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ART. LV.—*The Hip-joint: Considerations on its injuries and diseases, deduced from the anatomy.* *By* S. J. STRATFORD, M. R.C.S. England, Toronto. *Continued from No. 9.*

DISLOCATION OF THE FEMUR UPON THE CREST OF THE PUBIS.

In former numbers of this Journal, we have pointed out the three most frequent varieties of dislocation of the head of the femur—the first being upon the dorsum of the ilium, the second into the sciatic notch, and the third into the thyroid hole; it now remains for us to speak of the fourth and last variety; that which happens when the head of the femur is removed from the cotyloid cavity, and placed upon the crest of the pubis. The causes of this kind of displacement of the os femoris are generally of a character very similar to those which operate upon the bones in the variety last described; the flexure of the thigh upon the pelvis has, however, been less extreme at the moment of the accident than it was in the former case, dislocation of the femur into the thyroid hole. In this variety of displacement, the angle of the thigh upon the trunk would represent an obtuse angle, while in the dislocation into the thyroid hole, a right angle, or even an acute angle, might be considered as representing the position of the body and thigh; at that moment of time when the abducting power acting upon the extremity of the limb, has been powerfully called into play, and through its influence upon the bones of the pelvis, it has been sufficient to raise the head of the bone from the cotyloid cavity, and to tear the strong ligaments of the joint. It would also appear that the extreme eversion of the toe at this instant presented the head of the bone at the weakest point in the whole joint, at the deep notch or deficiency in the marginal wall of the acetabulum. Powerful abduction under these circumstances causes the dislocation, and operates upon the extended limb as upon a lever, while the trochanter major, resting upon the bones of the pelvis first, and secondly, the neck of the femur operating upon the edge of the cotyloid cavity as upon a fulcrum, serve to direct the head of the bone in its abnormal course; the force being continued at an angle less than when the head of the bone was forced into the thyroid hole, it is thrust in upon the os pubis, and lodged under the psoas magnus, and iliacus internus muscles—pushing up the tendons of these muscles under Poupart's ligament, so as to interfere with the spermatic passage. This accident may happen when a man walking forward, unexpectedly puts his foot into a deep hole; this reaches the ground at a much greater depth than was anticipated—the thigh continuing in the perpendicular, the body is suddenly thrown backwards in the effort to recover the true position, but failing to relieve or retract the leg, which is, perhaps, stationary—fixed in a hole—the patient, twisting his body, falls upon his side, powerfully abducting the limb from its fellow; now the head of the femur resting upon the weakest point in the capsular ligament, and most imperfect portion of the cotyloid cavity, this yields to the impulse. In this position the weight of the body suddenly comes upon these parts, lacerates the ligament, dislocates the head of the thigh bone, and places it upon the crest of the pubis.

The symptoms presenting themselves upon our examination of the patient while laboring under this variety of displacement, are, that the limb may be somewhat shorter than its fellow; this, however, is not so extreme as in some varieties of fracture of the neck of the thigh bone, with which it is said this dislocation may sometimes be confounded; indeed, from the position of the head of the bone, resting upon the pubis, it generally lies in a plane scarcely in any degree elevated above the acetabulum—so that this shortening of the limb is difficult to be demonstrated in many cases. The limb is always flexed upon the body, abducted from its fellow, and the toe is rotated outwards; the head of the thigh bone may be plainly felt, and even seen moving in the groin, upon the least attempt to rotate the limb; it may be observed lying external to the femoral vessels and nerves.

The action of the muscles upon the femur, although similar in direction in this variety of accident, must be somewhat different from the influence they exert when the head of the bone is placed in the thyroid hole; in this instance, the head of the bone, instead of descending far below the line of the acetabulum, is slightly raised above it; at the same time it is advanced more forwards, as well as towards the median line. The trochanter major is thrown completely backwards, and now lies against the cotyloid cavity; so that the result is, that the rotatory muscles at the back of the thigh are called into action; the pyriformis, the gemelli, the obturator internus, and quadratus, with all the posterior fibres of the glutei muscles, are evidently placed upon the stretch by the change of position; hence the permanent abduction of the limb, and the complete rotation of the toe outwards. The influence of the obturator externus muscle is almost completely annihilated, the head of the bone is clearly advanced towards its origin, while the point of its insertion by the changed position of the trochanter major, is brought in close apposition with it. The action of the psoas magnus, iliacus internus, and pectinalis muscles, must be somewhat diminished from the rotation of the toe outwards, and the advance of the trochanter minor—the insertion of these muscles towards their origin—hence, their loss of power. It is also certain that from the position of the bone, they have lost all power of opposing those muscles, which cause the abduction of the limb, and, consequently permit their full influence to come into action, so as to maintain the limb in a state of abduction, and the toe in a condition of evertion.

To restore the limb to its normal position in this kind of dislocation, we must call to mind the principles so often enunciated in the preceding varieties of displacement of the head of the femur from the cotyloid cavity; as in the description of accident last mentioned, we must abduct and gently flex the limb—this will relax all the muscles on the back of the hip—then, with powerful eversion of the foot at the same time, we shall turn the head of the bone towards the cotyloid cavity, and the muscles will restore the femur to its proper position. The same bony deficiency in the margin of the cotyloid cavity which facilitated the original displacement, now favors the return of the head of the bone into the acetabulum; the ligamentous and fibrous defences at this point having been destroyed, offer no impediment to the return of the bone—hence the facility with which the reduction of this dislocation is often accomplished by the simple means we have advocated. It must be clear that extension of the limb in this case, by means of pullies, must be perfectly unnecessary, and cannot fail to be painful and injurious, as the head of the bone now lies almost parallel, and little removed from its true position, save that it has advanced considerably towards the median line. During our attempts at reduction, if we should flex the limb too much upon the body, there is a possibility that we may carry the bone round into the thyroid hole; care, then, must be taken in all these cases that we do not very considerably exceed the obtuse angle, while by abduction we free the head of the bone, and by rotation present it to the opening in the cotyloid cavity. Should we have injudiciously applied our means of powerful traction, while the limb was still in a state of extension, we might possibly remove the head of the bone from its present position, but should be more likely to lodge it in the thyroid hole, rather than to return it into the acetabulum.

We trust that the lengthened detail and minute description of the several varieties of dislocation of the hip-joint which we have here ventured to present for the consideration of our readers, will have succeeded in convincing them, *that knowledge is power*,—that a due and scientific estimate of position, a proper understanding of muscular action, with a correct appreciation of mechanical force, will far more easily accomplish the end we aim at, and is preferable in every point of view to the employment of direct and powerful traction by means of pullies—that it is far less likely to produce evil consequences, such as laceration of the muscles, arteries, or nerves, and is certainly less likely to produce the pain and inconvenience to the patient which is sure to be caused by the powerful extension we have alluded to. Without a just comprehension of all the facts which present themselves in each variety of these dislocations, the pullies may, as it were, by accident, in some cases, restore the bone into its socket; but reduction cannot be accomplished with certainty and precision, without a clear and accurate knowledge of the true principles which should obtain in all these cases. The axiom which we have most strenuously endeavored to impress upon the surgical practitioner, and which we have repeatedly attempted to illustrate, is, that *we always endeavor to return the head of the bone to the articulating surface, by a course precisely the reverse to that which it took during its removal: bringing it back as it were by returned steps, from the position in which it is lodged, until we can accomplish the due arrangement in its normal situation.* We apprehend this is the true secret in all these cases of displacement, that will enable us readily to relieve our patients, and we flatter ourselves that when it is guided with science and knowledge, will seldom fail us in our attempts—it will be a certain demonstration that will elevate the surgeon above the quack—will indicate the difference between science and chance—while it will certainly proclaim to the world that even in the study of surgery the old and long established proverb above alluded to, is not without its application.

CONGENITAL DISLOCATION OF THE HIP-JOINT.

That dislocation of the hip-joint may occasionally happen prior to birth, is a fact clearly established; and that it happens subsequently to that period, from causes which have previously happened to the fœtus when within the uterus, is no less certain. Congenital dislocation is always dependent upon insufficient development of the coxo-femoral articulation, or malformation of the bones of the pelvis; it may proceed from an immense variety in the character and grades of these congenital defects, each of the component structures of the part being occasionally the direct or indirect cause of the accident. Among the vast variety of these conditions, we find that the head of the femur may be deficient, it may be smaller than natural, or it may be flattened before or behind, according to the direction of the limb—so, also, the neck of the femur may be diminished both in length and in thickness, while but a rudiment of these parts may sometimes be present, or even these may be entirely absent, so that the upper end of the femur terminates with the trochanter major. The cotyloid cavity may be increased in depth, reduced to a slight depression, or completely defaced. Again, it may be large in size, in proportion, to the head of the femur—the cartilage covering the different surfaces of the articulation may be more or less deficient. The round ligament may be morbidly short, inordinately increased in length, or completely absent. The capsular ligament may be extremely lax and greatly increased in size in every direction—or it may be morbidly contracted and greatly thickened. We may also find that the muscles around the joint undergo changes incompatible with their normal condition—they may be simply discoloured—be transformed into a yellow tissue of a fatty appearance, or be entirely wanting. When congenital dislocation is connected with deformity of the pelvis, we find a lateral compression of the sides, and a corresponding increase of the anterio-posterior diameter, or these conditions may be completely reversed. The tuberosities of the ischium may be deformed, so as considerably to change the outlet of the pelvis, while the bones may suffer from this disturbing influence, so as greatly to derange all the dimensions of its cavity.

It is evident from the above description that the original vice of conformation may produce consequences varying vastly, according to the period of fœtal life at which it may originate; for the earlier its advent the greater will be the changes that may result from it. In some cases it is clearly sufficient not only to change the character of the head of the thigh bone, to alter or totally destroy the cotyloid cavity—but also to compress and deform the bones of the pelvis, so as in after life to produce the most grave and serious consequences, impeding, or totally preventing natural delivery when the accident shall occur in the female—while these changes may also incidentally influence not only the spine, turning it from the perfectly strait and erect position; but it may also alter the size, shape, and direction of the inferior extremity—impeding its normal development, and presenting it as a lasting deformity. Again: the vice of conformation may be so trifling, that it may not be observable in early life, or influence the due and proper motions of the hip-joint, until some slight accident develope the deformity—produces dislocation of the femur; and, were we ignorant of the facts which we have here presented to our readers, we might be sorely puzzled to find displacement of the powerful hip-joint occurring from so trifling a cause, that we should feel inclined to doubt the evidence of our senses, and might be misled in our diagnosis in consequence.

Should the head of the femur be removed from the cotyloid cavity at an early period of fœtal life, from any deficiency in the coxo-femoral articulation, the thigh-bone may rest in every variety of position upon the bones of the pelvis; when the trochanter major alone remains, it may form a false joint with the bones of the pelvis, and here often a mere sulcus or depression marks its position. We may now find the cotyloid cavity completely filled up and entirely obliterated, while the complicated action of the muscles at this early period, from the false position of the bones, mould the pelvis into a great variety of forms. We find some of these muscles, according to the position of the bones, inordinately developed; hence their power or form from such causes; some may be found greatly reduced in size, or perhaps totally obliterated. Thus, we find causes in themselves at first of the most simple character, producing great and powerful effects; these operating upon the soft and pliant skeleton, while yet scarcely more than cartilage, they will produce effects more or less extensive according to the cause that shall produce or influence these deranged actions. We should remember that these causes may operate upon one articulation only, or influence both sides simultaneously, and when such is the case, more or less deformity is the certain result of these several varieties in the early vice of conformation.

That surgery should be able to rectify or remove many of the vast variety of defects and deformities that we have here detailed, is totally out of the question. In some very grave varieties, however, the proper application of means may not be completely unavailing; indeed, in all cases, our ability to remedy these congenital defects, such as dislocation of the femur, must depend upon our power to reduce the displacement of the bone—and this reduction, it is needless to say, we can only hope to accomplish when the vice of conformation has been so limited as not to permit the removal of the head of the bone from the cotyloid cavity until the bones of the pelvis have obtained sufficient solidity to prevent any very extensive deformity of the pelvis; or when the displacement has been sufficiently recent for us to hope that its return to the normal position may make a permanent and useful joint. Our diagnosis of the variety of dislocation, and our knowledge of the mode in which reduction of the bone should be accomplished, will here be called into exercise. When returned into the proper position, the joint should be firmly strengthened by a splint extending from the trunk to the ancle; the portions of the splint should be strait, having hinges which play in the region of the hip-joint and knee; and was this properly secured to these parts by straps around the body, the pelvis, the thigh, and the leg, it would so confine the lateral movements of the joint, and prevent their excessive operation in any direction, while it would also prevent the limb being acted upon as by a lever, and would guard against the head of the bone being again thrown out of its socket. As a matter of course the continued employment of these means must greatly depend upon the circumstances of the case; the time that has elapsed since the accident occurred—the amount of defect in the parts concerned—these must be our guide, and decide the continuance of such means. If the vice of conformation is but trifling, and we have succeeded in reducing the dislocation at an early period, nature and proper attention may soon cure the accident, and may enable us to remove our splints within a moderate time; but if the condition of the parts prove more unfavorable, we may find it necessary to employ the means to strengthen the joint during after life.

FRACTURE OF THE NECK OF THE THIGH-BONE.

During our consideration of the several varieties of displacement which may happen to the coxo-femoral articulation, we flatter ourselves that we have found correct anatomical data the only true basis on which to found a certain and reliable appreciation of their character and direction. So, also, shall we find that a similar knowledge is the only means that will surely lead us to a proper comprehension of the many varieties and complications to which fractures of the neck of the thigh-bone is liable.

We have shown in our consideration of the different kinds of dislocation which may happen of the head of the femur when removed from the cotyloid cavity, that the application of indirect force acting upon the limb or applied to the trunk, constituting it a lever, operating upon the articulation, is the positive cause of the accident. So, in these cases of fracture of the neck of the thigh-bone, the application of direct force will be found the means which invariably produces this variety of accident. The direction of this force may be infinitely varied. It may be from without inwards, by a blow upon the trochanter major, as when a fall upon the hip drives the head of the bone into the cotyloid cavity, or from a power propelled in the same direction. The influence of the force may proceed from above downwards—as, for instance, when a person, having made a false step, descending a stair, has not reached the bottom as soon as he had expected—consequently the whole weight of the body has been thrown directly across the neck of the thigh bone, and that in its most unfavorable direction.

When the force that causes the fracture is from without inwards, it may strike full upon the trochanter major in a direct line with the neck of the bone, so as to drive it with force into the cotyloid cavity, and here meeting with sufficient resistance, the neck of the bone acting as a wedge, may burst up the superior portion of the femur between the trochanters, when it may be implanted in the bone, and become wedged into its cancelli; and if the direction of the force be perfectly true with regard to the axis of the neck of the bone, it may remain fixed, and firmly held by the compact vitrious substance of the circumference of the bone, the walls of the cancelli having yielded, so that the neck of the bone is driven deep into its structure, and being immoveable, the characteristic data of fracture, crepitation must be wanting. In the consideration of this force, we must not forget the result of those changes which occur in the neck of the thigh bone, in youth and in old age. In youth the neck of the thigh bone stands off at an obtuse angle, is longer and firmer than in aged subjects; while in advanced years, the external dense portion of the bone appears atrophied, leaving a thin shell inclosing the cancellous texture; the neck, also, obtains a right angle, losing the obliquity which it formerly presented; and this condition may be particularly observed in the female; possibly in some degree dependent upon the greater width of the pelvis. At this period, also, these structures become extremely brittle. Under such circumstances it is more than probable that the application of the same force I here allude to will be attended with different effects in these various conditions. The blow upon the trochanter major that would at one period cause a transverse fracture of the neck of the thigh bone, would at another time break up the superior portion of the shaft of the bone into fragments.

Should the action of the force be less direct, not exactly parallel with the neck of the thigh bone, as in the injury just alluded to, its effect upon the superior portion of the shaft of the femur may be more extensive—the neck of the thigh bone acting as a wedge, may break it up, separating one or both of the trochanters from the shaft of the bone. Under these circumstances, the numerous muscles inserted into the part may cease to have any effect upon the shaft of the femur. These fractures may have occurred without implicating the capsular ligament of the joint; while in other cases these structures may have participated in the injury which has been inflicted upon the neck of the thigh bone. Should the direction of the force assume a greater angle with the neck of the thigh bone, which may readily occur in young people, when this structure obtains an obtuse angle, such a force acting superiorly upon the trochanter major, may cause a fracture of the neck of the thigh bone within the capsular ligament. Such are the varying influences which obtain in youth and old age.

Should the direction of the force be from above downwards, instead of being from without inwards, as we have pointed out; the oblique position of the neck of the thigh bone renders it liable to receive the whole weight of the body in an unfavorable direction; hence its liability to fracture; and the right angle which the bone assumes in old age, renders the patient now liable, especially in the female, to the production of this accident. Fracture of the neck of the thigh bone will occasionally occur with so little violence, that the fibrous reflection of the capsular ligament that is over the neck of the bone may escape laceration, and may serve to hold the fractured portions in contact. Should this happen, the free portion of the capsular ligament may remain whole, and prevent any very considerable retraction or displacement of the bones. Under other circumstances, the power of the force may be sufficient, not only to break the neck of the thigh bone, to lacerate the capsular and round ligaments, but also to drive the shaft of the femur high among the muscles upon the dorsum of the ilium.

The force may be so applied as to cause fracture of the trochanter major, or it may so influence the bone as to produce a separation of the epiphisis of this structure, when the accident happens in young people. Again: at a similar age we may occasionally find that a separation has occurred through the cartilage which unites the head of the bone to the shaft of the femur. This variety of accident is, for the most part, the result of indirect force; while that which we have just named is commonly dependent upon direct force.

The just appreciation and accurate diagnosis of this great variety in the character and degree of fracture of the neck of the thigh bone, must plainly require a correct knowledge of the structure and functions of the part—a due appreciation of the influence which the action of the various muscles impose upon the fractured portions—and a true comprehension of the nature of the operating force; these will, if duly studied, serve to explain the complexity of the symptoms, and the diversity of fracture which the neck of the thigh bone has experienced in each individual case. It will also show the reason of the great difference of opinion that has been held by surgeons from time immemorial, with regard to the diagnostic symptoms which indicate fracture of the neck of the thigh bone. Such knowledge of the anatomical structure of the part, will also point out to us the reason that union of the fractured bones may, in some cases, be accomplished, although never without a certain degree of deformity, and why, in the great majority of such accidents, we can seldom anticipate so favorable a result.

Having, in the distinctions we have drawn between fracture and dislocation, plainly shown that the nature of the operating forces are certainly distinct, each invariably producing its peculiarity of effect: that indirect force causes displacement of the bone, and that direct force produces fracture of the thigh bone; so in the consideration of fracture, the peculiarity of the mode in which this direct force produces its influence may be shown to cause a diversity in the kind and character of the lesion. Thus commonly when the direction of this force is from without inwards, we shall find fracture of the neck of the thigh bone implicating more or less of the trochanters and upper extremity of the shaft of the bone, possibly quite without the capsular ligament; but when it is from above downwards, it most frequently has its influence upon the neck of the bone within the capsular ligament. It must be remembered, however, that these latter deductions are only comparative; for although they may lead to the just appreciation of the case, they may be complicated in their cause and effects.

Let us now consider the symptoms and influence of each variety of these accidents. When the operation of the force upon the hip has been direct, from without inwards, and sufficient to drive the neck of the thigh bone deep into the cancelli of the shaft, without breaking up the vitrious structure, and is there firmly fixed, it is called an impacted fracture. This kind of accident is rare, but that it will sometimes occur is certain, while the symptoms that indicate it are always obscure, and even liable to deceive the most experienced surgeon. Upon an examination of the injured part, we commonly find the hip greatly swelled and acutely painful; this we might anticipate from the direction of the force causing the injury—we feel the trochanter major apparently in its true position, moving in a circle upon the rotation of the limb, but evidently in a diminished radius; no crepitation can be observed, for the neck of the bone, although fractured, is still firmly attached to the shaft, and performs its ordinary movements, as far as the injury to the soft parts will permit. Upon admeasurement of the two limbs, the fractured one may be found slightly diminished in length, but still the toe preserves its normal position. It may be observed that should this accident occur in youth, when the neck of the bone is fully developed and obliquely placed, the difference between the extremities may be readily appreciated; but should it happen in old age, especially in the female, the variation in the length will not be readily appreciated, from the angle which now obtains between the neck of the femur and the shaft of that bone. This shortening of the limb is the only mark that indicates this variety of the fracture, and if taken with a due appreciation of the character of the force, will commonly lead to a correct diagnosis. Should the character of the above mentioned accident not be rightly understood, and powerful extension of the limb, or free movement be attempted in our investigations into its character, a certain amount of crepitus will, after a time, be perceptible upon the continued movements of the bones. We may have changed the condition of impaction, have freed the bones one from the other, but we have certainly not advantaged the patient; consequently, we should be careful in these investigations. When this condition has been accomplished, the shortening of the limb has been increased, by the muscles of the back of the hip drawing up the shaft of the thigh bone; and if the parts are only loosened, the neck of the bone will simply be placed at a right, or even an acute angle, or it may finally be drawn upon the dorsum of the ilium. Should it have so happened that the surgeon has failed during his examinations to discover the true nature of the accident, and shall have separated the firmly-wedged portions, and left his patient with the supposition that a considerable bruise was the only defect, he will in all probability be vastly surprised and confounded at his next visit to find very considerable shortening of the limb, and upon retraction and rotation he may discover a distinct crepitus; or perhaps some rival, not a wit more intelligent than himself, has now been called in, and plainly indicates the nature of the accident; he talks loudly of ignorance and want of professional skill, and endeavors to exalt himself upon his brother's inadvertency. Such a case would be a clear demonstration how careful we should be in pronouncing a hasty opinion in many of these varieties of accidents which implicate the hip-joint, while it plainly shows the absolute necessity that exists for careful and inductive study of these parts. In confirmation of these facts I once heard a celebrated surgeon and teacher in the parent land say "he hoped that he might not be called to see these "accidents of the hip-joint, when fracture had happened, "until a few days after the injury had occurred, as it was "often then most difficult to decide upon their nature."

Should the application of the variety of force previously alluded to cause so much injury to the vitrious structure of the shaft of the bone, that the neck is driven deep into the cancellated structure, but is not firmly wedged in that position; in this case the parts obtain a certain amount of movement one upon the other; hence, with the shortening of the limb, we have a distinct crepitus from the first; as the swelling of the soft parts diminish, the trochanter major is found to possess an inordinate latitude of motion—the shaft of the bone moves freely upon the neck, and may soon after the accident be retracted upon the hip. The injury properly comprehended at this time; by the employment of proper means, we may, without doubt, accomplish a union of the fractured parts; but even here under the most favorable circumstances, and with the very best position, will it not take place without some degree of shortening of the limb, that will give the patient a trifling halt in his gait, and some appearance of deformity. The length of the limb, in the first instance, is slightly diminished, from its deep insertion into the shaft of the bone, and secondly, from the increase in the angle which the bone forms with the neck: should this point be materially overreached, the glutei muscles will raise the shaft of the bone upon the dorsum of the ilium; then there will be considerable shortening, as will be presently shown.

When the injury has been somewhat more extensive the trochanter minor may, by the wedge-like influence of the neck of the thigh bone, be forced from its position and completely separated with the fractured neck from the shaft of the bone. The trochanter major and shaft of the femur are now more or less raised upon the dorsum of the ilium, by the action of the glutei muscles. Now, the muscles at the back of the hip, the Pyriformis, the gemelli, the obturator internus, and quadratus femoris, also the obturator externus, all serve to rotate the toe outwards, while the separation of the insertion of the psoas magnus and iliacus internus, renders nugatory the influence of these muscles upon the os femoris. Had not this portion of the accident been experienced as soon as the shaft of the bone was raised upon the dorsum of the ilium, we should have the thigh flexed upon the pelvis, but now, under these circumstances, the limb may be completely extended; and this symptom is in a considerable degree diagnostic of the positive nature of the accident. Upon minute examination of the injured part, we find (when the swelling consequent upon the condition will permit) the trochanter major to be considerably raised above its true position, and that it rolls freely under the hand when we use the foot as a lever; this, with a short, quick motion, evidently different from the circle it used to perform during the action of rotation, when the shaft of the thigh bone stood fairly out from the pelvis. In these cases, this facility of movement forms a clear mark that plainly distinguishes the accident from dislocation of the hip-joint. As we have before pointed out, the limb is considerably diminished in length; but upon our applying extension this shortening is easily overcome, the fractured bones are brought into apposition, a crepitus is perceived; but as soon as we cease our efforts at extension, the bone is again retracted by the powerful muscles which clothe the back of the hip, and the limb soon again becomes as short as it was before our examination.

ART. LVI.—*On some Compounds of Urea, and on a new method for the determination of Chloride of Sodium and of Urea in Urine.* By JUSTUS LIEBIG, M.D., Ph.D., F.R.S., M.R.I.A.; Professor of Chemistry in the University of Giessen; Knight of the Hessian Order, and of the Imperial Order of St. Anne; Member of the Royal Academy of Science of Stockholm; Corresponding Member of the Royal Academies of Science of Berlin and Munich, of the Imperial Academy of St. Petersburg, of the Royal Institution of Amsterdam, &c. &c. &c.

In the number of the Annals of Chemistry and Pharmacy for October 1851, I have mentioned a compound of urea with protoxide of mercury, which is obtained in the form of a white gelatinous precipitate when a solution of protochloride of mercury (corrosive sublimate) is poured into a solution of urea previously made alkaline by potassa.

Werther had already previously observed that urea forms, with protochloride of mercury, a compound, crystallizing in flat prisms of a pearly lustre, which yields, according to Piria, a white precipitate with potassa, resembling the amide of mercury, and exploding when heated.

I have obtained three compounds of urea with protoxide of mercury, one of which is formed in a direct manner, and was described by Dessaignes a few weeks after the publication of my notice quoted above; the other two are obtained by precipitating an alkaline solution of urea with corrosive sublimate or nitrate of protoxide of mercury.

A. Urea and protoxide of mercury $2HgO+U$.*

On adding to a warm solution of urea protoxide of mercury suspended in water, the first portions are, as described by Dessaignes, completely dissolved in the liquor; an excess of protoxide of mercury is gradually changed into a white or yellowish-white powder. When dried in vacuo the colour of this compound is slightly yellow; when heated in a tube whilst dry it is decomposed, without explosion; ammonia is given off, metallic mercury sublimes, and a yellow residue of mellon remains, which disappears only on ignition, when cyanogen gas is disengaged. In the moist state it decrepitates under the same circumstances; in the dark, sparks of a green light are observed.

By digesting protoxide of mercury in the water bath with a solution of urea, I have in no instance succeeded in obtaining this compound entirely free from cyanate of protoxide or suboxide of mercury; a feeble, but very distinct disen-

* U. *Urea.*

gagement of ammonia invariably takes place. The yellowish white powder formed dissolves in this case in hydrocyanic and hydrochloric acids, leaving a black residue of metallic mercury behind, and disengaging a small quantity of gas. The solution, when treated with milk of lime, gives off ammonia. This is the character of the cyanates. When digested in the water-bath for a longer period, the urea compound loses its yellowish-white color, and becomes lemon-coloured and granular. This latter compound exhibits the deportment of a basic cyanate of mercury. 2,000 grains dissolved in hydrocyanic acid gave, on evaporation, a dry residue of urea and cyanide of mercury weighing 2.394 grammes; from which were obtained, by precipitation with sulphuretted hydrogen, 1.745 of sulphide of mercury, and by evaporation of the filtrate 0.429 gramme of urea. This gives for

100 urea and protoxide mercury.	protoxide of mercury	81.09,
	urea	21.15.
		102.51

The increase in weight, amounting to upwards of 2½ per cent., appears to confirm the formula of Dessaignes, according to which 1 equivalent of water separates by the entrance of protoxide of mercury into this compound. The urea obtained by me was not, however, perfectly dry: it remained somewhat pasty, and possessed feebly the reactions of sulphocyanide of ammonium. I am therefore not quite sure about the fact of water separating from the urea, inasmuch as in the silver compound which contains 3 equiv: of oxide of silver, a similar replacement of water by oxide of silver does not occur. By the same process of digesting a solution, containing about 10 per cent. of urea with protoxide of mercury, until the reddish-yellow colour of the oxide was completely converted into a yellowish-white, I have in several instances obtained a urea compound containing 3 equiv. of protoxide instead of 2, just as the compound to be described forthwith.

c. Urea and protoxide of mercury $3HgO+U$.

On adding solution of potassa to a solution of urea, and mixing with it a solution of corrosive sublimate, the liquid being continually kept alkaline by renewed addition of solution of potassa, a thick gelatinous snow-white precipitate is obtained, which when brought into boiling water while still moist, after being first completely washed, is converted into a sandy granular powder of yellow or yellowish-white colour. The water is thereby rendered alkaline, and takes up a certain amount of urea. When dried the powder is reddish-yellow; on being heated in a narrow glass tube it is decomposed with decrepitation, and when moist, frequently with explosion; in the dark the substance becomes luminous during this decomposition, and beautiful green sparks are observed; water and carbonate of ammonia are hereby given off, and metallic mercury sublimes, mostly without any residue of mellon. The compound dissolves in hydrocyanic and hydrochloric acids without effervescence; alkalies produce in the latter solution a yellowish-white precipitate.

The analysis of this compound from different preparations yielded the following results:—

I. 4.606 grammes* of substance, dried in vacuo over sulphuric acid, gave 4.152 of sulphide of mercury.
II. 2.685 " " " " gave 2.436 do.
III. 2.1904 " " " " " 1.9605 "
IV. 1.7578 " " " " " 1.5815 "
V. 1.000 " " by combustion with oxide of copper, gave 0.1144 of CO^2 and 0.0594 of water.
VI. 2.094 " yielded by direct determination 0.350 grammes of urea.

Composition in 100 parts:

	Calculated		I.	II.	III.	IV.	V.	VI.
Protoxide mercury,	324.	84.37	84.1	84.3	83.91	84	—	—
Urea	60.	15.63					15.6	16.6
	384.	100.00						

C. Urea and protoxide of mercury, 4 HgO+U.

On precipitating a solution of nitrate of protoxide of mercury, instead of a solution of corrosive sublimate, with an alkaline solution of urea, a white, somewhat less, gelatinous precipitate is obtained, which also loses this state in boiling water, and becomes a sandy powder. No distinct crystalline structure was observed under the microscope in either of these compounds.

The properties of this compound do not differ from those of the other previously described: it contains, however, more protoxide of mercury. The substance from different preparations, dried over sulphuric acid, yielded by analysis the following results:—

I. 0.987 grammes gave 0.932=94.4 per cent. of sulphide of mercury.
II. 2.200 " " 2.085=95.3 " " "
and 0.279=12.7 per cent. urea.
III. 3.000 " " 2.860=95.3 " " "
and 0.239=10.96 per cent. urea.
IV. 2.000 " " 1.880=94. " " "

These numbers correspond to the following composition:—

		Calculated		Found			
				I.	II.	III.	IV.
4 equal protoxide mercury,	432	87.804		87.82	87.3	88.72	87.4
1 ... urea	60	12.195		——	12.7	10.96	—
	492	100.000			100.0		

* Vide page 289, for the reduction of the gramme into English grains.

Urea and Protoxide of Silver.—When freshly precipitated, oxide of silver in the moist state is put into a solution of urea, and the liquid left in a warm place of from 40° to 50°; the oxide of silver changes its colour after one or two hours, appears to swell up from one point, and becomes granular and of a lighter grey colour; when the mass has assumed a uniform colour, and some of it is examined under the microscope, the powder is found to consist of transparent, pretty regular, scarcely coloured, prismatic crystals. The compound is readily soluble in nitric acid without disengagement of gas, but with difficulty in ammonia. When touched at one point with an ignited body, it undergoes a tinderlike combustion, a large quantity of ammonia being disengaged, and is converted into a darker-colored coherent mass, which now effervesces with acids and evolves with nitric acid, besides Co^2 nitric oxide or nitrous acid. On heating the burned mass in a tube, a strong smell of cyanic acid is perceived, and a white body sublimes possessing the properties of cyamelide. The urea compound is converted by the first combustion into a mixture of metallic silver and cyanate of the suboxide of silver. On being heated in a tube, a second incandescence takes place; the cyanate of suboxide of silver is decomposed, part of the cyanogen escapes in the form of cyanic acid, and another portion remains with the silver;—in fact, when the residue is boiled with dilute nitric acid, a mixture of white cyanide of silver and brown paracyanide of silver is left behind.

By analyzing the compound from different preparations there was obtained—

I.	From 2.0340 grammes of urea, oxide of silver dried in vacuo over sulphuric acid, 0.2960 grammes of pure urea=		14.25 per cent.
II.	From 1.052 of the compound,		0.826 of metallic silver.
III.	" 1.585	"	1.248 " "
IV.	" 1.547	"	1.222 " "
V.	" 1.758	"	1.385 " "
VI.	" 1.939	"	1.529 " "
VII.	" 1.574	"	1.242 " "

These results lead to the following composition:—

	Calculated		I.	II.	III.	IV.	V.	VI.	VII.
3 eq. ox. silver,	348	85.29	——	84.32	84.54	84.81	83.50	84.68	84.7
1 ... urea......	60	14.71	14.25	——	——	——	——	——	——
	408	100.00							

Nitrate of Urea and Protoxide of Mercury.—On adding a solution of nitrate of protoxide of mercury to a solution of urea, a snow-white flocculent precipitate is immediately formed, containing urea, protoxide of mercury, and nitric acid. According to the proportion in which the two solutions are mixed, or according to the amount of acid in the mercury salt, three compounds or mixtures of them are formed, differing by their amount of protoxide of mercury. These various compounds possess the following properties in common:—They yield, when burned with oxide of copper, a mixture of gases containing nitrogen and Co^2 in the proportion of 3 to 2 volumes; this is in the same proportion as that in nitrate of urea. On removing the protoxide of mercury, by means of sulphuretted hydrogen, pure nitrate of urea remains in the liquor filtered off, which crystallizes to the last drop. These compounds differ, therefore, only by their unequal amount of protoxide of mercury; they dissolve in hydrocyanic acid and hot nitric without leaving a residue. Potassa produces a white precipitate in the nitric solution. Heated for some time, when dry, in a current of warm air, decomposition ensues; they assume a yellowish color, and their nitric solution yields now a yellowish precipitate with potassa.

A. NO_5U+HgO. When the solution of urea is mixed with the mercurial solution in a *very dilute* and warm state, and the precipitate formed is digested with the liquor, it quickly aggregates to a heavy white powder, which presents under the microscope the form of roundish grains, consisting of very small concentrically grouped needles. In the dry state this powder rolls on paper like fine sand. After being dried in vacuo there was obtained, by solution in hydrocyanic acid and precipitated with sulphuretted hydrogen, from—

I. 1.990 grms. of the substance 1.680 sulphide of mercury=88.4 per cent.
II. 2.000 " 1.700 Hg S.=85 per cent.
III. 2.000 gave, after the removal of the mercury, 0.410 of nitrate of urea, and after its saturation with carbonate of baryta 0 681 nitrate of baryta and urea. *On treating this* 0.681 *grammes with alcohol*, 0.462 of nitrate of baryta was left. This compound, accordingly, consists of—

	Calculated		I.	II.	III.
1 eq. nitric..................	54	9.90	——	——	9.55
1 ... urea	60	10.98	——	——	10.95
4 ... protox. merc.	432	79.12	78.58	79.14	——
	546	100.00			

B. NO^5, $U+2HgO$. On adding a solution of crystallized nitrate of urea to a moderately dilute solution of nitrate of protoxide of mercury, mixed with some nitric acid, until a slight cloudiness ensues which does not disappear again, then filtering and allowing the mixture to stand quietly, solid hard crystalline crusts are deposited over night, consisting of an aggregate of small, rectangular, brilliant, transparent tablets. By treatment with boiling water these crystals are decomposed; they become dim and opaque, and are transformed into the compound just described, the water removing from them nitrate of urea. The analysis yielded the following results:—

I. 2.400 grammes gave 1.696 Hg S.
II. 2.000 " 1.414 "
III. 2.000 " 0.700 gramme nitrate of urea, from which were obtained 0.780 nitrate of baryta.

This compound, therefore, consists of—

	Calculated		I.	II.	III.
1 eq. nitric..................	54	16.37	——	——	16.1
1 ... urea	60	18.18	——	——	18.9
2 ... protox. merc.	216	65.45	65.6	65.7	——
	330	100.00			

C. $NO^5U+3HgO$. On adding a dilute solution of nitrate of protoxide of mercury to a solution of urea, as long as a precipitate is formed, and allowing the white mass to stand in a warm place of from 40° to 50°, the precipitate is converted into six-sided transparent tablets, amongst which are observed under the microscope roundish grains of the first and isolated square tablets of the second compound. In no instance have I succeeded in obtaining this compound quite pure and free from those admixtures: the microscope, however, plainly indicates that we have before us a compound totally different from the other two.

2.000 grammes gave 1.550 Hg S. 0.864 nitrate of baryta and urea, and from this 0.597 nitrate of baryta; 3.000 grammes gave 2.488 Hg S. 1.256 nitrate of baryta and urea, and 0.836 nitrate of baryta.

The first analysis yields for 100 substance 77.5 Hg S.; the second 80.3. Assuming the compound to contain 3 eq. protoxide of mercury for 1 eq. nitrate of urea, 79.4 per cent. of Hg S. should be obtained from it when in a pure state.

Quantitative Determination of Chlorine in Neutral Liquids by Means of Protoxide of Mercury.—Nitrate of protoxide of mercury added to a solution of urea immediately produces a thick white precipitate; this precipitate does not take place with a solution of corrosive sublimate. On mixing a chlorine compound of the alkali metals with nitrate of the protoxide of mercury, a mutual transposition into sublimate and nitrate of the alkaline base takes place. A saturated solution of chloride of sodium, mixed with a concentrated solution of nitrate of protoxide of mercury, solidifies into a foliated mass of crystals of protochloride of mercury.

On mixing a solution of urea with chloride of sodium, and adding gradually in small portions a dilute solution of nitrate of protoxide of mercury, a white cloudiness ensues at the place where both liquids meet, disappearing, however, immediately on shaking, leaving the liquor as bright and transparent as before; without the chloride of sodium it would have retained its cloudiness. This deportment lasts until the nitrate of protoxide added exactly suffices to transform the chloride of sodium into corrosive sublimate; beyond this limit a single drop of the mercurial salt produces a lasting white cloudiness.

From this deportment it is evident that, if we know the amount of mercury in the solution of nitrate of protoxide of mercury which has been added to a solution of urea, containing an unknown quantity of chloride of sodium, until a permanent precipitate is formed, we also know the amount of chlorine or of chloride of sodium in this solution. One eq. of mercury in the mercurial solution used corresponds exactly to one equiv. of chlorine (or chloride of sodium). If, on the contrary, the amount of chloride of sodium in the solution of urea be known, and the amount of mercury in the mercurial solution be unknown, the latter amount may be calculated with facility. This method of determining the amount of chloride of sodium is particularly applicable in the case of urine, inasmuch as no urea requires to be added. As a matter of course, it may also be used with advantage for the estimation of the chlorine in brine or sea-water, and, generally speaking, in every case where a large number of such determinations are to be made in the shortest possible time. In those cases in which the chloride of sodium is not to be estimated in urine, but in other neutral liquids, the method now to be described has to be modified in some points.

Preparation of the Nitrate of Protoxide of Mercury.—If chemically pure mercury be at our disposal, this preparation does not offer any difficulty. 5 parts of nitric acid of 1.425 sp. gr. are poured on one part of mercury in a beaker; this is then placed over a water-bath, and heated with frequent additions of a few drops of nitric acid, until no more red vapours escape, and a drop of it mixed with a solution of chloride of sodium ceases to produce a cloudiness; the solution is then evaporated in the same vessel, and over the water-bath, to the consistence of a syrup. The mercury of commerce cannot be used for this purpose, because it always contains lead and bismuth, which render the determination of chlorine uncertain. The presence of lead or bismuth immediately produces a white cloudiness, or an opalescence, on mixing the mercurial solution with a solution of chloride of sodium containing urea, which renders it difficult to recognize clearly the limit or the point when the urea compound of the nitrate of protoxide of mercury is precipitated. It becomes, therefore, necessary first to obtain a crystallization of nitrate of suboxide of mercury by boiling dilute NO_5 with an excess of metallic mercury, concentrating and cooling. The crystals of this salt are then separated from the mother-liquor containing the other metals, washed first with some dilute NO_5, and then with water, whereby part of the salt may be transformed into a basic salt; they are next dissolved in NO_5, and heated until no further escape of NO_5 takes place, and a drop of it tested by chloride of sodium is no longer precipitated. The solution of the salt of the protoxide is now evaporated in a water-bath to the consistence of a syrup, and diluted with ten times its bulk of water. Should there be, after twenty-four hours, a separation of basic salt of the protoxide from the mixture, it must be filtered off.

In order to employ this mercurial solution for the determination of chloride of sodium, it must be graduated—that is to say, reduced to a definite amount of protoxide. This may be effected in two ways: it is either graduated in a direct manner with a solution of pure chloride of sodium of a definite strength; or the amount of protoxide of mercury is estimated, and the solution then diluted with such a quantity of water that a cubic centimeter of the dilute solution indicates exactly 10 milligrammes of chloride of sodium. Both methods require a solution of chloride of sodium of a definite strength.

When pure transparent rock salt, in coarse pieces, is digested with water during twenty-four hours at a temperature of 12° to 24°, and the liquor frequently shaken during that time, an unvarying quantity of the salt is dissolved. 10 cubic centimeters of the clear filtered solution contain a definite, invariable weight of chloride of sodium; a determination by the balance is hereby not required. 100 parts of water dissolve, according to the experiments of Fuchs, 36; according to the latest investigations of Fehling, 35.91; and 10 parts by weight of the solution contain 2.6423 of chloride of sodium. The specific gramme, according to Karsten, of the solution is 1.2046; according to Anthon, 1.205: 10 cubic centimeters of a saturated solution should consequently contain 3.183 grammes of chloride of sodium.

By evaporating 10 cubic centimeters of saturated solution of chloride of sodium, I have obtained 3.185, 3.184, 3.195, 3.175; mean, 3.184 grammes of chloride of sodium. This is the number obtained by Fuchs and Fehling. If, therefore, 20 cubic centimeters of chloride of sodium, saturated at the ordinary temperature, be exactly measured by means of a pipette (the drop adhering to the end of the pipette not blown off), and mixed with 298.4 cubic centimeters of water, we have

298.4 cubic centimeters of water } 318.4 cubic centimeters.
20.0 " " solution Cl. Na. }

of dilute solution of chloride of sodium, containing 2×3184 milligrammes of this salt. 10 cubic centimeters of this solution contain, therefore, 200 milligrammes of chloride of sodium.

(To be continued.)

BOOKS RECEIVED FOR REVIEW.

Amputation of the entire Lower Jaw, with disarticulation of both Condyles. By J. M. Carnochan, M. D., Professor of the principles and operations of Surgery in the New York Medical College, Chief Surgeon to the New York Emigrant's Hospital, &c. &c., *with plates.* New York: Van Norden & Amerman, Printers, No. 60 William Street, 1852.

REVIEW.

ON RHEUMATISM, RHEUMATIC GOUT, AND SCIATICA; *their Pathology, Symptoms, and Treatment.*—By Henry William Fuller, M. D., Cantab: *Fellow of the Royal College of Physicians, London: Assistant Physician to St. George's Hospital, &c., &c. New York: Samuel S. and William Wood,* 261 *Pearl Street,* 1854. *Toronto: H. Rowsell.*

Concluded.

One of the most severe varieties of acute Rheumatism is denominated rheumatic fever; in these cases the amount of constitutional irritation which accompanies, nay, often precedes the articular affection, is most marked. The high tone of the system, and the profusion of the poison are, in all probability, the true causes. In such cases, the excitement of the sanguinous system is extreme—inflammation of the blood, as it was falsely termed by the old practitioners, exists; general exaltation of the functions evinces itself, especially in the nervous and the muscular; such also is the case among the excretions, but this is soon ended by oppression and delayed action, if we except the skin, which is often abnormally excited. A point, however, of marked importance in this condition of poisoning by lactic acid, is the extreme irritation of the lining membrane of the heart and arteries, which eventually produces positive disease, and inaptness of function that issues in palpitation, dyspnœ and dropsy. We apprehend that the mode in which these diseases of the heart and arteries are developed has not been indicated with that degree of plainness of which they are susceptible, and merit a lengthened consideration at our hands; to comprehend them fully, we must revert to the anatomical structure of the parts. We have already shown that the circulating system of bloodvessels, of which the cavities of the two hearts plainly constitute a part, may be compared to a shut sac, which, at the same time that it supplies the nutritive apparatus of each structure, with pabulum for the fulfilment of its growth, or the performance of its functions, only accomplishes this supply by transudation through its coats. The whole of these structures are lined like other fibrous sacs, with a serous membrane. This serous membrane consists of a fibrous basement membrane, which has distributed upon its free surface an abundance of pavement epithelium, intended as an organic defence to that structure, besides which it possesses in the subserous tissue a system of minute capillary vessels, which are intended to supply these parts with nourishment; it has been said that these pavement epithelium obtain their supply of nourishment from the blood itself; but we apprehend that the laws of endosmodic action plainly disprove the possibility of this fact; for the blood being of denser character than the fluid which transudes the cell wall of the epithelium, would rather serve to abstract fluid, than yield nourishment to these structures. Besides this serous membrane, the circulating apparatus is supplied with a muscular structure, whose fibres are, for the most part, both longitudinal and circular. In the heart this structure is very largely developed, less in the arterial tubes, and on the capillaries and veins most indistinctly to be demonstrated. The muscular fibres of the heart have been shown to be in a transitional condition, not the perfect striated muscle, while they are in a far higher grade of development than the non-striated variety of the vascular tunics. The yellow fibrous tissue of the arteries is more strongly developed, than the same substance in the veins or capillaries; while the third or external fibrous tunic bears a similar proportion. We have, then, this vascular apparatus, consisting of the following structures, doubtless evincing various grades of development, but all referrible to corresponding elements. We have the serous lining membrane which consists of three principal ingredients—the organised pavement epithelium, the inorganic or lowly organised basement membrane, and the nutritive capillaries, and these exist in the heart as well as in the vessels. In the second coat we have the non-striated muscular apparatus, both circular and longitudinal fibres. As a necessary consequence of the existence of this kind of structure, we find a capillary nutritive apparatus, and a distribution of nervous filaments—added to these we have the yellow fibrous elastic coat which constitutes the main bulk of the arteries, which in the veins and capillaries is particularly diminished, and in the heart itself is spread out and displaced. The outer and third coat of this system of tubes is formed from the white fibrous tissue, partially developed in the capillaries and veins, more marked in the arteries, and extremely prominent around the heart; when, as in other parts of the body, this substance is present, we find that constant motion developes a shut sac—a bursæ mucosæ—a serous apparatus—this constitutes the pericardium. From this marked uniformity of character in the various structures, a great correspondence exists in their diseases—while these vastly similate the affections of like structures in other parts of the body, as in most cases are clearly demonstrated by their anatomical peculiarities.

Let us assume, for example, that an abnormal quantity of lactic acid is the poison that abounds in the circulating system. It is, for the most part, apparently retained in the shut sac, the circulating apparatus. It is, in some degree, natural to the muscular structure—it does not appear to act very potently upon the nervous system: hence, we would conclude that the poison does not enter into the nervous apparatus; but as the capillary system supplies all these parts with blood, containing a varying amount of this poison, this system feels the influence, and is excited by it to increased action, as is shown by the rheumatic pulse. Hence, we shall have an exalted function of every organ as the result, and this is demonstrated as rheumatic fever. In this case the peculiar poison would appear especially to irritate the lining membrane of the heart and arteries from immediate contact. It induces irritation of the muscular apparatus, caused, in the first place, by exalted nervous influence, and succeeded by increased vascular activity of the capillaries of the muscular apparatus of the heart and arteries. When continued for any length of time, a hyperœmic condition of the nutritive apparatus is the necessary consequence. Hence is laid the foundation for disease in this system of vessels, which pervades the whole body. As we have before said, this condition is readily recognized by the full bounding pulse and general excitement of the whole frame when lactic acid abounds in the blood-vessels; so that, if from any local cause at this period of the disease the operation of cold, the result of an injury or any other debilitating influence, the powers of the part or organ are depressed, the local fibrous structures then suffer especially from the irritation of the poison, and acute rheumatism is developed. We may observe in this case, the poison is still operating upon the capillary vessels, for such in reality are the minute vessels that pervade the ligament; for these, although vastly diminished by fibrinous development, still they have not been occluded, and can admit an increased amount of blood, and this lactic acid contained within it. Such, possibly, is the explanation of the proneness of this and some other poisons to attack the fibrous structures, the calibre of whose vascular apparatus is of less diameter than the smallest capillary vessel.

Physiology and pathology both teach us that the continuance of the exalted function of the muscular coats of the heart and arteries, produced by the stimulus or irritation of the poison in the blood, cannot be continued for any great length of time, without producing capillary hyperœmia in the vascular system, and after a time that this condition must eventuate in debilitated function, or inflammatory action. Should debilitated muscular power be the result, dilitation of the fibrous structures will be apt to supervene. This will be produced by the powerful action of the heart, to which the fibrous structure now yields—hence, we have the first impulse of true aneurism in many of these cases. This condition is greatly facilitated, in consequence of the softening of the fibrous tissues which result from the increased supply of serous fluid presented to it as the result of the hyperœmic condition of the capillary vessels of this apparatus. The usual nourishment supplied to the fibrous element appears to be a slight amount of serous fluid that transudes the capillary vessel and preserves to the structure a certain amount of moisture and flexibility; when the natural amount is abnormally increased, softening and loss of tone is the result—and, facilitated by the loss of muscular power, permits the influence of the heart's action to have greater effect upon the vessel, and more readily to lay the foundation of true aneurism. The next influence in the onward progress of this condition is the sympathy with which the capillary apparatus of the serous lining membrane of the artery is now endowed; a softening of the membrane results from the increased supply of serous fluid to the basement membrane, and increased supply of nourishment to the pavement epithelium; that these become softened and swelled, and instead of the beautiful, smooth surface, we find it uneven and roughened; nay, these structures are not unfrequently cast, leaving the inorganic basement membrane more or less bare. When such is the case we have a friction sound presents itself; as in other serous membranes, we have a bruit de soufflet or bruit de diable, as it is often called. This sound is caused by the friction of the blood corpuscles coursing along the elevated irregular, and often broken-up pavement epithelial cells, or on the rougher surface of the basement membrane. Under these circumstances, this bruit de soufflet is a pretty certain evidence of some of the stages of inflammatory action in this serous membrane. It is said that this sound should be looked upon rather as a proof of the anœmic condition of the blood; when the blood is particularly deficient in fibrine, albumen and the red corpuscles; when it abounds in serum, and the blood is much thinner in consistency, then this condition may influence the epithelial cells, and may produce a condition similar in their structure to the one we have explained as resulting from the impulse of inflammatory action in their own capillaries; the cells swell, are enlarged, may be broken up and disarranged from this cause—at all events the surface is rendered uneven, and the friction sound is produced by the action of the blood corpuscles upon this uneven *surface*, while now, in consequence of the attenuated condition of the blood, these structures have a more considerably increased influence, and produce far greater friction; hence the two conditions go to produce similar effects, showing that the bruit de soufflet may result from an anœmic condition of the blood influencing this serous lining of the arteries, or from general or local inflammatory action in the nutritive structures of the part.

We have pointed out that disease of the muscular structure of the artery may show itself in exalted action, as is witnessed by the full and bounding pulse of rheumatic fever; or it may evince itself in diminished power, that assists in the formation of aneurism. The hyperœmic condition of the capillary vessels that form the nutritive apparatus of the muscular structure evinces changes that occur as the result of this condition, and make it certain that such exists; should this abnormal congestion continue for a considerable period, atrophy of the fibrillæ and degeneration of the muscular structure will happen, muscle will be replaced by fat; hence the atheromatous developments that continually present themselves in the diseased lining membrane of the arteries. When this condition has existed for a considerable period, crystals of chloresterine and globules of olein will be observed, while, after an indefinite period of time, these fats may be replaced by earthy salts, when we have what is called ossification of the artery. This condition of the non-striated muscular fibre may be observed in the heart, and may be found in its openings and upon its valves. We have observed these diseases of the muscular structure to be present to a very considerable extent, while it is still covered with the serous membrane. It often happens, however, that the epithelium are shed, the basement membrane exposed, and even ulcerated and removed; then there is a tendency to the collection of fibrine opposite to the part—still, however, the impetuous current of the arterial circulation constantly sweeps away any particles that might chance to be arrested. We find this fibrine constantly presenting itself in layers. It however demands great dilitation of the vessel and delayed vascular action, to permit the fibrine to coagulate and accumulate in the neighbourhood of the diseased arterial coats to any great extent; should any projection or very great dilation evince itself, then the current is delayed, coagulated fibrine speedily accumulates in the part, and may represent one of the efforts of nature to cure the disease. Again: we can understand that in some cases of partial disease of the serous membrane, the absence of epithelium, and the consequent temporary formation and delay of coagulated fibrine may happen; that the course of the circulating blood may drive such portions onwards towards the capillaries, and that it may be arrested in the blood vessels, giving rise to impeded circulation in the part to which the vessel is distributed, a circumstance that not unfrequently results in softening of the brain, when such an accident happens in its nutritive apparatus.

These are some of the consequences of a poisoned condition of the blood. Such influences will manifest themselves in all the varieties which occur; but the peculiar influences here represented appear to bear more expressly upon the condition of poisoning by lactic acid. The presence of urate of soda in blood does not seem to produce the same intensity of action, either constitutional or local, as the lactic acid; it will, however, prove a more permanent source of irritation to the fibrous structures, but does not excite the same intense action in the heart and arteries as lactic acid. When the presence of oxylate of lime is manifest—and it is often a source of irritation in some of the diseases which Dr. Fuller has so admirably described—it would appear to shed its influence more upon the fibrous tissues which constitute the fasciæ, membranes, and theca of nerves, than on the ligamentous structures of the joints; when, however, these last become affected, we fancy that there is a greater tendency to run into the formation of matter around the ligament, than when the urate of soda is the exciting cause of inflammatory action in these tissues.

In drawing the attention of our readers to some of the effects of the blood poisoning by lactic acid upon the circulating system, we have purposely maintained silence with regard to its influence upon the heart itself; the points which we have ventured to lay most stress upon are those which appear in some degree to explain the very practical views of our author, and which seem naturally to lead us to them; consequently, we must consider our attempted explanations comparatively useless, unless they are followed up by the study of the lucid descriptions given by Dr. Fuller of Pericarditis and Endocarditis. We would sincerely recommend all who are desirous of understanding the most improved and recent ideas upon rheumatism, and the consequences to which it gives rise, to consult the work in question. We fear that we have already ventured too far in this most interesting subject—that we have exhausted the patience of our reader; but we cannot terminate the consideration of this matter without expressing our conviction, that this is by far the most practical and useful treatise that has lately issued from the press on the subject of rheumatism, and we can commend the labors of Dr. Fuller to the attentive perusal of our readers.

EDITORIAL DEPARTMENT.

PROFESSOR LIEBIG'S PAPER.

We have great pleasure in presenting to our readers a most important paper by the celebrated chemist, Justus Liebig, "on some compounds of urea, and on a new method for the determination of chloride of sodium and urea in the urine." The article has already been published in the *Quarterly Journal of Chemical Science*, but has not been printed upon the Continent of America. The ready determination of the quantity of urea in urine has long been a desideratum with the practical physician, and is a point of such vast importance in the consideration of disease, that we would desire to give the subject a most conspicuous position in our pages: trusting that a just appreciation of the facts and the facile employment of the means offered as data in the investigation of disease, will be appreciated by every medical practitioner. The process advocated by the celebrated chemist is founded upon the fact, that urea forms with four equivalents of the pernitrate of mercury an insoluble compound; if then we add to urine a solution of this substance of known strength, we can precipitate and determine the amount of urea with the greatest accuracy.

We need not here stop to point out the importance of this fact, that shall enable the physician to demonstrate with exceeding accuracy the positive amount of urea in urine, and that with the greatest celerity; for when the method here presented to our observation shall have become familiar to the practical physician, it will be as necessary in his every-day analysis of disease as the stethescope or microscope. It is really curious to see how slowly the practical physician is inclined to adopt the advantages which the improvements in science offer as aid and assistance in the investigation of disease. We maintain that precision in the analysis of disease cannot be obtained without the assistance of such means, and we would advise every practical man to gain a knowledge of their use, and habituate himself to their employment. Heretofore it has been the habit to guess at disease, to trust to personal experience in the case; but ere long, we believe, that with the light of science we shall be able readily to interpret all the symptoms of disease appertaining to any particular complaint, with an accuracy and despatch unknown to the old school. We must confess that we should like to see the medical officers of the Toronto General Hospital set a good example on this matter; they have a noble opportunity of testing the advantages of these several inductive processes, if they would set about it with spirit and energy. Some twenty years ago, we full well remember how the *old practical men* in the profession—as they were called—used to sneer at the stethescope; they used to ridicule the idea that it could be of the least service in the investigation of disease; at the present time the perfect accuracy in the diagnosis of chest diseases by means of the ear, is only equalled by occular demonstration. The sure advantage of the data offered by the stethescope is now universally acknowledged, and we would ask why will not the profession generally make use of similar assistance offered by the *microscope* and *organic chemistry?* No hospital that lays any claim to a consistent progress in medical science should be without such aids. These are points of especial employment in all the hospitals of Europe and America, that endeavour to keep pace with general advancement of science; and we would ask why the Toronto General Hospital should be behind in these matters? Persons at a distance would imagine, from the known liberal antecedents of the present hospital trustees, that it would cause them to stimulate and encourage their medical officers, in the fullest developments of medical progress, and lend them every aid and assistance necessary for the work. A microscope is an absolute necessity in the investigation of disease in the present day, and it should form part of the hospital establishment; it is a wonder and amazement to us how the medical officers can possibly get on without one. We sincerely trust that when the noble hospital now building shall have been finished, that the trustees will afford the required facilities for the use of such necessary investigations; and that they will even appoint an assistant medical officer, whose duty should be the especial attention to such matters. It has been urged that the acknowledged want of unanimity among the medical officers is a bar to such improvements: we should rather look upon the matter in a different light—an individual having the love of science, and the advancement of the medical profession in view, if properly supported, would rather be a stimulus to union, an encouragement to the belligerents to forget their personal and political differences in the love and prosecution of that science, which they are all bound to honor and promote to the best of their abilities. It has been said that the medical officers at the Toronto General Hospital get nothing for their services, and cannot be expected to waste their time and spend their means in the pursuit of such matters: we would maintain that the position they occupy, if it be used in a proper manner, would more than compensate for all their trouble. A medical man's progress must be onwards, and here he might find a noble field open to him for the study of disease, in which he could prove the truth of his deductions and confirm his knowledge. In the attendance upon the hospital, we acknowledge that the division of labor would be very desirable, and if the matter was carried out with unanimity, would greatly conduce to the benefit of all concerned. We confess we cannot see why the patients cannot have the full benefit of all the recent improvements in medical and surgical science, and the students gain the full advantage in their studies of the most recent and approved methods of scientific induction in the investigation of disease. The Toronto General Hospital should not be behind in this matter; we would say, let party and politics be forgotten, let all join in a fervent desire to advance medical science, then, and only then will the medical officers be fully respected, and the Toronto General Hospital gain that standing in popular estimation, and public usefulness, which its metropolitan position, and great advantages confer upon it.

TORONTO GENERAL HOSPITAL.

With feelings of profound grief, we lay before the Medical Profession the following correspondence; it it is another terrible demonstration of that despicable antagonism and desperate combination of parties, that has long been the curse of the Medical Professsion in the City of Toronto. It is to a great extent, the source of the humiliating position and degraded condition of the Medical Profession in Canada West, which nothing but an act of incorporation can correct. We have every reason to believe that the determined manner in which we have advocated this measure has caused our present persecution; we can solemnly declare that the common good and advantage of the Medical Profession has been our only aim, consequently we can afford to despise such attacks, and shall cheerfully suffer persecution if it shall but tend in the slightest degree to improve or advantage the condition of the Profession.

We present the correspondence in question as it appeared in the *North American* newspaper of this city.

To the Editor of the North American:

Sir,—Having a strong desire to maintain my knowledge and improve in the practice of my profession, I wished to attend the practice of the Toronto General Hospital in this city; consequently I offered the usual fees demanded by the hospital trust of students attending that institution; when I was informed that I had a right, and was at perfect liberty to attend the hospital under the printed bye-laws. rule the 4th, section the 7th, which states "That all Licensed Practitioners may walk the wards in company with the visiting medical officers at the usual hour, and may attend all operations; but may not dictate or interfere in the practice." I consequently made a rule of attending the hospital at the usual hours, invariably showed the greatest respect to the medical officers, and am perfectly certain that I never infringed any of the hospital rules.

Suddenly, and to my surprise, I received the following order from the Board of Hospital Trustees:—

(Copy) "Toronto Hospital,
May 20th, 1853.

"Dear Sir,—I have received a letter from the Secretary to the Hospital Trustees requesting me to inform you that by order of the Board in consequence of your interference with the Medical officers of the Hospital, your admission under the 7th clause of the 4th Rule of the printed Laws of the institution shall cease.

"I am, dear Sir,
Your obedient servant,
E. CLARKE, R. S.

"S. J. Stratford, Esq.,
Surgeon, &c."

On which I made an application for the charges laid against me, in the following letter addressed to the chairman of the Board:—

(Copy) "Yonge Street,
DEAR SIR: Toronto, May 20, 1854.

"I having received a letter from Dr. Clarke, House Surgeon to the Toronto General Hospital (a copy of which I enclose to you) to the effect that I am ordered to cease my visits to that institution, in consequence of interference with the Medical officers, will you permit me to ask you for a statement of the charges which have been made against me, and which induced such an extraordinary decision, as I deny having in the slightest degree infringed the rules of the Hospital, and can prove that I have always treated the Medical officers of that institution with the most marked respect.

"I have the honor to be, dear Sir,
Your obedient servant,
S. J. STRATFORD.

"The Hon. C. Widmer,
Chairman of the H. T. B."

This application was met with the following rejoinder:—

(Copy) "Office Toronto General Hospital,
May 25th, 1854.

"Sir,—I am instructed by the Board of Trustees to acknowledge the receipt of your letter to the Chairman, dated 22nd instant, and in reply to inform you that the decision of the Board, as communicated to you through the Resident Surgeon, is grounded on a full corroboration of the evidence adduced.

"I am
Your obedient servant,
J. BRENT,
Secretary.

"S. J. Stratford, Esq.,
Surgeon."

I then addressed a second letter to the Board to the following effect:—

(Copy) "Yonge Street,
Toronto, May 25, 1854.

"Sir,—In a letter I addressed to the Chairman of the Board of the Hospital Trustees I solicited a statement of the charges made against me that could have induced the Board to come to so unjust and hasty a conclusion as to deprive me unceremoniously of a right accorded by their printed regulations (Rule 4th sec. 7th) to all Licensed Medical Practitioners. In answer you say, that you are directed by the Board to inform me that the decision is grounded on the full corroboration of the evidence adduced. Will you, a *second time*, permit me to solicit a copy of the *charges*, and I will now add of the *evidence adduced* also; as I emphatically deny having transgressed the rules of the Hospital, and maintain that the Board cannot deprive me of the right to visit that institution, without giving me a chance to disprove any false statements that have been made against me.

"I beg to assure the Board that I do not desire to follow a litigious course, but consider it is a duty I owe to the public, to the medical profession, and to the public press, to demand the charges and evidence under which I am deprived of my rights in the matter. The meanest criminal in the land can claim the privilege of being heard in his own defence; so that, should the Board continue to evade my just demand, I must denounce such a course as the height of tyranny and oppression.

"Trusting that on reflection the Board will see the justice and propriety of granting my request, or of rescinding their order,

"I have the honor to be, Sir,
Your most obedient servant,
"J. W. Brent, Esq., S. J. Stratford.
Secretary, H. T. B."

I understand that the letter has been laid before the Board, and that no answer will be returned to me—consequently I consider it my duty to lay the matter before the public, and trust that your love of fair play, and hatred of tyranny in every shape, will cause you not to refuse this communication a place in your columns.

In thus stating my case, I will simply say that the law under which the Hospital Trustees manage the affairs of that institution is a public act,—16 Vic. cap. 220. They have power to make bye-laws which require the sanction of the Governor General in Council within 30 days. In this case I have shewn that they have made a bye-law (Rule 4, sec. 7th) and I, as a licensed medical practitioner, had a right to avail myself of that law, until it was repealed; when the bye-law was sanctioned by the Governor General it became a law of the Province, and was as binding upon the Hospital Trustees as it was upon me. Such a law cannot surely be permitted to become an instrument of tyranny and oppression in the one case, and of favoritism in another, without some slight indication of the reason which has caused the action of the Board. Whatever might have been the secret influence which malice has brought to bear against me, it must be pretty evident that the crime I have committed cannot be very heinous or the Board would not in all probability have hesitated to render it public. It is to the Board I have to look for redress in this matter, and as it has been refused me, I shall be under the unpleasant necessity of laying the matter before the Executive Government. My desire would be, not to oppose the Hospital Trustees in this matter, as it is evident they have been misled by false statements; but as they have assumed the case, there is no alternative left me, and consequently as all means are allowable in a state of treacherous warfare, I feel myself bound to make a complete report of the circumstances of my own individual case, together with a detail of the condition of the Hospital, and of the treatment of the patients I have witnessed therein, for the special information of His Excellency the Governor General. I regret extremely the last alternative, as I did not desire to expose any professional matters connected with the institution; but when I have been treated so basely and tyrannically, I shall, I am sure, be excused the employment of all reasonable means of bringing my opponents to a sense of their injustice. I take these means with less hesitation, as I consider that the whole medical profession has been insulted and injured by the injustice heaped upon me. In its present condition the section of the Hospital by-law I have pointed out, is a snare and a delusion, if it can be so completely trampled under foot without reason or excuse; and as long as it can be employed after the above manner, it is simply an insult to the whole profession, any member of which may be treated precisely as I have been.

Hoping that the importance of this matter, will excuse the length of my communication,

I remain, Sir,

Your obedient servant,

S. J. STRATFORD,

M.R.C.S. Eng.

Toronto, 12th June, 1854.

N. B.—I have reason to believe that the crime which has called down the vengeance of the medical officers of the Toronto General Hospital, is the publication of a Clinical Lecture (to which I was publicly invited), in the Upper Canada Medical Journal, and which the subscribers to that journal know was given with fairness and justice.

S. J. S.

Sincerely desirous of avoiding the necessity of compromising Dr. Widmer in this unpleasant affair, we determined to exhaust every effort to effect a settlement of it or to obtain the charges made by the parties who quietly shelter themselves behind him, we addressed the following letter to Dr. Widmer, but have not received any reply; in consequence we give the letter, and shall make our remarks as to the cause of the combinations against us at a future period, and may possibly illustrate our observations by the mode in which polypus tumours of the uterus are treated and amputation of the thigh is performed in the Toronto General Hospital; but we would far rather not touch upon subjects of this nature until compelled to do so.

Yonge Street,
Toronto 20th June, 1854.

Dear Sir,—The honor and respect I naturally owe to you, as one of the senior members of the Medical Profession in Canada West, would have been sufficient to have caused me to hesitate in severely reprobating the course you have pursued towards me, with regard to my expulsion from the Toronto General Hospital; when this is coupled with an intimate acquaintance for upwards of twenty years, —and to it may be added the personal esteem I have ever expressed towards you, as the friend and school-fellow of my late lamented father,—this would be a still further reason why I prefer again appealing to your kinder feelings and better judgment in this matter, rather than of proceeding to extreme measures,—why I would prefer to solicit that you act as mediator between me and my unknown assailants, rather than that you stand in the breach as an unjust and tyrannical despoiler of the rights of the Medical Profession, and have yourself to bear the odium of this unpleasant affair. In making such an appeal to you, be assured it is not from any fear of the consequences or results, for I am sure when the matter becomes public that I shall be upheld by the public, the press and the Medical Profession; but simply from a desire to save the necessity of placing you in so despicable a position before the world and of exposing the extreme degredation to which some of the members of the

Medical Profession have arrived at in Canada West. Be sure that the parties who are now urging you onwards, who are making you a tool for their own base and illiberal ends, cannot but in the end meet with the due reward which their conduct richly merits. Some of them hate and detest you, and often call you by the vilest epithets; and as soon as they have attained their ends, and you have served their purpose, will be found among the first to execrate and abuse you. These are the motives that induce me to endeavour to arouse the better sympathies of your nature before I shall proceed to any dernier resort. I am sure that upon reflection, you must be fully convinced that I have committed no act against the Hospital laws. which had you not have been misled, or otherwise interested by your connection with some of the parties, you could not have construed into a violation of the rules; and had you desired to act as the fair administrator of the law, with which you have been entrusted for the public good, you would not have essayed to punish me, without giving me a chance to have exposed the falsehoods which may have been charged against me. Without urging any further reasons, I would simply request you to act as a mediator in this matter, and beg to assure you if it can be reasonably shown that I have erred in whatsoever is laid to my charge, I shall be (as I ever have) ready to make the *amende honorable* which the case shall demand.

I shall await your answer; but if I do not receive one before eleven o'clock to-morrow I shall conclude that you are disinclined to do me the justice my case plainly demands at your hands.

I have the honor to be, dear Sir,

Your obedient servant,

S. J. STRATFORD.

SELECTED MATTER.

A COURSE OF LECTURES ON ORGANIC CHEMISTRY.

Delivered in the Laboratory of the Royal Institution of Great Britain, by Dr. A. W. Hofmann, F.R.S., Professor of the Royal College of Chemistry.

LECTURE IX.

GENTLEMEN—

I promised you, in the last lecture, an account of urea as a substance closely connected with the three extraordinary acids which I have brought under your notice. I now proceed to describe to you this highly remarkable body. Urea was first observed as early as 1773, by a French chemist, named Rouelle, who described it under the name of *extractum saponaceum urinæ*. Rouelle, however, obtained this substance only in a state of great impurity, and it was not until 1799 or 1800 that its investigation was successfully performed by Fourcroy and Vauquelin. We may, therefore, say, that the knowledge of urea dates from the commencement of this century. The first accurate analysis of urea was made by Dr. Prout. His experiments have been repeatedly corroborated by many other chemists, and they led to the formula

$$C_2 \; H_4 \; N_2 \; O_2$$

for urea. But let us first ascertain how this substance is procured from urine before entering into farther details regarding its characters. For this purpose the fresh urine of man is evaporated to a weak syrupy consistence, and mixed, after cooling, with pure concentrated nitric acid. A brownish crystalline mass is at once separated, which consists of a combination of urea with nitric acid. These crystals, which are exceedingly brown, are collected and pressed, and then re-dissolved in boiling water. Treatment with animal charcoal and one or two re-crystallizations renders them nearly white. It remains now only to separate the nitric acid. This may be done in a variety of ways. A simple plan consists in adding to the solution an excess of carbonate of baryta, and evaporating the mixture to dryness in the water-bath. The residue, which now contains free urea, together with nitrate of baryta and excess of carbonate of baryta, is exhausted, at a moderate temperature, with strong spirits of wine, from which the urea crystallizes on cooling.

The existence of urea in so important a secretion as the urine, would have at all times secured a considerable amount of attention to this compound. But the interest of this body has been very much increased by the results obtained in the study of its chemical characters, and by the remarkable circumstances under which its formation was observed at a later period.

In the last Lecture, you became acquainted with the salt which is produced by the action of oxidizing agents upon cyanide of potassium, and which I described to you as oxycyanide of potassium, or cyanate of potassa. You recollect, moreover, the curious process by which Professor Wöhler succeeded in producing the hydrogen-compound corresponding to oxycyanide of potassium, or cyanic acid. An elaborate investigation of the salts of this acid has led Professor Wöhler to one of the finest discoveries which is upon record in the domain of organic chemistry. Among the several salts examined by this chemist, was the ammonium-compound. He produced this substance by the action at the common temperature of dry ammonia-gas upon the vapour of cyanic acid,

$$\underbrace{NH_3}_{\text{Ammonia.}} \quad + \quad \underbrace{H \; Cy \; O_2}_{\text{Cyanic Acid.}} \quad = \quad \underbrace{NH_4 \; Cy \; O_2}_{\substack{\text{Oxycyanide of} \\ \text{ammonium.} \\ \text{Cyanate of ammonia.}}}$$

when the compound condenses as a white, light powder, upon bringing the two bodies in contact. The substance thus produced possesses all the characters of a true oxycyanide. It is decomposed like the potassium-compound, when boiled with either dilute acids or alkalies; the latter evolving ammonia, an alkaline carbonate remaining behind, while the former evolve cyanic acid. the greater part of which splits, as you have seen, at the moment of liberation, into carbonic acid, which escapes with effervescence, and ammonia which is fixed by the acid. Oxycyanide of ammonium, obtained in this manner, is apt to contain an excess of ammonia. Professor Wöhler endeavored to purify his substance by re-crystallization from water, in which it is easily soluble. The aqueous solution, when concentrated by evaporation, deposits, on cooling, long acicular crystals of remarkable beauty. But how great was Wöhler's astonishment, when he found that, by this simple operation, the character of his original substance had undergone an entire alteration. It exhibited no longer any of the properties which distinguish the oxycyanides; dilute acids and alkalies had no longer the slightest effect upon it, although its composition was still that of the true oxycyanide of ammonium. A minute examination of this modified compound soon convinced Professor Wöhler, that the substance in question was nothing else but urea, the crystalline constituent of urine. We have, in fact,

$$\underbrace{NH_4\,Cy\,O_2 \quad = \quad NH_4\,C_2\,NO_2}_{} \quad = \quad \underbrace{C_2\,H_4\,N_2\,O_2}_{}$$

Oxycyanide of ammonium.	Cyanate of ammonia.	Urea.

This discovery, to which I have alluded already in the commencement of this course, was of considerable importance, inasmuch as it proved, for the first time, and in a most striking manner, the possibility of producing artificially the proximate constituents of plants and animals.

The artificial formation of urea is so readily effected, that urine is scarcely any longer used for preparing this substance. However, to produce economically large quantities of urea, it is necessary somewhat to modify the process. Instead of preparing the cyanate of ammonia by the direct union of its constituents, chemists produce the same effect by double decomposition, according to a method suggested by Professor Liebig,—namely, by the action of sulphate of ammonia upon cyanate of potassa,—when sulphate of potassa and cyanate of ammonia, or urea, are formed.

$$K\,Cy\,O_2 \;+\; NH_4\,SO_4 \;=\; KSO_4 \;+\; NH_4\,Cy\,O_2$$

This flask contains a solution of cyanate of potassa, obtained by exhausting with cold water the black residue which remains after the tinder-like combustion of a mixture of dry ferrocyanide of potassium and binoxide of manganese. I mix this liquid with a saturated solution of sulphate of ammonia. On agitating, crystals of sulphate of potassa are deposited, which I separate by filtration or decantation. The solution now contains the true cyanate of ammonia, as can readily be proved by the action of acids and alkalies. With dilute hydrochloric acid, the liquid powerfully effervesces: while the addition of hydrate of lime causes the evolution of ammonia. Meanwhile, I have heated the remainder of our solution to ebullition; and you observe, that the solution is now no longer affected by these re-agents. Under the influence of heat, the molecules of cyanate of ammonia have re-arranged themselves, and have assumed the stabler position which they hold in urea. The presence of urea in the solution may be exhibited, moreover, by the addition of concentrated nitric acid, which immediately precipitates a crystalline magma of nitrate of urea.

In order to obtain the urea perfectly pure, according to this method, the mixture of cyanate of potassa and sulphate of ammonia must be evaporated to dryness on a water-bath. The residue is exhausted with alcohol, from which the urea crystallizes upon evaporation.

The remarkable change which cyanate of ammonia undergoes in all its properties, when passing over into urea, has greatly engaged the attention of chemists. Many speculations as to the mode in which the molecules may be arranged in urea have been brought forward, in order to explain the new character which the compound assumes. These speculations have not, as yet, led to any decisive results; the molecular construction of urea still remains undetermined. Suffice it to say, that the properties of cyanic acid in this substance have entirely disappeared; that urea, in its deportment with other substances, has retained the fundamental properties of ammonia which enters into its composition. Like the latter, it combines with acids, and gives rise to a series of well-defined salts. Of these the nitrate, which I had repeated opportunities of mentioning, and the oxalate, which falls down as a crystalline precipitate on mixing urea-solution with oxalic acid, are the most familiar representatives. The former contains $C_2 H_4 N_2 O_2$, HO, NO_5; the latter $C_2 H_4 N_2 O_2$, HO, $C_2 O_3$. In fact, urea belongs to the class of compounds generally designated as "organic bases,"—a group with which I hope to make you better acquainted in a future lecture, and of which ammonia is the type. Urea and its compounds may be viewed as ammonia and ammonia-salts associated with an equivalent of cyanic acid, which has become perfectly latent in this association, and which re-appears again only under the influence of powerful agents of decomposition. Thus we find that concentrated sulphuric acid, or fusion with an alkali, produces upon urea the same effect as dilute acid and dilute alkali upon cyanate of ammonia. Under these circumstances, urea assimilates 2 equivalents of water, and is converted into carbonic acid and ammonia.

$$C_2 H_4 N_2 O_2 + 2\,HO = 2\,CO_2 + 2\,NH_3$$

The same change occurs if urea is left in contact with fermenting substances. Thus we find that putrid urine contains at least not a trace of urea. Addition of nitric acid to the evaporated liquid no longer produces a precipitate of nitrate of urea, but a brisk effervescence of carbonic acid, while the action of an alkali evolves ammonia in abundance

The presence of cyanic acid in urea may, likewise, readily be traced by the action of substances which destroy the ammonia in it. Chlorine decomposes free ammonia, with the formation of hydrochloric acid and evolution of nitrogen. Ammonia undergoes the same decomposition when associated with cyanic acid; the latter, in the moment of its liberation, assimilates water, and splits into carbonic acid and ammonia, which, in its turn, is likewise decomposed by chlorine.

$$\underbrace{C_2 H_4 N_2 O_2}_{\text{Urea.}} + \underbrace{2\,HO}_{\text{Water.}} + \underbrace{Cl\,6}_{\text{Chlorine.}} = \underbrace{N_2}_{\text{Nitrogen.}} + \underbrace{2\,CO_2}_{\text{Carbonic acid.}} + \underbrace{6HCl}_{\text{Hydrochloric acid.}}$$

In a similar manner nitrous acid behaves, the action of which upon ammonia gives rise to the formation of water, and to the evolution of nitrogen By passing the vapour of nitrous acid, disengaged by the action of nitric upon arsenious acid ($NO_5 + As\,O_3 = NO_3 + As\,O_5$), into a solution of urea, a lively effervescence ensues, and equal volumes of nitrogen and carbonic acid are disengaged, the latter arising from the decomposition of liberated cyanic acid.

$$\underbrace{C_2 H_4 N_2 O_2}_{\text{Urea.}} + \underbrace{2NO_3}_{\text{Nitrous acid.}} + 2\,HO = \underbrace{N_4}_{\text{Nitrogen.}} + \underbrace{2\,CO_2}_{\text{Carbonic acid.}} + \underbrace{6HO}_{\text{Water.}}$$

This re-action explains why, in the preparation of urea from urine, often not a trace of urea is obtained, if the nitric acid used in precipitating the urea, as nitrate, contain a small quantity of nitrous acid. It is, lastly, by the action of heat upon urea that the presence of cyanic acid in this compound may be readily exhibited. At a moderate temperature, urea fuses without change, but, when strongly heated, it is decomposed, with copious evolution of ammonia. The residue in the retort consists almost entirely of cyanuric acid, which retains but a minute quantity of ammonia. If the process of heating be continued, the cyanuric acid undergoes the change which I pointed out to you in one of the preceding lectures—it is converted into cyanic acid. This re-action is at once intelligible if you recollect that urea contains the elements of cyanic acid and ammonia. Three equivalents of urea split into three equivalents of ammonia which are evolved, and three equivalents of cyanic acid which, at the existing temperature, coalesce to form cyanuric acid, and separate again only upon submitting the latter to a higher degree of heat.

$$\underbrace{3\ C_2\ H_4\ N_2\ O_2}_{\text{Urea.}} = \underbrace{3\ N\ H_3}_{\text{Ammonia.}} + \underbrace{H_3\ C_6\ N_3\ O_6}_{\text{Cyanuric acid.}}$$

$$\underbrace{H_3\ C_6\ N_3\ O_6}_{\text{Cyanuric acid.}} = \underbrace{3\ H\ C_2\ NO_2 = 3\ H\ Cy\ O_2}_{\text{Cyanic acid.}}$$

In order to complete the history of urea, it remains now only to add a few words regarding the methods used in determining the amount of urea in urine. The quantity of urea secreted in the animal organism during a given period, may be viewed as a measure of the change of matter in the organism, *i. e.*, of the chemical transformation of its tissues. An exact knowledge of this quantity is of considerable interest, both to the physiologist and to the medical man: and chemists have long endeavored to find a simple and accurate method of arriving at this result.

Several methods of determining the quantity of urea in urine have been proposed. The oldest plan consisted in evaporating the urine to a certain degree of consistence, and then forming the difficultly soluble precipitate of nitrate of urea, which I have had repeated opportunities of exhibiting to you. This precipitate was collected upon a filter, washed with the smallest quantity of water, dried, and weighed. On account of the solubility of this compound, it is obvious that this method could yield scarcely approximate results, inasmuch as the quantity of urea retained upon the filter entirely depended upon the quantity of water used in washing.

Another method consisted in determining the amount of nitrogen in the evaporated urine, from which the uric acid had been previously separated by the soda-lime process which I have explained to you in one of the earlier lectures. This method is based upon the assumption, that urine, freed from uric acid, contains no other nitrogenous principle except urea, an assumption which, although true in most cases, is known to be incorrect in some. Moreover, the necessity of performing a complete nitrogen-determination which, although sufficiently easy in the hands of the chemist, is scarcely simple and expeditious enough for the physiological and medical observer, has prevented this method from being generally adopted.

Two other suggestions for the determination of urea are founded upon some of the changes of urea to which I have already alluded. I have pointed out to you, that, under certain circumstances, urea behaves like cyanate of ammonia. This is the case, for instance, when urea is treated with water under pressure; it then assimilates water, and splits into carbonic acid and ammonia. Of this reaction Bunsen has availed himself. He exposes urine mixed with an ammoniacal solution of chloride of barium, in an hermetically sealed glass tube, to a temperature of from 480° to 500° Fahr. (250° to 260° C.), when the urea is decomposed, and furnishes a quantity of carbonate of baryta which is equivalent to the amount of urea.

Millon, lastly, makes use of the change which urea undergoes when acted upon by nitrous acid, and which I have illustrated to you experimentally. The carbonic acid evolved in this reaction is collected in an ordinary potash apparatus; from the increase in weight the amount of urea is calculated without difficulty.

Both these methods which, in the hands of skilful experimenters, yield perfectly accurate results, have, nevertheless, been almost exclusively employed by their inventors; the successful performance of these analyses requiring no small amount of experimental dexterity, and involving, moreover, a considerable expenditure of time and labour.

Pressed by his physiological friends, Professor Liebig has devoted much time and work to the elaboration of an accurate, and, at the same time, easy and expeditious process. His labours have been perfectly successful.

The new method is founded upon the fact of nitrate of urea forming, with protoxide of mercury, a white compound which is perfectly insoluble in neutral solutions. This compound is not affected by carbonate of soda, while salts of mercury yield with this re-agent a precipitate of yellow protoxide of mercury. The process, therefore, consists in adding to the urea solution which is to be analyzed a standard solution of nitrate of mercury, until a drop of the liquid in which the white precipitate is suspended, when mixed upon a watch-glass with carbonate of soda, exhibits a yellow colour, which shows that an excess of mercury-solution has been added. The performance of the experiment involves several precautions, which will become at once intelligible if we examine somewhat more minutely the composition and the properties of the new urea-compound $C_2 H_4 N_2 O_2$ being represented by Ur, this compound contains—

$$Ur, NO_5 + 4 HgO,$$

and is formed according to the following equation:—

$$Ur + 4 (HgO, NO_5) = Ur, NO_5 + 4HgO + 3NO_5$$

In the formation of one equivalent of the compound, not less than three equivalents of nitric acid are set free, retaining in solution a considerable quantity of the precipitate, which is rather freely soluble in nitric acid. This nitric acid has to be neutralized. Generally, this is done by baryta-water, which is added in the commencement of the operation, and thus separates, moreover, the phosphates and the sulphuric acid present in the urine.

Another difficulty presents itself in the solubility of the urea-compound in liquids containing common salt. This solubility depends upon the conversion of the oxide of mercury into the chloride which produces no insoluble compound with urea. You observe, corrosive sublimate has not the slightest effect upon a solution of urea. Now, since nitrate of mercury is converted into chloride by the action of common salt, which is itself converted into nitrate of soda,

$$HgO NO_5 + NaCl = HgCl + NaO, NO_5$$

it is evident, that this re-agent will produce a precipitate in a solution of urea containing common salt, only after the whole of the common salt is converted into nitrate of soda. Now, since urine contains almost always small quantities of common salt, (from 1 to 1½ per cent) it is necessary to make an allowance for this deportment; for, unless we did so, the amount of urea found would be too high. In accurate experiments, Liebig obviates this error by determining the amount of chloride of sodium by means of a second standard solution of mercury which is added to the urine, until a permanent precipitate indicates that all the chloride is converted into nitrate of soda. The quantity of chlorine in the urine being thus determined, the chlorine is now removed from a fresh portion by means of a solution of nitrate of silver likewise graduated. The urine thus freed from chlorine may now be directly tested with the standard solution of mercury.

I refrain from entering into farther details regarding this beautiful process, which furnishes the amount both of common salt and of urea in urine with great accuracy and surprising rapidity; my object being to delineate to you the principle of the method, not to teach you the actual performance of the experiment, for which the practice of the laboratory is indispensable. The manipulations and the preparations of the standard solutions of mercury are minutely described by Liebig in his paper on this subject, which has been communicated to the Chemical Society of London*.—*Medical Times and Gazette.*

* Quarterly Journal of the Chemical Society, Vol. VI.

On a Substance presenting the Chemical reaction of Cellulose, found in the Brain and Spinal Cord of Man. By RUDOLPH VIRCHOW. (Sept. 4, 1853.) Virchow's Archiv. B. VI., H. 1, p. 135.

It is well known that Carl Schmidt * was the first to discover in the Ascidians the presence of a principle previously known to exist only in plants, viz. *cellulose*, and to show that it was a constituent of the animal tissue. The researches of Kölliker and Löwig,† of Schacht,‡ and of Huxley,§ have established this important fact. The occurrence of this substance, however, was limited to a comparatively very low class of the Invertebrata,

* 'Zur Vergleichenden Anat d. Wirbellos.' Thiere, 1845, p. 61.
† 'Ann. d. Sci. Nat.' 1846, p. 193.
‡ 'Müll. Archiv.,' 1851, p. 176.
§ 'Quart. Journ. Micr. S.,' vol. I. p. 22, 1853.

and the further discovery made by Gottlieb, in *Euglena viridis*, viz. that this *infusorium* contains *paramylon*, a body isomerous with starch, also had reference only to a creature in the lowest class of the animal kingdom.* Nothing of the kind, on the other hand, has been known as existing in the vertebrata, and it is only since the discovery by C. Bernard—that the liver produce sugar—that we have had reason to suppose that substances belonging to the *amylum* series may also have a representative.

From histological considerations, it had struck me that the umbilical cord of man presented a great resemblance in structure to the cellulose tissue of the Ascidians (Wurzb. Verh. 1851. Bd. II., p. 161, *note*), and I was only the more confirmed in this notion by Seacht's observations, so that I have since directed my researches with care to the subject. But in many instances this was in vain, as, for instance, in the *ova* of amphibia and fishes, the remarkable vitelline plates of which I described (Zeitsch. f. wiss. Zoologie. 1852. Bd. IV., p. 210).

I was more fortunate when, a short time since, I directed my attention to the so-termed *corpora amylacea* of the brain, upon the precise nature of which, contrasted with the other kind of amyloid bodies in man, I had not previously arrived at any accurate notion. (Wurzb. Verh. 1851, Bd. II., p. 51.) It was now apparent that these bodies assumed a pale-blue tinge upon the application of iodine, and upon the subsequent addition of sulphuric acid, presented the beautiful violet colour which is known as belonging to *cellulose;* and which in the present instance appears the more intense from the contrast with the surrounding yellow or brown nitrogenous substance.

I have repeated this experiment so often, and with so many precautions, that I regard the result as quite certain. Not only have I instituted comparative researches in different human bodies, and in the most various localities, but I have also noticed the action of the reagents under all possible conditions. The experiment is best made in the mode adopted by Mulder and Harting, with regard to vegetable cellulose (*vide* Moleschott 'Physiologie des Stoffwechsels,' p. 103), viz., by causing the action of diluted sulphuric acid to follow that of a watery solution of iodine. The iodine solution should not be too strong, for the observation may then be impeded by its precipitation, and, on the other hand, care must be taken that the iodine exerts due action upon the substance. Owing to the volatility of the iodine, and its great affinity for animal substances, its action is usually very unequal, so that the border of the object and not the centre may be penetrated by it, or perhaps, of spots in close contiguity, one will contain iodine and the other not. It is consequently always advisable to repeat the application of the iodine several times, but to avoid the addition of too much. Upon the subsequent addition of sulphuric acid, if the action have been too powerful, the result is a perfectly opaque, red-brown colour. The most certain results are obtained if the sulphuric acid be allowed to act very slowly. In fact, I have procured the most beautiful objects in allowing a preparation covered with the glass to remain undisturbed with a drop of sulphuric acid in contact with the edge of the covering-glass for 12 to 24 hours. Under these circumstances, the most beautiful light violet-blue was occasionally presented. Lastly, I would just intimate that accidental mixtures of starch or cellulose may readily happen, seeing that very light fibres or minute particles from the cloths with which the object and covering-glasses have been cleaned, may very easily be left upon them, which would afterwards exhibit the same reaction as the above.

Every precaution having been taken, the following results will be obtained:—

1. The *corpora amylacea* (Purkinje) *are chemically different from the concentric-spherical corpuscles, of which the brain-sand is composed,* and with which they have hitherto usually been confounded. The organic matrix of the brain-sand granules is obviously nitrogenous: it is coloured of a deep

* The pertinacity with which German naturalists cling to the animal nature of Euglena we must confess, is very surprising to us, who are equally satisfied that it is as much a subject of the vegetable kingdom as the motile zoospores of any Alga, such as *Volvox, Hydrodictyon, Protococcus,* &c.

yellow, by iodine and sulphuric acid. This is true not only of the sabulous matter in the pineal gland and choroid plexuses, but also of that of the Pacchionian granulations and of the *dura mater*, as well as of the dentate plates in the spinal arachnoid. In all these parts I have, in general, nowhere obtained the blue reaction, except in a few spots in the pineal gland. It would therefore, for the future, be convenient to restrict the name of 'corpora amylacea' to the bodies containing cellulose.

2. These bodies exist, so far as I have at present found, only in the substance of the *ependyma ventriculorem* and its prolongations In this I include especially the lining of the cerebral ventricles and the transparent substance in the spinal cord described by Kölliker as the *substantia grisia centralis* (Mikrosk. Anat. Bd. II.1, p. 418). With respect to the cerebral ventricles, I have already repeatedly stated that I find them to be lined throughout with a membrane belonging to the connective tissue class, upon which rests an epithelium. This membrane contains very fine cellular elements, and a matrix, sometimes of more dense, sometimes of softer consistence, and is *continued on the internal aspect without any special boundary between the nervous elements*. In the deeper layers of this membrane, and in immediate contiguity with the nerve fibres, the cellulose corpuscles are found most abundantly, and they are also especially numerous where the *ependyma* is very thick. They are consequently very abundant on the *fornix*, *septum lucidum*, and in the *stria cornea* in the fourth ventricle. In the spinal cord, the substance corresponding to the *ependyma* lies in the middle, in the grey substance, in the situation where the spinal canal exists in the fœtus. It there forms evidently a rudiment of the obliterated canal, such as it presented in the obliteration of the posterior cornu of the lateral ventricle, which is so frequently met with. In a transverse section of the cord, it is easily recognised as a gelatinous, somewhat resistant substance, which may be readily isolated. Its cells are much larger and more perfect than those of the cerebral *ependyma*. This *ependyma spinale* forms a continuous gelatinous filament, which extends to the *filium terminale*, and might therefore, perhaps, be most suitably described as the *central ependymal filament*. In it the cellulose granules are also found, though, as it would seem, more abundantly in the upper than in the lower portion. In other situations I have sought for these bodies in vain, and in particular I have been unable to find them in the external cortical layer of the cerebrum, or anywhere in the interior of the cerebral substance.

3. Since, from the experiment of Cl. Bernard, who produced saccharine urine by wounding the floor of the fourth ventricle in the rabbit, there appeared to be reason to conclude that the existence of cellulose was connected with that phenomenon. I sought for it also in rabbits, but in vain. I found in that situation both in the fourth, and the third, and in the lateral ventricles, a very beautiful tessellated *epithelium* with very long vibratile cilia, but no cellulose.

4. The cellulose granules, therefore, appear to be everywhere connected with the existence of the *ependyma-substance* of a certain thickness, and might perhaps be regarded as a constituent of it. They occur of excessively minute size, so that the *nuclei* of the ependyma scarcely correspond with them. Can they be formed out of the latter? The larger they are the more distinctly laminated do they appear. But there is never any indication in them of a nitrogenous admixture, recognizable by a yellow colour. The centre only is usually of a darker blue, and consequently perhaps more dense than the cortical laminæ.

5. As to an introduction of these bodies from without, such a supposition is the less probable because a similar substance is nowhere else known. We are acquainted with a series of varieties of vegetable cellulose, but the substance now in question appears to be distinguished above all by its slight power of resistance to reagents, seeing that concentrated acids and alkalies attack it more powerfully than is usually the case with the cellulose of plants.

6. In the child I have as yet sought for it in vain; so that, like the "brain-sand," it appears to arise in a later stage of development, and probably may have a certain pathological import.

Since writing the above, Professor Virchow has repeated and confirmed his observations, and ascertained in addition that similar bodies also occur in the higher nerves of sense. He found them most abundantly in the soft gray interstitial substance of the olfactory nerve, less frequently in the acoustic, although the observations of Meissner (Zeitsch. f. rat. Med., N. F., Bd. III., pp. 358, 363), would indicate a proportionately great disposition to their formation in that situation. Rokitansky appears to have seen them in the optic nerve, and from an oral communication the author has learned that Kölliker has found them in the retina.

Having already stated that the *ependyma* is continued without special limitation among the nervous elements, the author goes on to observe that it is now apparent that there is a continuous extension of the same substance in the interior of the higher nerves of sense. From a series of pathological observations, he concludes that a soft matrix referrible mainly to connective-tissue substance, everywhere pervades and connects the nervous elements in the centres, and that the *ependyma* is only a free superficial expansion of it over the nervous elements. The opinion, that the epithelium of the cerebral ventricles rests immediately upon the nervous elements, appears to have arisen from a confusion of this interstitial substance with the true nerve-substance.

The isolation of the *corpora amylacea* in larger quantity, in order that they should be subjected to chemical analysis, the author has not yet succeeded in effecting. Nevertheless it seems impossible to entertain any doubt as to their cellulose nature. No other substance is known which affords the same reaction; and although the author has examined the most various animal tissues, and has accurately investigated, particularly, the concentric corpuscles occurring elsewhere, as in the *thymus* in *tumours*, &c., nothing of the same kind has presented itself.—(*Sept.* 25, 1853).

An abstract of the above observations also appears in the 'Comptes Rendus,' for the 26*th Sept.*, 1853, p. 492, but containing nothing additional.

Being desirous of verifying the above observations, I have examined the brains of one or two individuals; and, as my results differ in some respects from those of Professor Virchow, I will here briefly state them, leaving a more detailed account of the matter to a future opportunity, my observations at present having been too scanty to justify the expression of any settled opinion. The first case I examined was that of a young man who died of the consecutive fever of cholera, after an illness of five or six days, during the whole of which period the renal secretion was completely suppressed. What I noticed in this case was:—

1. The enormous abundance of the *corpora amylacea* in certain situations, as the *ependyma ventriculorum*, particularly on the *septum lucidum*, and more especially also on the choroid plexuses, upon gently scraping the surface of which a fluid was obtained containing these bodies in a most surprising quantity.

2. That they existed in immense abundance in the olfactory bulbs and in the superficial parts of the brain, both cortical and medullary, contiguous to the tract of the olfactory nerves. But scarcely any part of the *cerebrum* and *cerebellum* could be examined, at all events towards the surface, without meeting with some or more; and they occurred abundantly in the very middle of the *cerebellum*. Their distribution, however, was very irregular, inasmuch as they abounded in some spots and were nearly, if not altogether, wanting in others. I could find none in the *corpora striata*, where they seemed to be replaced by 'brain-sand,' of which more will be said afterwards.

3. The cerebral substance in immediate contiguity with the *corpora amylacea* appeared quite natural.

4. The corpuscles were starch and not cellulose, and possessed all the structural, chemical and optical properties of starch, as it occurs in plants, as the following few details will show:—

They were of all sizes, from less than a blood-disc up to 1-500th inch or more—generally more or less ovate, but many irregular in outline, and apparently flattened, as all the larger kinds of starch I believe are. Many of the larger ones showed the appearance which, in starch, has been erroneously described as indicative of a laminated structure; whilst in others this appearance under any mode of illumination certainly did not exist. The point that would correspond with the so-called *nucleus* of a starch-grain was, unlike that of most kinds of starch, central, and consequently the laminated marking was concentric to the grain, which is rarely the case in the starch of plants. This apparent lamination depends, as I believe, upon the same circumstances as in other starch (*vide* Trans. Micr. Soc., Quart. Journ., vol. i., p. 58), that is to say, upon the corrugation of a thin *sacculus*. That this was the case I satisfied myself by the use of sulphuric acid and of Schultz' solution (chloride of zinc and iodine), in the mode described in my paper above quoted. By these means, but more readily and conveniently by far by the latter, the *corpora amylacea* could be seen to unfold into empty, flaccid, thin-walled, blue sacculi, six to eight times larger than the original grain. Their structure thus appearing to be identical with that of starch, the identity of their chemical composition was rendered evident with equal facility. Simple watery solution of iodine coloured them deep blue, which ultimately became perfectly black and opaque. They were soluble after swelling and expanding in strong sulphuric acid, and by heat; and, moreover, they acted upon polarised light in the same way as starch does. Some of the smaller grains exhibited a distinct and sharply-defined black cross, of which the lines crossed at angles of 45° in the middle of the grain, but in the majority there was only a single dark line in the long diameter of the grain, and which seemed always to correspond with an irregular fissure or hilus, as it might be termed, in the same direction, which was presented in a great many of the grains, and seemed to be the indication of a partial inrolling of them, as in the starch of the horse-chestnut. This longitudinal fissure was not unfrequently crossed by a shorter one at *right* angles. When the covering-glass was closely pressed, the grains were easily crushed, breaking-up in radiating cracks around the margin; and sometimes, when thus compressed, a concentric annulation would become evident, which was before inapparent.

In the *corpora striata*, as I have mentioned above, I could find few or no starch-grains, but here an appearance presented itself which seems to be connected with their formation. Many particles of sabulous matter or crystalline corpuscles of the ordinary "brain-sand," were met with, all of which, instead of lying like the starch-grains, in the midst of unaltered nerve-substance, were lodged in irregular masses of what appeared a fibrinous or immature connective tissue-substance; and, in this instance, upon the addition of iodine, each mass of crystals was found to be immediately surrounded by an irregular thickness of a transparent matter, which was turned not *blue*, but a light *purplish pink* by that reagent—a substance, in fact, closely resembling in that respect the very early condition of the cellulose wall; for instance, in *Hydrodictyon*,—an immature form, as it may be termed, of cellulose.

In a second case, that of an old man—dead of chronic dysentery, and who died comatose—I found the ventricles distended with about three ounces of clear fluid. The surface of the *ependyma* throughout all the continuous cavities was studded like shagreen with minute transparent granulations, which, on microscopic examination, appeared finely granular and homogeneous, or sometimes faintly fibrillated. In this case there were, I think, no *corpora amylacea* in the *ependyma* (at least I found none), nor in the central substance of the brain: a few were met with in the peripheral portions, especially on the summits of the hemispheres, and still more in the much-developed Pacchionian granulations, and there commingled with *other* concentrically-laminated bodies, which formed botryoidal masses imbedded in a stroma of immature connective tissue: these bodies, which might, to distinguish them, be termed the 'chalcedonic corpuscles,' were rendered yellow by iodine. In this case also I did not notice the *quasi* cellulose-deposit around the particles of 'brain-sand,' but in several instances I saw minute amylaceous particles (coloured blue by iodine) contained in cells which they only partially occupied.—GEO. BUSK.

Note.—In the 'Comptes Rendus,' No. 23, (Dec. 5, 1853.) are some further observations on the "Animal Substance analogous to Vegetable Cellulose," by R. Virchow, in which he announces the discovery of corpuscles presenting the same reaction as the *corpora amylacea* of the brain, in the Malpighian corpuscles of diseased human spleens—in the condition termed "waxy spleen" (Wachsmilz).

THE CHOLERA.

[Some months since we endeavoured to call the attention of the Government to the probable advent of the Asiatic Cholera, which may be reasonably expected to shew itself among the inhabitants of this Continent during the ensuing summer; and we endeavoured to impress upon all parties the necessity of being prepared before hand for the terrible visitation. We particularly urged the necessity for the establishment of local dispensaries and proper medical officers along each section of the public works; and more recently we have endeavoured to show the advantage of the introduction of a law compelling the municipalities to open public dispensaries for the gratuitous attendance upon the poor, in their several districts. It is clearly desirable to be prepared before hand in this case; so that we sincerely hope that the Government will not let the matter go unheeded.

Besides which, we find an excellent paper in the Lower Canada *Medical Chronicle* from the pen of Dr. Marsden, which agrees so perfectly with our own views of the hygienic management and treatment that should take effect during a cholera period, that we cannot forbear inserting it among our selected matter. The suggestions are highly practical and excellent of their kind, and should be followed out in all cases where it is possible; they should have effect not only in the cities but in the rural districts, as far as may be found practicable.]

Practical Remarks and Suggestions on Asiatic Cholera. By WILLIAM MARSDEN M.D., Governor of the College of Physicians and Surgeons, Lower Canada.

The possibility of this country being again visited, during the coming season by that democratic scourge of the human family—asiatic cholera, has induced me to throw together the following practical suggestions.

I shall, in the observations I am about to make, avoid any illusion to the debateable point, the contagiousness or non-contagiousness of cholera asphyxia, which is still a *vexata qustio* among many of the most eminent members of the medical profession, both in this country and abroad, to the very serious detriment of public hygiene; but I will, in the recommendations I may make, cast all the doubts into the human balance of public safety.

My suggestions will be of an individual or private character, as well as of a public and general nature. They are the results of my own observation and experience, during five distinct invasions, of active professional occupation, in the midst of sickness and death, and have impressed me with the conviction that no case is entirely hopeless. The vulgar maxim, that "prevention is better than cure," is hardly so applicable to any other form of disease "that flesh is heir to," as in cholera.

There are many persons, however, and among them medical men, who, from superficial observations, entertain the fallacious idea that cholera sometimes pounces upon its prey without any premonition of any sort, and hurries away its victim in a few short hours. I deny this position, and fearlessly call for proof to the contrary. I maintain that no individual in robust health has ever been suddenly attacked with the worst forms of cholera, and carried off without some premonitory symptom. We may all of us have seen persons walking abroad one day, apparently in perfect health, and hear of their having been consigned to the silent tomb on the next. I have, myself, frequently heard of such cases, but I never knew one. I have, on the contrary, invariably found, on diligent inquiry, that the self-deluded victim had not been quite as well as usual, or had indulged in some unaccustomed habit, and had been suffering under some species of functional derangement (most commonly "bowel complaint"), for some hours, and not unfrequently for some days, previous to the invasion of the fatal disease, and this, generally, under a false or assumed courage, the effects of fear, which they wished to hide from themselves, as well as from their friends. A more fatal delusion than this cannot possibly exist. I am firmly of Dr. Kirk's opinion, "that diarrhœa in this country always precedes cholera asphynxia; that this diarrhœa is always a curable complaint: and consequently, that this formidable disease,—the ways of which were wrapped in mystery, and inspired us with no feelings but gloom and despair,—may be calmly viewed by the eye of philosophy and common sense, as a malady, the secrets of which open *to us*, and the controul of which we have in our hands." Among the best prophylactic means in individual cases, I would suggest: regular and active bodily and mental employment; good air and exercise out of doors, daily (if possible); cleanliness, sobriety and temperance in all things; good and wholesome food, *and of the same description that* the person is in the habit of using: even in the continuance of old habits, that may in themselves be evil, when the disease has once made its appearance amongst us; avoiding such things only as experience has taught to be hurtful in each individual case: warm and comfortable clothing and bedding, and regular rest; the rigid avoidance of all quackery and quack medicines; and, early application to some honest and discreet physician, in the event of indisposition.

Among the public or general means of action that I would recommend, the first is, the organization of a board of health, which shall be furnished with absolute powers and ample means to carry out any plan of hygiene that may, by the exigencies of circumstances, be demanded. The board not too numerous, and to be composed of gentlemen of education, experience, and decision of character, without regard to their politics, and to consist of laymen, clergymen and physicians in about equal proportions.

Reports to be made daily to the Board of Health, from which all orders shall emanate, but no reports to be published until the season is closed, or cholera has disappeared, if it should come. The reports and proceedings of the Board, however, to be open to all persons for inspection, that may desire to see them.

The city to be divided into wards of convenient size, and each ward to be placed under the control of a visiting physician, appointed for the purpose, who shall make a daily domiciliary visit to every house in his ward.

All patients to be prescribed for and attended at their own residences if possible: and if not, at an hospital, of which there shall be a small one (with a dispensary attached) in each ward, or one in the centre of two wards, if practicable.

The visiting physician shall, in his daily domiciliary visits, enquire personally into the state of health of every member of each household in his ward; and, if there be any one sick, shall insist upon the immediate attendance of the family physician, or any other that they may choose; or if they have no choice at all, to prescribe for them, or remove them forthwith to the hospital.

The hospital wards shall be small, and contain from two to four beds in each, and in no case more than six, and then only for the use of convalescents.

The influence of mental impressions upon the health, as especially fear, is too well known to require more than a passing remark, in order to justify the withholding of published reports.

The object in having a small hospital in each ward is, firstly, that the patient may be at once placed under medical treatment, without the loss of valuable time, which has frequently occasioned death; and secondly, that the public gaze may not be shocked, and terror spread by seeing an unfortunate fellow-creature transported from one extremity of the city to another, often writhing in the agonies of death.

The advantages of attending the sick and afflicted at their own houses are manifold, as, besides the saving of time, the patient's mind will be at ease by being surrounded by the kindly attentions of sympathising friends.

The plan of having small hospital wards, in cases of cholera, is not new, but was first introduced (I think) at Guy's Hospital, London, and was found to answer the purpose intended admirably; the congregating of large numbers of patients together having been found to increase infection, as well as the virulence of the disease. The beneficial effects upon the minds of the patients in not seeing themselves surrounded by multitudes of their fellow-creatures writhing in agony in various stages of disease were also most apparent.

These remarks have been thrown together hastily, and I have to apologise for their imperfections and want of details; yet, if their effect, either directly or indirectly, be to snatch one single valuable life from the fangs of the fel destroyer, I shall consider myself amply repaid.

Quebec, 1854.

P.S.—The suggestions of a local nature contained in the foregoing hasty remarks were designed for Quebec, but they may, with the general principles be applied to any other locality. W. M.