to the death of deceased; and I say *previous to*, on account of the effusion of about two ounces of dark bloody serum which was found in the pericardium. Dr. Ladouceur describes both lungs as filled with black blood, with spots of even a darker colour at the lower and posterior parts, with white excrescences which, upon opening, yielded a white froth; and in the cavities of the pleura, i. e., that part of the serous membrane which covers the lungs, Dr. Ladouceur's autopsy shows us in the right pleura a little more than a pint of sanguinary serous matter of a very dark colour, and in the left cavity an effusion of the same liquid, with the same colour in rather larger quantity, without giving the exact quantity. Passing on to the abdomen, I find from Dr. Ladouceur's report, that the stomach contained a blackish mucus, and in the report of the analysis we are told that the mucus in the stomach was of a dark red colour, especially towards the large curve, and further there appeared brownish grey spots spread over the surface of the mucus. Dr. Ladouceur represents the intestines as being in a very advanced state of putrefaction. The liver, without anything very strange being remarked about it, seemed a little larger than natural. The spleen was swollen and much congested. The state of the brain might bring on a nervous disease accompanied with convulsions. The state of the heart, with the effusion in its covering; the strong congestion of the lungs, with the accumulation of liquid above mentioned, in the cavities of the pleura on both sides, might cause apprehension of imminent death. The well pronounced state of inflammation of the stomach, and the commencement of gangrene noticeable, (for no other name can be given to the inflammation remarked in the mucus of that organ,) the advanced state of putrefaction of the intestines, which denotes a chronic inflammation, or one which has lasted some time, are certain indications of a speedy death; and I come to the conclusion that the deceased may have died either from the lesions stated to exist in the thoracic cavity, or from those existing in the abdominal cavity; such lesions as above described, threaten

in themselves, and each one by itself threatens a speedy and certain death. It may sometimes happen that in some of these diseases, and especially in morbid lesions found in the organs, in the thoracic cavity a nervous disease declares itself, which would then be looked upon as a symptom of the lesion of the organs, and such nervous disease might have symptoms similar to those described by the witnesses of Frs. X. Joutras' death. There are several nervous diseases which resemble that of which deceased died, but the one most like it is angina pectoris in a violent attack, there are two anginae, one idiopathic i. e. which comes on without any organic lesion and one symptomatic which is brought on by organic lesions similar to those remarked in the autopsy. To show how sudden and dangerous this disease is from a statistic of Dr. Forbes' it is shown that out of 164 cases 94 died. I heard Watson quoted from, according to this author I can say that in angina pectoris there is always an intermittance, else the patient could not exist—it is the same in all nervous diseases. There are tetanic symptoms in angina pectoris when it is very violent, otherwise they do not always exist. In this disease there is often fear of death, anxiety an agonizing feeling, tetanic convulsions and contractions? Such cases are rare, but you must not conclude that they cannot occur. From the lesions of the heart, in a word, from the endocarditis which, judging from the effusion, must have existed sometime, the heart was unable to receive the quantity of blood sent to it by the lungs, the residue of the blood must have remained in the bronchial capillaries, and coagulated gradually. The first part of coagulated blood would rapidly decompose, and the serous portions of it must have filtered through the pleura, to produce in its cavity the effusion of bloody serum that was found there; in this way the blood would gradually accumulate and coagulate, and congest and render the lungs more or less incapable of performing their functions i. e. oxygenizing the blood. The difficulty of breathing in the last moments may have brought on the attack of angina pectoris with great violence, the series of symptoms of which, would give an "ensemble," such as remarked by the witnesses who were present at Joutras' death. What makes me believe in hydrothorax more still, is that there was a well defined anasarca condition, for Dr. Ladonceur tells us that when he made an incision on each side of the sternum, there escaped from the cellular tissue covering the sternum, a quantity of serum, which makes me believe in the existence of hydrothorax prior to death. I mean to say that in experiments for the discovery of strychnine or any other vegetable poison, certain processes are adopted by those who operate, and that the results have been very uncertain. Some authors such as Christison, Angers, Fandin and Orphila tell us that the results obtained by means of reagents are very uncertain and illusory. Other authors pretend they can always find it if it exists, and think they are able to discover it into the last fibre of the human organisation. Not having had any personal experience in these cases, I form my opinion upon such authorities as I should consider the best established and the most reasonable; I consider that a medico-legal witness in this kind of evidence should give his report with all conscientiousness and after the utmost possible reflection, if his report is to be believed by the jury, and that he should only give his evidence upon the best possible proof. To arrive at a certain conclusion of a case of poisoning, it is first necessary to ascertain the existence of poison in the victim, you must then demonstrate the finding of it, and if possible show it in its natural

state; after this to corroborate, for many poisons resemble each other in appearance, it should be tested, physiologically. The physiological test, I consider as indispensable to enable you to draw a certain conclusion. Protein and other things contain the same elements as strychnine but with proportions that vary according to different authors. Prussic acid, which certainly had never been administered, has been discovered in Protein by means of reagents. A powder sprinkled on the outside might filter through any organ it was thrown on, but could not be passed from one to the other. From the symptoms described in the post mortem examination and chemical analysis, it is impossible to conclude, with any certainty, that Frs. X. Joutras was poisoned by strychnine. I have had no experience in chemical analysis—I never saw a case of poisoning by strychnine except on a dog—I never made a post mortem examination in such cases, nor have I seen the ante-mortem symptoms verified. I never made any chemical analysis of any sort. Strychnine has never been produced in protein by means of reagents. Gangrene may not have been far advanced on the 31st. An inflammation very soon develops itself, from the commencement of inflammation of the intestines up to the time of suppuration might take about eight days. The inflammation of the intestines to the acute stage could not have taken more than eight days. The congestion of the brain might be caused by the congestion of the lungs. Endocarditis is not a disease that places life in danger. The only way it could endanger it would be by preventing the heart from receiving the quantity of blood that was sent to it. This endocarditis must have existed for two or three months before his death. Hydrothorax may have been caused by the endocarditis. The disease of the liver and spleen may have been caused by congestion of the organs. The disease of the heart must have brought on congestion of the lungs. I have seen people, who died of gangrene of the stomach, walk about and smoke an hour previous to death, with their intestines in a state of putrefaction. A man that is attacked by it cannot live long. The degeneration, the ossification of the coronary arteries and the diseases of the valves of the heart are morbid symptoms of this disease. I consider the symptoms described by the witnesses, more compatible with angina pectoris than with poisoning by strychnine which leads me to say that they are incompatible with strychnism. I understand the symptoms of poisoning given by the witness. I think Flaudin and Taylor say that there are other matters, besides strychnine, which give the same series of colours.

DR. JEAN FRANCOIS REGIS EDELMAR ST. CYR.—States from the description of the symptoms, as given by the witnesses, from the reports of the autopsy and of the chemical analysis, I cannot as a doctor conclude that Frs. X. Joutras died from poisoning by strychnine. The symptoms in the last illness of deceased are no proof that death was caused by strychnine. The autopsy mentions organic lesions, more than sufficient to cause death. The series of colours is something, but I am of opinion that you cannot come to a definite and certain conclusion concerning the presence of strychnine unless you test it physiologically. The anatomical lesions mentioned in the autopsy might have produced nervous attacks and brought on angina pectoris. The "ensemble" of the symptoms described by the witnesses of Joutras' illness exist in angina pectoris. Angina Pectoris declares itself suddenly after an illness, that would leave in the body of the sufferer similar lesions to those mentioned as existing in Joutras'. In the symptoms descri-

bed by the witnesses I remark the absence of one essential symptom of strychnism—the prominence of the eye which seems as if starting from the socket. The suspension of respiration is also an essential symptom of strychnine for strychnine when absorbed acts upon the spinal marrow, which in its turn acts upon the muscles of the respiratory organs and impedes their function. I think it would be impossible for a man under the effects of poisoning by strychnine to pray up to the moment of his death. A man who complains of rheumatism, lassitude in the legs, and who was subject to nausea and pains in the stomach and was depressed, might be suffering from a disease which would produce the lesions remarked in the autopsy. An attack of rheumatism might cause the lesions remarked in the heart. The congestion of the lungs must have taken place gradually, as the circulation must necessarily have been decreased. In such congestion of the lungs, the watery portion of the blood would, in time, necessarily separate from the solid part, and by filtering through the capillary bronchial vessels, gradually accumulate in the cavity of the pleura, and cause what is called hydrothorax. This effusion is owing to some impediment to the circulation of the blood, and this impediment or defect in the circulation easily explains the state of anasarca of the cellular tissue covering the sternum. I find in hydrothorax a cause of angina pectoris of a violent attack of which Frs. X. Joutras, in my opinion died. The extraordinary state of putrefaction stated in the autopsy and the pathological state of the stomach as remarked in the report of the analysis, are indications of another disease which might cause death. I should call this disease, inflammation of the stomach with a commencement of gangrene.

To Mr. Armstrong.—I have been practising medicine for six years. I never made a special study of chemistry. I never attempted to find strychnine in a body by chemical process. If by means of reagents I found the series of colours peculiar to strychnine, I could not come to a certain conclusion without a physiological test. I have my doubts concerning a series of colours produced by bichromate of potash, because, according to Dr. Letheby, this is the worst reagent of any in use. I approve of the process employed by the Drs. who made the analysis, but I think that having found strychnine in abundance, as they have stated, they ought to have made a physiological test of it. However small the quantity of strychnine found, it would produce, if not death, at all events symptoms of poisoning upon certain animals. I cannot say that the gall-bladder would absorb more strychnine in proportion than the intestines. From the word "abundance" made use of by the Doctors who performed the analysis, I should understand that they had found sufficient strychnine to make a physiological test. With the $\frac{1}{1000}$ part of a grain I think a physiological experiment might be made on a mouse or on a frog. I do not know how much strychnine it would take to affect a mouse. I agree with the opinion of Dr. Letheby who says that $\frac{1}{4000}$ part of a grain of strychnine would be sufficient to produce the series of colours. A hydrothorax which killed a man on the 31st of a month, might have produced tetanic convulsions on the 22nd of that month, but I do not think it could also have produced them on the 24th and 29th of that month. In the attack of the 22d December, I find some of the symptoms of poisoning by strychnine, such as the jerking of the limbs, the bending back of the body, the interval between the spasms, the fear of death, the nervous shock caused by

a sudden noise, the foreseeing a fresh spasm. I do not attach much importance to Mme. Cajolette's evidence, because she, by herself, could not have given a series of symptoms like those, and besides if these symptoms had really existed, they would have been remarked by the doctor upon his arrival, as we must suppose that they still continued. In comparing the symptoms observed on the 22nd with those of the illness of the 31st, I do not in the last attack find all the symptoms existing with the degree of violence they must have had to cause death. These symptoms compared, do not indicate the same disease. In my opinion on the 22nd, the symptoms observed were those of a violent attack of angina pectoris, and on the 31st, the gradual congestion of the lungs together with nervous attacks, caused Joutras' death. I agree with Christison and Taylor upon the "ensemble" of symptoms produced by strychnine. Symptoms in diseases vary in intensity and in character. If the illness of the 22nd had been caused by strychnine, Dr. Ladouceur would have perceived the symptoms which characterise poisoning by it. In angina pectoris of which the distant cause should be rheumatism, and the determining cause hydrothorax there would be lesion of the heart and lungs. To the Court—In one disease symptoms are remarked which are not remarked in another. I know of no substance but strychnine that will produce the same series of colors. There can be no doubt of the presence of strychnine, if after producing the colors it is tested physiologically. I think, but I am not certain that the quantity of strychnine that could produce the colors might be sufficient for a physiological test. If there is sufficient strychnine in an organ to repeat the color test with several times, I think there would be sufficient for a physiological test.

CURE OF OPAQUE CORNEA.

If we are to believe M. de Luca, opacity of the cornea is no longer one of those intractable affections which shame the Doctor in the public mind. In a memoir just presented to the French Academy, the *savant* we have mentioned states that he has found that sulphate of soda has the power of removing corneal spots in an almost incredibly short space of time. M. de Luca was led to experiment with this reagent from the circumstance that it maintains the fibrine of the blood in a state of solution. In the first trials he employed the sulphate dissolved in distilled water. The liquid he allowed to fall drop by drop on the ball of the affected eye, and the result was that after some days' treatment the opacity was to a considerable extent diminished. It then occurred to him to try the sulphate in the state of fine powder. On using it in this condition, and allowing a few particles of powder to fall upon the eye, a more decided result was obtained—in one instance, a patient who had been previously almost completely blind regained a certain amount of distinct vision. These results are, if true, exceedingly remarkable. We trust English ophthalmic Surgeons will give the new remedy a trial, and we hope that the beneficial effects may not be of an evanescent character.—*Medical Times & Gazette.*