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THE

UPPER CANADA JOURNAL

OF

Medical, Surgical, and Physical Science.

FOR JANUARY AND FEBRUARY, 1853.

ORIGINAL COMMUNICATIONS.

ART. XXVII.—*The Hip Joint, considerations on its injuries & disease, deduced from the Anatomy, by S. J. Stratford M. C. S. Eng. Toronto. Continued from the last Journal.*

INFLAMMATION OF THE SYNOVIAL MEMBRANE.

In the last Journal, we endeavoured to present a detail of anatomy of the Hip-joint, confident that a due appreciation of the rious structures will lead us to a just knowledge of its diseases—wl an attentive consideration of the action of the various muse which operate upon the parts, will at a future period, we do not, be found clearly to indicate the nature of the accidents; and v teach us a facility of relieving them, especially when displacem has occurred, that will appear very surprising when contrasted wi the common modes of proceeding in such cases.—We also entered upon the consideration of inflammation of the synovial membrane of the Hip joint, when we endeavoured to point out the indications of Hyperœmic action — of serous effusion and of the production of false membrane in the joint—and must now proceed to the consideration of the next stages of the disease.

We have shown that from the effusion of the exudation Corpuscles, we have the formation of false membrane, a variety of areola tissue, this is a marked indication of the law of analogous formations,

there is no doubt that the laws of Analagous formation, in the several structures of the body, are obscured with doubt and confusion.—It is clear however that the blastema, in which is produced or generated, all the variety of cell formations, both normal and abnormal, is a product of the blood, effused from its vessels—and the question to be decided appears to be, whether the white corpuscle of the blood escaping with the blastema, that circulates in each different structure, is the true exudation corpuscle—or if within the blastema escaped from each structure, we have a cell nucleus capable of generating its like in the new formation. The strict conformity of some morbid products, such as pus, which is the same in every variety of structure, would lead us to believe that the difference consisted in a change of the exudation corpuscle rather than the escape of these cell-nuclei, for if such was the case, these must exist in the blood of every individual structure as a primary element perceptible to the microscope—this is not consistent with the fact, for the blastema was but a few moments before, part of the Liquor Sanguinis passing to, and indiscriminately nourishing all the normal structures of the body without giving rise to any such formation as pus.

There is one fact which may in some degree serve to explain the difference of Pathologists respecting the formation of the pus-corpuscle from the white globules of the blood, is that these, with a good microscope may be seen to be of various sizes in the normal condition, hence in the Liquor puris, we should expect to find pus-corpuscles in different stages of development, consonant with this curious fact—and so we find them.

Suppose the disease should still progress, the arterial excitement already spread to the other textures of the joint—these now participate in the inflammatory action. If it has extended to the capsular ligament, the amount of pain is greatly increased, and its nature is considerably changed, it is the dull sickening ache of the fibrous tissues; could we observe the appearance of the ligament it would be seen of a pink colour, like the selerotic coat of the eye, for now its capillary vessels carry red blood. So also will the cartilages of the Joint participate in the Hyperœmic action, the vascular structure from which it receives nourishment, becomes distended with a denser fluid, that causes the fibrous portion of its structure to swell, fills its cells to their utmost extent, and may be one of the principal causes of the elongation of the thigh in acute inflammation of the Hip Joint. The inflammatory fever is now extreme, the pain also becomes insufferable, so that the least movement of the joint causes excruciating suffering, and the patient instinctively and rigidly maintains one position, he cannot bear even the least change of his pillow, even the rude walking

of people upon the floor, increases his irritability if not his actual torture, and he pertinaceously lies in all the filthiness of a sick bed. This condition of things is doubtless a provision of nature, to preserve the most profound rest to the joint, for in this case motion would do the greatest harm, it would increase the inflammatory action, and assist to develop the formation of matter in the joint, a point when it has happened, from which we shall have to date changes of the most formidable character, and which in our opinion must ever after be associated with lameness, and deformity.

The disease may now stop short of the actual development of the pus globule, as I have shown the plastic lymph may have taken on a healthy action, may have become organized, but the joint remains swelled, stiff, and attended with considerable lameness, which subsides but by slow degrees; should now however the presumptuous Quack, interfere with this process of nature (which I have known to occur) and dare to twist and turn the limb, under pretext of reducing luxation of the joint, I need not picture the dreadful intensity of the patient's suffering, or show the enormity of the act, which will in all probability hurry the disease to a fatal termination, whereby perpetual lameness and deformity, if not actual death is the result. This state and condition of the joint, is not unfrequently caused by falls upon the trochanter major, whereby the head of the bone is violently driven into the acetabulum—the delicate synovial apparatus of the joint is injured, and more or less inflammatory action of this structure is the result.

Should the progress of the disease continue, the plastic cytoblastema effused into the joint and surrounding tissues, begins to soften, the formation of the pus-corpuscle now happens, and matter rapidly accumulates in the joint. The advent of this period is generally marked by rigours of more or less severity which seize the patient in token of the alarm the constitution now feels, for the vast importance of this stage of the disease—the pus thus formed in the joint, is developed from the effused lymph which has been described as one of the results of congestive action, in the vessels of the synovial membrane, during a state of inflammation: this instead of becoming organized, and remaining a permanent false membrane in the joint, the effused plasma sooner or later begins to soften, and we observe corpuscles to be formed in the dissolved fibrine, these floating in the Liquor puris, are the pus-corpuscles.—The perfectly formed pus-corpuscles are cells containing one or more nuclei, sometimes even nucleoli. Thus we may observe a simple and apparently vesicular nucleus, placed excentrically in a transparent elastic, and round cell-wall; at a subsequent period the nucleus seems to have a granular,

amorphose precipitate around it, without a clear outer circumference; upon an attentive examination we can also observe in the fluid minute granules less than the 1000th of an inch in diameter, while larger corpuscles identical with the nuclei of pus-corpuscle are observable. The history of the process would lead to the belief that two or three of these nuclei may be grouped together, in all probability and to all appearance exudation corpuscles, these having taken on a required action and having a cell-wall developed around them at first pale and transparent, but subsequently becoming thickened opaque and covered with granules—hence the various structures visible by a microscope, observed floating in the Liquor Puris—the progress of this process in the development of the pus-corpuscles, is often extremely rapid, a few hours sufficing to exhibit a full grown corpuscle; as soon as the cell-wall is formed the corpuscle grows by endosmotic action, and after a time having ran through its course, it bursts, and liberates the granules, which are often all that can be found in the pus that has been evacuated from the body after several days.

The joint now soon becomes greatly distended with pus, so much so, that ulceration of the synovial membrane, and also of the Capsular Ligaments will take place, from the great distension of the part, when the patient experiences a temporary relief from the pain —by degrees the pus escaping among the muscles of the hip, burrows down in every direction, until it finds its way to the surface; and this may show itself near the groin, or on the back of the hip, while sometimes the abscess will be found to open a long way down the thigh. Coincident with the formation of matter in the joint, we find a change in the character of the constitutional irritation; up to this period the fever has been more or less of the inflammatory type, the pulse has been full and quick, the tongue white and loaded, the skin hot and dry, but now the shivering, which but too plainly marked the baneful change in the nature of the disease, is frequently repeated, it is followed by heat, and flushings, and is succeeded by profuse perspiration; the change indicative of this variety of constitutional irritation, returns with more or less constancy, while the pulse has an enduring frequency, is small and sharp; a gradual wasting of the body and a progressive debility of the whole frame, distinctly points out to us the nature of the change, which has happened in the character of the disease of the Hip-Joint.

The discharge from the joint is generally in the first instance normal puss (pus bonum et laudabile) a creamy looking, thick opaque, and homogenous fluid, having a faint yellowish, sometimes a white, or even greenish tinge, it has a peculiar smell when fresh, but looses it on standing, has a sweet mawkish taste, and is

specifically heavier than water—when first evacuated it has an alkaline reaction, but after standing, changes by degrees, so as to exhibit an acid condition; this character of the discharge continues for a longer or shorter period, in all probability dependent upon the amount and rapidity of softening, of the effused blastema, but by degrees the pus looses its normal character, it ceases to be thick and opaque, but becomes thin and transparent, often has an offensive smell, and not unfrequently appears peculiarly acrid and irritating. It now appears evident that a state of transition is progressing; the cells in the amorphose blastema, have become to a considerable extent exhausted, and now the structures of the joint itself are submitted to the dissolving influence of the discharge, are more or less destroyed, and by such means are removed from the system, so that perhaps destruction of the Synovial membrane has become general, ulceration of the cartilages to a considerable extent may have taken place, and the disease have progressed in the bony structure itself until we find that the neck of the thigh bone, and a very considerable part of the cotyloid cavity has been removed from the joint. Consequent upon this destruction and removal of these portions of the joint, we find a great change to occur, for instead of the head of the thigh bone, placed upon its long neck descending into the deep and firm cavity of the acetabulum, having so secure and strong a hold as almost to bid defiance to our attempts at removal, and that even after the tough capsular ligament has been entirely cut through, we find what remains of the head and neck of the bone, protruded from the now comparatively shallow cavity in the bones of the pelvis; in fact the very character of the joint has been changed by the disease, so that the natural action of the muscles which perform its several movements in a normal condition, are now able to produce a separation of the bones; all the natural continuity, between the femur and cotyloid cavity being dissolved, dislocation of the hip joint is the consequence. The direction in which this dislocated extremity of the thigh bone shall be placed, would seem to be dependent upon the position of the limb at the moment of this separation, if the patient lying upon his back in bed, should have flexed the knee, adducted the thigh, and have rotated the toe inwards, so as to have relieved the surface of the joint from all pressure in the first instance, and have subsequently maintained that position inflexibly, as soon as the disease shall have so far progressed that the joint shall cease to offer the natural impediments to the retraction of the thigh bone this will be drawn upwards by the action of the great muscles, and lodged upon dorsum of the Ilium; again the position of the patient may have become changed from the weariness of his posture, should he have turned upon his side, and thereby have abducted

the flexed thigh, the bone may be located in the thyroid hole; it may be placed in any position in which the action of the muscles shall be favoured by the position of the bone at the moment of separation; this may perhaps account for the striking varieties we find in the deformities dependent upon this disease.

This separation of the diseased surfaces of the joint, would appear to be a provision of nature, towards the cure of this complaint; the diseased structures now comparatively cease to be a source of mutual irritation, and the patient often dates the favourable changes in the diseased action, from this period of time. As soon as the dislocation of the thigh bone upon the dorsum of the Ilium has been produced, considerable shortening of the limb is the result, the knee is bent and the foot rotated inwards; that the amount of this inversion (which varies with the circumstances of each case) will depend upon the length of the neck of the thigh bone that remains attached to the shaft; if this be considerable the action of the rotator muscles of the hip arising from the pelvis, and inserted into the trochanter major, will bind the shaft firmly to the pelvis, while the extended neck preventing the rotatory action of these muscles, will be found to preserve the limb in the one position; but should the neck have been wholly removed by the diseased action, the rotatory movement of the shaft will be permitted; and we may even find a complete evertion of the foot, should the bone when removed from the colyloid cavity, have been placed in the thyroid hole, the limb will be somewhat lengthened, the thigh abducted and the toe turned outwards—As the patient gains his strength and assumes the erect posture, the change in the position of femur will cause an alteration in the line of the pelvis, and as a necessary consequence of this condition, a sigmoid flexure of the spine is the result —according to the amount of the inclination of the pelvis from its normal position, will be the amount of this curvature of the spine. In dislocation upwards the pelvis is inclined to the diseased side, to enable the shortened limb to rest upon the ground, the vertical condition of the vertebral column is deranged, and flexion in an opposite direction is the necessary result; no sooner has this been accomplished than the body is thrown too far on the opposite side, and to gain the true perpendicular, so that the head may rest perfectly on the top of the column, and be truly balanced in the centre of gravity, that another curve is necessary, but this is scarcely more than half the dimensions of the former —hence the characteristic sigmoid flexure of the spine. This condition has equally an effect, if the limb is lengthened by being placed in the thyroid hole, but exactly in the reversed direction.

I have already pointed out, that the formation of matter in

the joint, has been followed by ulceration of the capsular ligament, and the escape of the pus without the shut sack—that it burrows in many directions, undermining the parts about the joint, isolating the muscles from their connection with the bones, causing extensive disease of the bones of the pelvis, or having extended among the muscles of the hip it will destroy the fascia, and leave very extensive sinuses—moreover the disease may extend by ulceration, (especially in young subjects) through the bottom of the colyloid cavity, insinuate itself under the Iliac muscle within the pelvis, and has even been known to cause adhesions and disease of the large intestine in its immediate vicinity, so that the matter in the diseased hip-joint has been evacuated through the bowels.

This state of things may have continued for a longer or shorter period, often in young people the progress of this disease may be very rapid, may have caused intense constitutional irritation that was attended with violent delirium or continued hectic, accompanied with profuse sweating; may have so debilitated the patient that the powers of his constitution sink, and death closes the scene. But if on the separation of the diseased bones, or from some other favourable cause, the diseased action should take a more fortunate turn, the great purulent discharge begins to subside, the sinuses to heal up, and the patient's constitution to regain a degree of tone and elasticity, the harbinger of returning health. The bone now begins to be accustomed to its new situation among the muscles of the hip, exostosis occurs to a certain extent, often very considerable, it surrounds the extremity of the femur, and after a time accomplishes the formation of a new cavity, giving rise to the production of a new joint—in whatsoever part the femur shall be located, whether it be on the dorsum of the Ilium, the thyroid hole, or in any other situation. Sometimes the shaft of the thigh bone becomes firmly adherent in the new formation, and fixed in one position, often not the most advantageous, and although it is fully able to bear the weight of the body in its new position, it is not permited the least latitude of motion—it is an anchylosed joint. The Colyloid cavity also becomes completely filled with a new formation of bone, and the result of the healthy action, is the total subsidence of the disease, a cure, which under the most favourable circumstances however, is but an alternation between death and deformity.

Such is the course of the inflammation of the synovial membrane of the hip joint, that after a time has evidently extended to all the other structures, implicating them in changes of the most grave description; doubtless every case will exhibit a shade of difference either in the intensity of its symptoms, or the character

of its existing cause. In some cases preeminently acute, the symptoms will be extremely rapid, will evince all the characters of intense inflammatory action, and may arrive at a fatal termination in the short space of a week or ten days; but in the generality of cases the progress of the disease will be much more tardy. It may come on with scarcely an indication of its approach, by the sudden appearance of swelling of the joint attended with acute pain caused by any active exertion; it may have as suddenly subsided, without any permanent ill effects, to be reexcited however upon the application of any other exciting cause, or the advent of any inflammatory condition of the constitution, which will predispose to such diseases.

In the knee the swelling and effusion, which so rapidly occurs, in inflammation of the synovial membrane, is easily recognized, and truly forms a most characteristic feature in the complaint, although equally present in this disease when occurring in the hip-joint, but from the greater depth of the cotyloid cavity, is far less easily recognized, but even here it may be observed upon due and attentive consideration. It must however be confessed that the inflammatory action in this disease will extend so rapidly to the other structures of the joint, as speedily to obscure this distinctive symptom, as in its progress it involves the other structures of the joint; while itself becomes a frequent accompaniment of other diseases, developed during their progress, and this is especially the case, in inflammation of the Capsular Ligaments of the joint, on which it is a pretty constant attendant.

It is to be observed that the rapidity with which the symptoms of this disease generally progress, are the most distinctive characteristic of inflammation of the synovial membrane—the acute pain and rapid swelling, serve to mark the distinction from chronic inflammation of the cartilage, and that variety of irritation which proceeds from deposition of tubercular matter in the several structures of the hip-joint; while the character of the pain, and the constitutional pecularities, serve to distinguish it from inflammation of the Ligamenteous textures.

To be continued.

[Errata in last number, page 207, line 2, for pernation read hæmatin.]

Art. XXVIII.—*A Practical Treatise on the Art of making and Preserving Microscopical and other Preparations.* By Henry Goadby, M.D., F.L.S.

In the preserving fluids that I use, and which are known by my name, the following ingredients occur, viz.: rock salt, alum, corrosive sublimate, and the white oxyd of arsenic, or arsenious acid.

These materials are never all employed at one time, and they should be used judiciously, to prevent the contingency of destroying rather than preserving specimens of Natural History.

To this end, I think it desirable to describe the *properties* of the materials respectively, before giving the necessary formulæ for the fluids.

Rock (or bay) salt is very preservative, and will maintain the characteristics of all tissues unimpaired, better than any other agent with which I am acquainted, provided the *strength* be well regulated; and I make much greater use of the purely saline, or B fluid, than of any other.

Alum possesses very important conservative properties; it is astringent, coagulates albumen to some extent, rendering transparent tissues opake in proportion to the volume of alum brought in contact with them; but it *destroys the carbonate of lime,* converting it into the insoluble sulphate. The aluminous, or A, 2, fluid, however, is a very valuable composition; and to it I owe many important preparations, which may be found both in my own possession, and in the Hunterian Museum of the Royal College of Surgeons, of England, and which never could have been made without its assistance.

Alum *combines* with animal tissues so perfectly, that it cannot be dissolved out of them by long continued maceration in water. Whenever it is considered necessary to use the aluminous fluids either to give form, and support, to an animal, or any part of an animal, or a delicate tissue, by reason of its astringent property, or to render diaphanous animals or tissues opake enough to be visible, *the excess of alum* should be washed away with water, and the animal, or whatever it might be, with few exceptions, removed

from the aluminous, and preserved permanently in the B fluid. It should be constantly borne in mind that the effect of fresh volumnes of the aluminous fluid should be cautiously watched, lest the alum produce mischievous results; but with care it may even be used to the full extent of its valuable properties on the soft parts of an animal enclosed in a shell of carbonic of lime, or othwise possessing that earth, for the muscular, nervous, and other soft tissues, will be much sooner affected by the action of the alum than the denser tissues containing earthy matter. It will hence be seen that the aluminous fluid is not of universal application.

Corrosive sublimate is also astringent, and the coagulator of albumen; the intention of its application is not for the sake of either of these properties, but simply to prevent vegetation *growing in the fluids* respectively. But inasmuch as albumen takes from corrosive sublimate a portion of its chlorine, and thus converts it into calomel, and as all animal tissues are more or less albuminous, the propriety of using it at all, may well be questioned. In places where the sporules of fungi abound, as in the store-rooms of large museums, not even the presence of corrosive sublimate can prevent them from *growing upon the surface of either* of my preserving fluids, if they contain animal matter of any kind and are in *open* vessels, i. e., not hermetically sealed; but in a long experience of this fact, I am bound to say, although I have had open jars, dishes, and other vessels containing dissections of animals waiting their turn to be mounted permanently as preparations, in which the surface of the fluid has been covered during the summer months with vegetation of considerable substance, and which has continued to increase, and flourish magnificently for weeks, yet, I have never known it to descend into the fluid, or affect the dissections (provided they were well covered with fluid) in any way. Indeed either of these fluids will preserve plants, as easily, and certainly, as they preserve animals; and were the fungus to grow *into* the fluid, it would die, and be preserved, neither have I at any time, during fourteen years experience of preparations made by the use of my fluids, and contained, and sealed down, in the several forms of vessels and cells, also of my invention, ever found a particle of vegetation in a single preparation; and during the last six years I have

been using the fluids, both for permanent preparations, and stores, without the addition of corrosive sublimate, and always with satisfactory results. I believe, therefore, that the corrosive sublimate may be safely left out, although I shall include it in the receipts of the fluids.

Arsenic possesses the power of softening animal tissues to a remarkable extent, and this property has no limit.

A few years ago I was desired by the Examiners in Anatomy, of the University of London, to preserve a body during the summer season for their examinations in the autumn. Desiring to retain the tissues severally in as natural a state as possible, I added arsenic to the B fluid. For some months nothing could exceed the success of this experiment, and if I had changed the fluid and substituted B fluid without arsenic, I believe the body would have been permanently preserved. It was neglected however, in this respect, although I watched it with some solicitude until, after the lapse of rather more than twelve months, I found the entire body (with the exception of the bones) reduced to the condition and appearance of decomposing size, except that it remained perfectly sweet. I have made a number of experiments with the like results. I have seen the characters of muscle, tendon, nerve, &c., gradually disappearing, until nothing but a glairy fluid remained, but was always perfectly sweet. As arsenic acts upon glass, and glass vessels, by combining with the lead, and for the above reasons, it cannot be employed for preparations that are desired to be permanent. I have made a few such attempts, but they have all ended in failure.

The softening property is that for which I employ arsenic: either to recover animals that have been hardened, and corrugated in alcohol, or to enable me to proceed with elaborate dissections of nerves which must necessarily be tedious. My friend Dr. T. S. Beck of London could never have made such a display of the nerves of the uterus—the finest dissection of nerves in the human subject that I believe has ever been made—without the aid of arsenic, which was never allowed to do any mischief, occasionally washed away, then renewed, and so on; and the nerves, under its well regulated influence were as tough as copper wire,

and although very delicate in appearance, would bear pulling and stretching with impunity.

The alluminous fluids I originally designated by the letter A, and I called them 1, or 2, as the same weight of the ingredients were dissolved, either in one quart of water or two quarts; they are thus made.

A, 2

Rock salt, ...	4 ounces.
Alum, ...	2 ounces.
Corrosive sublimate, . . .	4 grains.
Boiling water, ...	2 quarts.*

The A 1 only differs from the above in having half the quantity of corrosive sublimate, and water. It is very rarely used, being generally too astringent.

B

Rock salt, ...	8 ounces.
Corrosive sublimate, . . .	2 grains.
Boiling water, ...	1 quart.

The corrosive sublimate must never exceed, under any circumstances, two grains per quart of water; otherwise there will be in time a white precipitate on the preparation that cannot be removed, and which will greatly disfigure it.

When the B fluid is made according to the above receipt, its specific gravity at a temperature of 60° will be 1,100, and with it, terrestial and fresh water-animals can be well preserved. In cases where it is desired to preserve the *redness* of muscle, it is only necessary to add the nitrate of potash to the preserving fluid: about half an ounce per quart of fluid.

For marine animals the strength of the fluid must be increased by the addition of salt to 1.148, otherwise they will be decomposed. A great number of marine animals in the first stages of the preserving process require alum, but it must be cautiously used, and carefully watched, and as soon as it has done all that is required of it, the animal should be well washed in clean water and placed in the B fluid. There is no objection to frequent contact with alum, if necessary, provided the process be conducted on the principle here laid down.

* The *imperial quart* of 40 ounces is intended to be understood throughout the paper, and the weight, avoirdupoise.

The Arsenical Fluid.—When I employ arsenic for its *softening* properties, I use it alone, unless the process is likely to occupy much time, and in that case, I combine it with the B fluid, in the following proportions, and call it C : B fluid, as directed above, arsenic 20 grains. Arsenic can no more be trusted with carbonate of lime, than alum; and if it be desired to employ it on any molluscous animal the creature should be removed from its shell prior to its introduction to arsenic. The solutions of arsenic that I have employed differ in strength from 20 grains to 60 grains to a pint of water, (imperial measure, 20 ounces) or to the pint of B fluid. It is not easy to dissolve this mineral, and the only plan which I have found successful, is to place the quantity of arsenic to be dissolved in a Florence oil flask with half a pint of water, apply a spirit lamp, and boil till the whole be dissolved, it can then be diluted by the addition, either of more water, or preserving fluid. I may mention one singular fact of preservation by this fluid, no less of the animal, than (which is most important) its color.

Upwards of six years ago my Son collected for me several specimens of the larvæ of Cossus Ligniperda, the peculiar color of which had never been preserved. In alcoholic fluid, of any strength, it turns quite black, which is a common result of the application of spirit for preserving caterpillars; in addition, most insect colors are soluble in alcohol.

The specimens included larvæ of the first and second year, and one fine sample of a three-year larva about to turn to a chrysalis. Of the former specimens I preserved some in the A 2, and the rest in the B fluid, and placed the last in a solution of arsenic. The aluminous fluid has hardened and disfigured the caterpillars nearly as much as spirit would have done; they are softer, and in better state for dissecting, in the B fluid; but they have lost all their rosy redness of color in both fluids, and are partially black.

It was reserved for the arsenic to give me one caterpillar so beautifully preserved that all its characteristic color, even to the most delicate tint, is maintained to this time. I *believe* that the interior has not been destroyed by the softening tendency of arsenic

before alluded to, because, if so, I think there would be considerable deposit in the fluid, which has not occurred; neither in that case would the insect retain its roundness, and fullness, but on the contrary become flaccid by the removal of those tissues (muscles) that give form to the integument. As this caterpillar had been secluded from the operation of light (the fruitful agent for destroying color in animals) for more than twelve months, I determined to try the effect of constant exposure, to which I submitted it for three years in England, and for six months in this country; its beauty is still unimpaired. As it was a sole specimen, and I am not likely to obtain another, I am unwilling to dissect it.

I have been particular in speaking of the successful application of arsenic in the preservation of *color* in this caterpillar, because I believe it is of some importance. It is most interesting to collect the larvæ of Lepidopterous and other insects, as far as possible, but they lose much value for the purpose of instruction and for collections, unless their color can be permanently preserved; and I have great hopes that the fluid which has proved so eminently successful in the instance of the caterpillar of the goatmoth, which takes on the described blackness a very few days after death, in every other perserving fluid, may be equally efficacious in the preservation of color, in the other species.

Mode of using the Preserving Fluids.—A knowledge of the proper method of using these fluids is essential to success, for in other hands than my own, they have led to the destruction rather than the preservation of specimens. Men have constantly treated my preserving fluids as though they were using spirit, entirely overlooking one very important consideration, namely, the vast difference between their specific gravity and that of alcohol. In the latter, we have a fluid so light that every animal is *heavier*, and will instantly sink in it; the conditions are exactly reversed in the former case, where every animal, from an animalcule to an elephant, is lighter, and will float upon either of them.

Neither of my fluids (always excepting the arsenical) *can be employed of full strength in the first instance*, and anything to be preserved in them should undergo previous maceration in clean

cold water, to which, after a time, preserving fluid may be added until the animal rises to the surface. The fluid and the water must be intimately mixed mechanically, or the water will rise through the denser fluid, and retain its integrity for a long time.

The animal will insensibly absorb, and become saturated with the *ingredients* of the preserving fluid, but in a state of considerable dilution; the strength of the fluid must now be gradually increased, and intimately mixed, until the animal again rise, and in time it will become saturated with this denser fluid. In many instances, it is advisable to keep the animal forcibly at the bottom of the vessel in which it is being preserved *by weights*, and this is particularly necessary in hot weather when the preserving process should be expedited with all the speed that is consistent with safety. It is easy to ascertain if the animal be saturated with the fluid by removing the weights, and in that case, to go on increasing the strength of the fluid: in fine, success depends on carying out the laws of endosmose and exosmose. The diluted fluid used in the first stages should be thrown away, and frequently renewed, as, being replete with animal fluids, it contains within itself the elements of decomposition, and increases the difficulty of obtaining success. This remark, however, applies less to the aluminous, than to the B fluid, as in the former, the coloring matter, and animal deposits of all kinds so abundantly seen when spirit is used and which occasions the steady and constant discoloration of that fluid, for, in some cases, many years, and which so generally tends to the disfigurement of preparations in museums is altogether *insoluble*, from the instant it or they come in contact with alum; and for this very important reason alum may be almost *always* used in the early stage of preservation, the former cautions on this subject being strictly attended to. Preservation of animals by means of these fluids, then, can only be accompiished by the daily addition of fluid until the creature be saturated with the fluid of the full strength indicated. Nor should the solicitude of the operator end immediately at this point, as it will be necessary occasionally to renew the fluid and often to test its strength. To shorten this species of labor, I long ago procured a glass jar, or tube, two and a half inches long, and three-fourths of an inch wide, with a flat bottom, to be used as a proof-glass; I then adjusted some speci-

fic gravity bubbles so accurately that they *rise very slowly* in the fluid, the precise strength of which they are intended to indicate; if the fluid be weaker than exact strength, they fall to the bottom, and there remain; if stronger, they quickly rise. They are marked on the top A 1, A 2, B, S, the latter indicating a *saturated* solution of rock salt with which it is convenient to increase the daily strength of the B fluid in the manner already described.

By pouring a little fluid into the small proof glass, and applying a bubble as the test of strength of the fluid that has been employed, the operator will instantly learn, not the exact strength, (which is unnecessary,) but that the fluid is either *the* strength, or weaker or stronger; all the information he needs to guide him in his labors.

Instead of the bubble marked "B" I would substitute two, one indicating 1.100 the other 1.148 and the Italian barometer makers could easily graduate such bubbles. The whole apparatus is enclosed in a japanned tin box 1 inch deep, 1¾ wide, and 2⅞ths long, which can be carried in the waiscoat pocket, and costs but little.

When either of the foregoing fluids are required for the display of preparations in a public or private collection, they should be *well filtered*, and for this purpose they may be passed a great number of times through fine flannel rammed into the nozzle of a large earthen funnel, or once through a filtering machine, or twice or thrice through good filter paper. If the filtration be properly performed, these fluids are remarkably bright, white, and brilliant, far exceeding in this respect any alcoholic fluids. Rough filtration may be satisfactorily effected by once passing through the thick flannel used for a jelly bag; but if this be not at hand, it is only necessary to allow the fluids to stand quietly in the vessel in which they were made until quite cold, then carefully pouring off the top, the extraneous matter always found in rock salt will gravitate, especially in the aluminous fluid, which has the property of throwing down any thing which disturbes the transparency of water. Neither of my fluids can be retained in open vessels, glass jars, or even stoppered bottles, for any length of time, without additional protection,

In *open* vessels, the water evaporates, and the salt crystallizes to the total destruction of the specimens included. Salt being highly deliquescent, the volume exposed to atmospheric influence (the upper portion) becomes more or less diluted when the atmosphere contains moisture, and ascends into the neck of the bottle, even around a well ground stopper, by capillary attraction; it gains the upper surface of the stopper and then descends the sides of the bottle, and will lie as a pool on the shelf on which the bottle stands.

As the weather changes, and becomes dry, the salt crystallizes, and thus forms a conduit for the fluid the next rainy day, by which it can greatly, and readily, extend its outposts; and by this means, in time, it will pass completely out of the bottle or other vessel. Bladder will not confine it, applied to a glass jar on the plan employed for spirit preparations; and the only plan is to cover the jar with a plate of *flat* glass (patent British plate manufactured by Messrs. Chance of Burmingham, is the best) and seal it down with the patent marine glue, applied to the glass, with a hot iron.

The best, neatest, and readiest mode, in my experience is the plan of my invention, namely: first place in the upper vessel of a small copper glue pot some marine glue cut small; in the lower vessel, where the carpenter would put water, for the careful dissolution of animal glue, put linseed oil, and then apply heat; the temperature of the boiling oil will dissolve the glue the first, second, and even a third time, with care; after this it becomes altered in its proportion, and refractory.

The dissolved glue should be rapidly aplied to the rim of the glass jar (which must be quite dry and free from grease,) with a brush, and the only brush that will stand, I make in this way. I take a piece of rattan cane as long as a cedar drawing pencil, and cut off the cortex carefully from one end of it to the length I desire the brush to to, being particular not to let the knife go into the substance of the cane any more than I can help. I macerate the prepared end of the cane for a short time in water, and then, while yet wet, I pound it with a hammer upon some hard substance (iron or stone) constantly turning it with my hand until all the fibres of the cane be liberated, and my brush

is complete. I still use a brush of this kind which I have employed for several years extensively, and none other will stand twice using, the hairs come out with the glue, and are in the way of a good joint. A disc of glass should be cut to fit the top of the jar, made clean, and the part that is to be in contact with the jar also finely coated with the hot glue. The disc should previously have had a small hole drilled through the centre, (about one-eight of an inch in diameter) for a reason that will presently appear.

The two surfaces of glass being *apparently* coated with marine glue, but *really without contact*, the latter must be insured by means of a hot iron which should be carefully passed over the surface of the glue several times till it and the glass become hot, care being taken to keep the iron constantly in motion, and always on the edge of the jar, or of the disc, as in that case the expansion will be equal, and no danger occur even if the iron be *red hot;* but, it will instantly break if the iron be allowed to linger in one place, or touch any but the outer portion of the disc, or rim of the jar.

The jar should be thus prepared while *empty, and dry,* and when complete, the fluid may be poured in, to about one-half the height of the jar, together with the preparation to be suspended in it. The strings necessary for this purpose may be brought over the edge of the jar, and pressed into the glue on the surface, if soft enough to admit of it; the preparation may now be regulated to the required height in the jar, and the threads of suspension kept in their place by a wet string passed round them on the outside of the jar, several times, and tied. If any fluid chance to be on the surface of the marine glue on the rim of the jar, it should be removed; and when dry, the prepared surface of the disc should be placed on the jar and the two brought together in intimate contact by the hot iron, which as in the former case, must be constantly passed round on the edge, and the disc simultaneously pressed down, until the process be completed. The extraneous glue on the outer edge may be made smooth and neat, by the hot iron.

By means of a syringe, to which a small pipe is affixed, fill up the jar with the preserving fluid, not quite full, however, as

the great expansion of the fluid, (the B. especially) in sudden increase of temperature, may cause the breakage of the top glass; then cut a cork to fit the small hole tightly, insert it, pare it off level with the surface, place upon it a piece of solid marine glue made to adhere to the cork by means of the point of the hot iron, and cover it with another disc of glass about the size of a ten cent piece, or an English six-pence, and the preparation is finished.

It is a good practice to prepare the portion of thread that is to come outside of the jar, the cork, and the surfaces of glass to be coated, with a solution of the marine glue, which may be made by dissolving a piece of glue in an excess of white-wood Naphtha.

Should a stopper become fixed in the neck of a bottle by the crystallization of the salt, it may be easily removed by dissolving the salt by water, and gently tapping the cross piece of the stopper at its extreme ends, (*never across its shorter diameter,*) with a door key. If the cross piece come off, make it and the remainder of the stopper that is in the neck of the bottle hot with the iron, apply marine glue, and cement them together,—when cold, renew your operations,—the stopper is stronger now than before, and will easily come out, and last longer than one not broken. To keep the fluids in stoppered bottles, and to prevent the possibility of the salt crystallizing on the outside of the stopper, the marine glue may be advantageously employed; or a cement, proposed by Prof. Olmsted, of Yale College, and made by melting resin and lard together by the application of heat, and intimately mixing them. The respective quantities of the materials will depend on whether the cement is required to become hard, or not. If the former, the resin must be in excess; if the latter, use more lard. For the purpose that I indicate above, it should be *stiff* and *ropy;* remaining just soft enough in hot weather to spread with a pallette knife.

As a final remark I would say, that the preservation of animals, either in alcoholic, or any fluids, is greatly facilitated by employing, in the first stages of the process, a large volume of fluid.

Crowding animals together in a limited space, and with only a small quantity of fluid, is a fruitful source of injury and loss of the majority, if not of all the specimens; when, however, the preservation is completely effected, the specimens may be packed very closely together, in a small vessel, and as much fluid of the required strength as will occupy the interstices is amply sufficient for transportation, or stores, and will last for years, especially if the fluid be kept in, by running some marine glue round the stopper and neck of the bottle with a hot iron, or by using the resinous cement.

INSTRUCTIONS FOR MAKING WET PREPARATIONS OF ANIMAL SUBSTANCES.

It frequently happens to the Naturalist, and the Microscopic observer, to meet with animals, or tissues, which, from a variety of circumstances, cannot be retained in any other form than that of a *permanent preparation*. They may be small, and so delicate, that they would be entirely lost if put into a bottle; and in such a case, it is desirable to mount them, without delay, as preparations for the microscope.

If the object be merely a filmy tissue, take a piece of glass of good quality, good surface, and flat; the substance is not material.* Clean it with liquor potassæ or dilute sulphuric acid, or use both these fluids, mixing them on the glass; they effervesce, decompose each other, and at that moment, clean the glass; rinse it in clean soft water and dry it with either a clean muslin handkerchief, or a piece of chamois leather; now test it with a drop of water placed on the centre of one side of the glass, and if the water can diffuse itself *evenly over the whole surface, the glass is clean;* if not, it must be made so.†

This, which is frequently the most difficult part of the whole process, being accomplished, place the glass in the vessel in which the tissue to be mounted lies in preserving fluid, and float it on to the glass; withdraw the latter carefully from the vessel.

* The best glass for this purpose is the "patent British plate," manufactured by Messrs. Chance at Birmingham.

† It sometimes happens that neither acid, nor potash can clean a piece of glass sufficiently well to enable it to endure the test proposed; in such a case watery solution of gum arabic may be used, or what is still better—the human saliva will clean it instantly.

With a fine (needle) point adjust the tissue to the centre of the glass, and soak up the excess of fluid with a camel's hair pencil, leaving enough to cover the preparation. Now take a piece of *thin* glass, such as is used by microscopists, previously cut of less width than the slide or glass on which the tissue lies, and having cleaned it by the mode described, hold it at one end by a pair of finely pointed forceps, and apply the outer extremity, holding it almost vertically, to such portion of the other glass as to leave the preparation in the centre of both.

Gradually lower the top glass, and the fluid will run before it until the preparation be covered, and the top glass finally rests upon the lower one.

A quantity of fluid will yet remain outside the top glass which must be carefully taken up with the camel's hair pencil until the surface of the lower glass, around the top one, be made quite dry, when the following cement must be applied to the clean, dry glasses, to shut in the fluid, and render the preparation permanent.

Take Egyptian asphaltum and dissolve it in camphene to the consistence of a thick paste; this process is greatly facilitated by the application of moderate heat. Keep it in a well secured vessel, and label it. Then take *japanner's gold size*, which may be obtained at the varnish makers, but generally it is too thin, because new. Inspissate it by the continued application of heat until it acquire the consistence of molasses, then with a muller, upon a marble slab, grind up with the gold-size as much lamp-black as you can, until you have formed a *very stiff paste;* this should also be well secured and labelled. The properties of these ingredients are as follows:

Asphaltum is hard and brittle.

Gold-size is highly tough, and elastic, and retains these properties for many years. By combining elements respectively *too hard*, and *too* soft, the one is made to counteract the objectionable properties of the other, and the lamp-black not only assists to give good consistence to the whole, but is desirable from its indestructibility.

Japanner's gold-size is composed of boiled linseed oil, dry red lead, litharge, copperas, gum animi, and turpentine.

To use the cement, take nearly equal parts of each of the above materials, taking care that the gold-size composition should rather preponderate over the asphaltum, than the contrary; mix them intimately on a slab with a small palette knife; if too thick to work well, add a few drops of camphene, *but beware of making it too thin.* Apply the cement, thus made, with camel's hair pencil to the outer margin of the top glass; do not use too much for the first coat, but rather by successive layers, applied at different periods, fill with cement the space between the lower and upper glasses of the preparation, until a good solid layer be formed, when the process is complete. It is, however, most important *to isolate the several layers of the "black" cement,* for the turpentine contained in a newly applied coating will act upon, and partially dissolve, the old and dry layer; in this case, the upper surface being exposed to the atmosphere will speedily *dry* and *contract,* and acting upon the softened cement below the surface, will drive it between the glasses, and spoil the preparation.

Either of the following compositions may be used for the purpose of separating the layers of the black cement.

Gum arabic, ...	3 drams.
Sugar	1 do.
Corrosive sublimate, ...	1 grain.
Water, sufficient to make a thick mucilage.	

Marine glue, dissolved in an excess of white-wood naphtha, to form a thin solution of the glue. This, which is by far the best application for the purpose, dries nearly as rapidly as it can be used.

Having devoted upwards of thirty years of my life to the dissection of small animals by the aid of the microscope, and in the preparation of the elementary tissues of all animals, from man downwards, and being desirous of preserving and making permanent the results of my (frequently) very tedious labors, my wants, in this respect, were necessarily peculiar. The ordinary form of vessel, then, (and now,) in common use—a bottle, was altogether unsuited to my especial necessity; I could not place a *bottle* under the microscope for the examination of its contents, nor see the preparations without the microscope, the aberration,

resulting from the figure of the bottle, precluding the possibility of defining with precision, the preparations contained within. Thus, the work I had been able to accomplish by suitable optical assistance could not be rendered apparent to my friends, by the use of a microscope; and whether it were an exposition of the nervous system, or other organic structure of an insect, or a minutely injected tissue of a frog, or a man, they were alike inaccessible to unassisted vision; moreover, to increase my difficulties they required to be kept as *wet* preparations. Having been in the constant habit of dissecting under water, in tin pans of various forms and sizes, and always covering these pans with a plate of glass to keep out dust, &c., when they contained unfinished dissections, or an animal simply prepared for dissection, I was struck with the beautiful appearance of an insect, or other entire animal, lying as naturally as possible, with all its full proportions displayed, retaining its characters in their utmost integrity, and so arranged as to be easy of access to the most superficial observer. To my vision, there could not be a more charming sight, than a finished dissection of the nervous system in situ of any insect, especially of the Blatta Americana—one of which I dissected at ten years of age—while lying in the pan in which the dissection had been performed; and sorely have I grieved at the sadly changed appearance of the same insect, at the instant I placed it in a bottle containing alcoholic fluid ostensibly *to preserve* it, but actually to complete its disfigurement. Neither could I suspend a delicate preparation in a bottle, in such a manner as to insure its safety. With a quantity of air always contained in the bottle, *the fluid* is put in motion by the act of taking up the vessel to examine its contents, and the particles of fluid beating against a delicate tissue will inevitably in time break or displace the structure that had cost the patient labor of many tedious hours to dissect and display. Thus, by my own act, not unfrequently, and by the carelessness of others, I was continually losing my preparations; and this determined me to attempt a form of vessel that should agree, as far as possible in all general particulars, with the pans, in which, I used then, and still continue, to dissect. Believing *glass* to be the very best material for my purpose, I consulted several operative glass-grinders on the subject; who all declared the work I required could not be done, and that if it

could be accomplished, the cost would prove prohibitory. Not to be diverted from my purpose, nor discouraged by the statements of the glass-grinders, I determined to try and work out my plans with my own hands, although I had not received education in any branch of mechanics. Moreover, in connection with my project as a whole, I required a good cement for the glass vessels, and some other preserving fluid than alcohol. These subjects occupied me more or less for twenty years, during which time the failures were frequently quite disheartening, chiefly as regarded the *mechanical* part. On one occasion, I possessed about three dozen of glass vessels, each full of fluid, hermetically sealed, and containing a minute dissection, which had remained permanent for a period of two years. A gas microscope had just been invented, and was then on exhibition in Bond street, London. In an evil hour I submitted my preparations to this instrument; the intense heat of the gases melted my cement, and all my treasured dissections were destroyed before my face—this occurred about eighteen years ago. In the years 1839–40 and 41, I worked most perseveringly at my glass cells, and vessels, with a view, either to complete the plan, or to give it up: at the latter end of '41 I possessed a large collection of preparations all of them contained in vessels similar to those I now use and intend to describe. I submitted them to the inspection of the Society of Arts who, having invited the assistance of a large number of eminent men, awarded me their large gold medal "for his method of putting up anatomical preparations." The medal was awarded in November, '41, and presented on the distribution day in '42.* I have felt it necessary in my own justification, to give this history of a plan of mounting zoological or anatomical preparations, now in very extensive use, as I observe the method is recommended and explained in a recent publication without giving me the credit of originating and perfecting it.†

* The preparations here alluded to were subsequently purchased from me for £500 sterling, or $2500, by private subscription, headed by H. R. H. Prince Albert, and presented by the subscribers to the Hunterian Collection, in the Royal College of Surgeons, where they now remain.

They were also rewarded by the late Sir Robert Peel, at that time First Lord of the Treasury, who presented me with a check, on the Royal bounty fund, for £150—$750.

† Since the above was written, a second addition has been published of the book alluded to, and the author, Mr. J. T. Quekett, has therein acknowledged my claim as the sole inventor of the plan.

There are many objects for the microscope, of great zoological or physiological interest, which possess more substance than will allow of their being treated in the way already described, although their characters can be preserved only as *wet* preparations; for all such, a *cell*, or a glass box, must now be prepared, and the following is the way to proceed.

Firstly, accurately measure the length, breadth, and substance of the preparation to be mounted; select a piece of flat glass of substance agreeing as nearly as possible with the thickness of the preparation and with a glazier's diamond cut off two pieces from one-eighth to three-sixteenths of an inch wide, and of equal length; these are to form the sides of the cell; the ends must be of the same width but not so long. Although the cell should *fit* the preparation in regard to *depth* or thickness, a good space should always be allowed around the sides and ends, for example: I desire to make a cell for a preparation measuring one inch long, and five-eighths wide, I should make the cell one and a half inch long, and one inch wide, *inside* measure; when finished the preparation looks better, is more accessible to the microscope because the sides of the vessel are not in the way, and, what is most important, *there is more room for preserving fluid*, than if the vessel be contracted to the actual size or thereabouts of its contents. The *depth* should be exact for two reasons: one, that thereby the object is retained in the center of the cell, being lightly pressed upon by the top, and bottom glasses; the other, that there being no greater substance of fluid between the object and the microscope than must needs be, a better definition of the object is obtained.

When glass is cut with a diamond it always leaves a rugged, uneven surface; for example, when broken off, one piece of glass will present a series of projections, which have left corresponding cavities in the piece to which it was attached; when placed together, they lock into each other and the addition of a thin layer of cement will form a perfect joint.

I avail myself of this fact in constructing cells of the kind just described, thus: fig. 1 represents a piece of glass of the exact length and breadth, *outside measure*, that the cell is required to be.

The two long pieces, or sides, are first cut, and before breaking them off they are marked with the scratch diamond so

as to include the ends. As the width of the cell is not always sufficient to admit a number of lines, I first make a diagonal mark, then 1 and 2—rarely 3, which is unnecessary. I now separate the pieces, discard number 5, and take care to cement them to the bottom glass or slide, in the order in which they are marked, and to insure accuracy in this respect, keep the marked surface upward. As it is necessary to have a bottom glass before we can cement the pieces just cut and marked, I proceed to give some.

DESCRIPTION OF THE SLIDES.

My peculiar wants have necessitated slides of larger size than that proposed for general adoption by the Microscopical Society of London; moreover, I had a collection of uniform preparations on slides of my size, long before that Society had existence. The slide I chiefly use measures when cut $3\frac{3}{4}$ inches, by $1\frac{3}{8}$ths: the glass should be the "patent British plate," before referred to, which being ground and polished on both sides is generally very flat: its substance varies from less than $\frac{1}{72}$th to $\frac{1}{8}$th of an inch.

Cutting Board.—To cut the slide expeditiously and uniformly it is necessary to have a *cutting board*, fig. 2. It consists of a mahogany board 11 inches by $9\frac{1}{2}$, half an inch thick and recentangular in shape; on one of its long sides, *a*, is fastened by means of pegs or screws and glue another piece of mahogany, the *guide board*, *b*, $2\frac{1}{2}$ inches wide and $\frac{1}{4}$ thick; this must be planed so as to be true, as the front is to form a *straight edge*. By reference to the figure it will be seen that spaces have been cut out of the guide board, the use of which will presently appear. A flat rule or gauge should be made of mahogany, 11 inches long and $\frac{1}{4}$ thick, the width to be ascertained as follows: mark out in card board a patern of the slide intended to be used, apply the glazier's diamond to a line indicating one side of the pattern and accurately measure the distance between the diamond and the other side which will give the required width of the gauge. In other words, the gauge must be of the width of the pattern, less *the* "*rake*," (or setting) of the diamond. In addition to gauges, a *square* is essential; the most useful is of mahogany, one-fourth of an inch thick, with sides $6\frac{1}{2}$ inches long and solid, i.e., not open.

The glass intended to be cut into slides should be placed on the cutting board, and if none of its sides have a true edge a narrow slip must be cut off its entire length to form one. The straight side of the glass must now be brought against the guide board to ascertaing if either of the sides, at right angles to the cut side be perfectly square with it; if not it is only necessary to square one side: for this purpose place the side to be squared so that it project a little beyond that part of the guide board which is cut away at *c*, apply the square, and cut of a narrow slip in in a direction contrary to the former cut: thus the two sides of the glass are made true.

Keep the glass still against the guide board and removing the square, apply the *gauge;* cut the whole length of the glass and you have the width of the slides. Now turn the squared end of the glass just cut into the space at *c*, pressing it firmly against the angles of the guide board, (which must also be made quite true,) place the gauge against the guide board in its former position, cut the glass transversely, as shewn by the dotted line, and you have the length of the slide; and in this way cut up the remainder of the slip of glass as far it will yield slides of the proper length.

In like manner the spaces *d* and *e*, in the guide board, give the length of other slides, the width of which has been cut previously with other gauges adapted to the purpose. By this arrangement of the cutting board one gauge is alone required to cut the width and length of a slide of any given dimensions. From the forgoing description it will be obvious that the gauges must be first made, the length of the spaces in the guide board determined by their assistance, and they must be cut in it before it be affixed to the cutting board.

Grinding the Glass.—Unless the slides are to be covered with paper, the sharp, rough edges left by the diamond cut should be removed by *grinding the glass.* This can be accomplished on a perfectly flat stone of sharp grit with water; the process is greatly facilitated by the addition of emery, but a better tool, in my experience, is a plate of *soft pewter*, or the emery plate.

My pewter plate was formed in a mould made for casting the

plates on which to engrave music; its outside measure, therefore, corresponds to the size of a printed page of music, but it is ¾ of an inch thick, and weighs 14lbs. It is important that the surfaces be made quite flat, and every care should be taken to keep them so. *Soft* pewter is desirable because it contains a much greater quantity of lead than the hard, in which tin preponderates. The metal is used only as the vehicle of the cutting material, which is emery. The latter, in time, becomes thoroughly impacted in the metal, so that it will cut with the assistance of water alone, and the wear of the plate is too trifling to be estimated. When in use it should always be charged well with "superfine" emery, and water; coarser tears the glass.

In the year 1851, during a long residence in Albany, N.Y., I was enabled, with the kind assistance of my esteemed friend John E. Gavit, Esq., Bank Note Engraver of that City, to carry out a plan that had long occupied my mind in relation to forming a less weighty, and more efficient tool for grinding glass.

Constantly travelling from city to city, the *weight* of my pewter plate, together with several pounds of Emery, was a very serious task upon me, whilst the want of a fixed residence prevented me from instituting any experiments whereby I might be relieved from the incumbrance alluded to. My desire was to convert emery, by the addition of another substance, into a hard, solid, compact cake of any required dimensions, and for this purpose I believed shell-lac to be the best material. My friend caught the idea and proffered the assistance of himself and his work-shops for the purpose of seeing what could be done. Our first experiment was successful, but clearly indicating that great improvements could yet be made. We used the ingredients respectively in various proportions; submitted the plates to different degrees of pressure, and lastly, we tried emery of different degrees of fineness. Without recording our comparative failures, it is enough to say that plates composed of emery, nearly 16lbs; shell-lac, somewhat less than 2lbs; subjected to a pressure of 5 tons, have answered the best. We have used in these plates with the greatest success, Flour Emery, number O, and Superfine.

The latter cuts very fast. Of course, the emery and shell-lac can only be brought into contact by means of *heat*, and great

care is necessary in melting the shell-lac, for, if the temperature be too high, or too suddenly applied, the shell-lac becomes like a mass of liver (decomposed), and you can do nothing with it.

Neither is it the easiest thing imaginable to knead so small a quantity of shell-lac with the large quantity of emery already stated. Success, however, depends upon this operation being well performed.

Subjected to *moderate* pressure, the plate will cut glass *incredibly fast*, but at the expense of the plate which will wear nearly through in a few hours.

The plates made under a pressure of 5 tons (in my occupation) have been used constantly and severely for 14 months without any sensible diminution. My desire to submit these plates to the severe test of time and use, induced me in this, as on many previous occasions to delay publishing any account of them, notwithstanding I once more laid myself open to the piracies of unprincipled and unscrupulous men.

These plates are round, some of mine as much as 10 inches in diamater, and from $\frac{1}{16}$ths to $\frac{3}{8}$ths thick. Although the plates were pressed between *two level metallic surfaces*, they yet require grinding to make them true; this is easily accomplished by means of superfine emery and plenty of water, and as in the course of time and use this operation requires to be repeated, it is necessary to have two such plates, and as each plate possesses *two* surfaces, it is easy to obtain truth by grinding the sides interchangeably.

To use these plates for grinding, it is only necessary to keep the surfaces well wetted with water.

Unlike the pewter plate, the emery plate can be advantageously employed for grinding sections of bone, teeth, spines of echini, the selerogenous tissues of plants or fossils: in the former instrument the loose, sharp cutting emery becomes impacted in the tissues *and never can be removed*, but in the latter the emery is too tightly held by the shell-lac and cannot get into any tissue. The only sensible wear is, not of the *emery*, but in the shell-lac; and thus, occasionally, the plate will

cease to cut, and the surface will be covered with a gummy something which keeps the glass or other material from actual contact with the emery—this is shell-lac.

If you now wash the surface of the plate evenly with alcohol, or Liquor Potassæ which is better, the shell-lac will be dissolved, and this should be well washed off with a copious stream of water—the well cleaned teeth of the emery will now cut sharper than ever.

When my friend had acquired, what the Yankees call "the hang of it" he made a batch of these plates (some 50 or 60) believing them to be important in many mechanical operations no less than to supply his own wants; some of these he intended to sell, and for all that I know to the contrary, they may yet be procured from him by those persons who have not the tools for making them, but would like to possess such appliances.

Hold the glass slide to be ground at an angle of about 45°, that *the outer line of the edge* may alone touch the plate of metal: grind by a quick, light, circular motion—to and from, round the corners—until the line be straight and beveled; change the position of the glass to grind the *opposite* outer line in the same manner; now hold the glass vertically, and make the edge smooth. By beveling the outsides of the edge of glass in the first instance they are saved from breaking, which is inevitable without this precaution; it is true that beveling can be done at any time, but it is not easy to grind out the deep irregular holes caused by splitting the edge. In this way, the four sides of the slide are to be ground.

To keep the pewter plate, or emery plate flat, grinding should invariably be conducted all over its surface; but as this is somewhat difficult with small pieces of glass on a large surface of metal, I devote one side of my plate for slides alone, and reserve the other for purposes where the utmost flatness is necessary. Its flatness, however, should be frequently tested with a straight edge, and if elevations appear, they should be reduced by grinding them down. Optical glass grinders and other mechanics, who require a plane grinding surface, have three similar tools; when one of them becomes untrue by use, it is ground with one of the others,

until they present a like surface, neither of them true. Tool No. 1 being now ground with tool No. 3, the inequalities left by No. 2 are obliterated, and a flat surface is the result. As it would be particularly inconvenient for me to carry with me three plates each weighing 14 lbs. for the sake of keeping one of them true, I resort to another, but equally efficient plan: I take a piece of plate glass of the same length as the pewter plate, the width not being very material, with plenty of emery and water I grind the metal all over its surface with the *flat* side of the glass until they present a corresponding surface; if the metal be not sufficiently flat, I turn the glass and grind the other side: by this process the flatness of the metal may be insured; with the surfaces of the shell-lac and emery plates the correction, as before described, is very simple.

To abbreviate the time of edging the slides, it is expedient to hold one in each hand and grind them simultaneously; and although this may be somewhat difficult at first, a little practice will give all the facility and tact necessary for thus grinding *two* glasses in the time of one.

Cement for the Cells.—The slide being ready, the cell is to be cemented to it, and for this purpose a good, and water-proof cement is necessary. Canada balsam is too brittle; gum mastic is equally brittle and difficult to use, and I could not for some years find anything equal in toughness and durability to my own composition—gold size and lamp black—and I have now in my possession cells containing wet preparations cemented with it 14 years ago, every portion of which is perfectly sound. It is, however, in every respect, vastly inferior to the marine glue already alluded to. In the year 1842 my attention was directed to this composition by the newspaper accounts of experiments made with it at the Royal dockyards at Woolwich.

I consulted the patentee, Mr. Jeffery, and desired to know if it could be applied as a cement to glass; of this he knew nothing, and gave me some to try, and general directions how to use it. It failed; and for some months continued to fail, until the inventor made some specially for my use at the College of Surgeons, with which I had the most complete success. As made for general use, the marine glue consists of different degrees of

hardness, distinguished by numbers, from one, downwards; the particular composition made for me agreed nearly with the ordinary "No. 4," but as in addition to caoutchouc and shell-lac—the staple ingredients of the marine glue—this contained another and most important material, as applied to glass, it was agreed to call it "No. 4, G. K.;" subsequently the same valuable ingredient enters into the composition of every form of marine glue, so that "No. 4" is now a sufficient description of it.

Another and very beautiful preparation of the marine glue has been made in the United States, suggested by Dr. P. B. Goddard of Philadelphia.

It consists of caoutchouc dissolved in chloroform by the application of gentle heat to the consistence of a thick mucilaginous paste; then add clean, carefully selected tears of gum mastic, until the composition becomes sufficiently liquified to use with a brush, when it should be filtered to free it from the dirt always combined with the gums in question. The gum mastic not only readily dissolves in chloroform, but it is a somewhat curious fact that it should reduce the thick solution of India rubber to the condition of a transparent, lympid fluid; it must not be made too thin however, for when dry it will be brittle from the excess of mastic. This is a very elegant cement; it can be used with or without heat, and when dry it possesses the great advantage of being perfectly colorless and transparent: I have not employed it for vessels of much size, but simply for shallow cells.* The patent marine glue requires heat, and I have already described one mode of melting it; the following is the way *to cement the cells.*

Aparatus used in cementing the Cells.—I employ for this purpose an aparatus that I made many years ago for mounting preparations in Canada balsam. It forms an important part of the contents of my "manipulating box," and it is one of the things pirated by the author of the modern work indicated.

A plate of wrought iron 6⅜ths by 2⅜ths and ⅛th thick, ground

* Since writing the above, I find that the preparations for which I used it 12 months before have not stood, I cannot, therefore, recommend it.

on its upper surface,)fig. 3 *a* is supported by four legs of brass wire (*d*) $\frac{1}{16}$ths diamater, and 3 inches long in the clear;* they screw into holes at the corners of the iron plate, and their free ends are placed in sockets in a mahogany board (*b*) the size of the iron plate, and $\frac{1}{4}$ thick. When in use, the plate becomes so hot that it cannot be touched with the hand, and the legs by conducting the heat, mark the table; to render the entire aparatus more convenient, I found it better to add the mahogany board; the holes for the legs are defended with brass plates, and they fit tightly, so that the whole can be moved bodily out of the operator's way. The spirit lamp (*c*) is $1\frac{1}{4}$ inch square and $2\frac{1}{4}$ high to the top of the brass wick holder—exclusive of the ground glass cap. Every part of the glass that is to be coated with the marine glue should firstly be lightly painted with the fluid solution of it before described. Thus prepared, the slide and the four pieces of the cell should be placed on the iron plate, and the heat of the lamp applied beneath.

The position of the lamp should be frequently changed, to impart an equal temperature to the iron plate, for if there be too great an accumulation of heat at any one point, the glass will instantly break; should the plate become unnecessarily hot, lower the wick, or remove the lamp for a short time.

The solid glue may be cut into long thin slips with a knife or scissors, and applied to the painted surface of the slide and pieces of the cell, until the glass be hot enough to melt it, when it should be distributed evenly over the glass by means of another piece of glue held in a pair of short, strong forceps. Then search for, and remove, particles of grit and dirt which are contained more or less in the glue—they are best seen by removing the glass from the iron plate and placing it on a piece of clean, dry, white paper; they can be easily removed by the point of a knife, or a piece of the solid glue. Extraneous particles are frequently broken into fragments between the glasses by the pressure necessary to form a joint, but they should always be removed, as they act mechanically as a wedge, and preclude the possibility of a permanent joint.

* The height of the spirit lamp must determine the *length* of the legs.

At a certain temperature the glue will bubble and boil, at which point it should be removed from further contact with heat; otherwise it will be decomposed, and all its characteristics destroyed.*

For neatness and uniformity, the cells should be placed in the centre of the slides, and to accomplish this it is best to mark the outline of the slide on paper or card-board with a pen, and then draw a cross, the centre of which is the centre of the slide, its limbs extending the whole length of the long and the short diameter of the figure.

The glass being hot, and the glued surfaces freed from dirt, the several pieces of the cell are to be turned quickly over with a pair of forceps, and placed upon the slide in the relative position they should occupy.

The slide should now be put on the card-board figure, each piece of the cell should be pressed down to the slide with two pieces of wood, and rubbed to and fro to express the excess of glue, and make as near approach to contact with the lower glass as is compatible with the thinnest layer of glue. The four pieces of the cell having been cemented to the slide and to each other, its position can be readily adjusted to the centre, by the aid of the cross figured on the card. Should the glass become a little too cool and the glue set, replace it on the iron plate and complete the adjustment.

Before the glass and the glue become quite cold and hard, it is desirable to remove the superfluous glue which holds most pertinaceously when cold: the best form of instrument for this purpose is the lozenge-shaped tool used by engravers, keeping the point close to the sides of the joint, or a knife-point may be used, taking care not to scratch the glass. As a rule, it must be borne in mind that, whenever a cell consists of more than one piece of glass, it should be ground flat on the pewter or emery plate, after being glued to the slide before it is fit to be trusted: the slightest inequality, either in the substance of the glass at

* This is the peculiar decomposition of the shell-lac before described.

one end, or in the layer of the marine glue, will prevent the possibility of making a good joint hereafter with the top glass or cover.

To clean the glass perfectly, I use a small piece of cotton wool gathered into a knot, held by a pair of strong steel forceps, and a drop or two of liquor potassæ, or a saturated solution of caustic potash, which softens the marine glue and admits of its removal. Care must be taken not to allow the potash to remain in contact with the joints, as it decomposes the glue, and will render the joints unsound. The glass should be well rinsed in a large quantity of clean water, to remove the potash.

A top glass or cover must now be cut for the cell, and this should be somewhat smaller than the outer diameter of the cell on all sides, to allow room for the cement. The edges of the cover, and surface of the cell, should be painted with the naphtha solution of marine glue; and the cell will then be ready for the reception of the preserving fluid, and the preparation. It is best to fill the cell over some other vessel to catch the excess of fluid that is sure to run over the sides; a small shallow dish or saucer will answer this purpose; and if the cell be supported upon a level something placed in the dish, the better, as the operator will have his hands at liberty.

Having filled the cell with fluid, take a short but strong camel's (or badger's) hair pencil, and rub the fluid into the corners, along the sides of the cell, and even the bottom glass, for this reason: in pouring the fluid into the cell, it remains separated from the glass in every direction by a filmy layer of atmospheric air, which can only be removed, and the fluid and glass brought in contact mechanically, or by the thin gum-water, or saliva, formerly referred to. If a vessel be sealed down without attending to this precaution, the air will be liberated by degrees and form a great number of minute bubbles, glistening in rows upon the sides, in the angles of the cell, and even upon the bottom glass:—ultimately they coalesce, and an air bubble of some size is the result. For the same reason (the displacement of air), the preparation must be placed carefully in the cell, and if it, possess cavities in the under surface, they should be pressed out

if possible, while the preparation is entering the cell; and this should be managed so that one end of the preparation goes into the vessel first, and the remainder lowered gradually. A good steady stream of fluid should be poured into the cell, the preparation being held down by a camel's hair pencil or forceps, until all extraneous particles be washed away, and the fluid continue bright and clear. When at rest the fluid in the cell will present a *convex surface*, bounded on all sides by the painting of marine glue.

Now prepare one surface of the cover, either by the tongue or brushing on the fluid as before, and holding it by a pair of forceps at one end of its *longest* diameter, lower the other end to the cell, and let it down gradually—the excess of fluid running before—until it be in its place; then adjust it accurately, press down the cover till it touch evenly every part of the bed on which it is to rest, remove with a brush whatever fluid may yet remain on the outer edges, and paint them *once* more with the naphtha solution, including this time the top glass: apply the black cement, and the preparation is finished. Should another layer of black cement be required, allow the first to become quite dry, and isolate it, as before directed, with the napththa section; successive layers of the black cement must always be thus separated.

Drilling or cutting circular holes in glass for cells.—A more elegant mode of constructing a cell, is by drilling a circular hole through a piece of glass (fig. 4); but when I first attempted this plan, (in the years 1839–40,) the cost was prohibitory. At last I employed at the same time, three workmen in London to ascertain the lowest rate of cost at which holes of all sizes could be drilled in glass, in any quantity not less than one gross. Neither of these men employed the same means; one of them, a German, cut most beautiful cells, perfectly square inside, but he could not do them for less than 1*s.* 6*d.* or 36 cents each.

Another made very indifferent cells at 1*s.* or 24 cents each; and the third made excellent work at 6*d.* or 12 cents each. Subsequently, another man undertook to do them equally well for half that sum, and ultimately I procured them at the same price, ex-

cellently cut, from Mr. Dennis of 1 Charles street, St. John's Street Road, London. The following is the plan of proceeding: procure a copper tube (or drill, as it is called) of the diameter you desire your cell to be: I have long since discontinued cells of *all* sizes, and chiefly use one of $1\frac{3}{8}$ths diameter, cut out of a square of glass $1\frac{1}{2}$ inch *full*, outside measure.

With my large slides such a cell enables me to preserve great uniformity in my preparations, affords abundant space for the transmission of light around the object, holds a sufficient quantity of preserving fluid, and the squares can be cut with the same guage used for the slides themselves. The length of the drill may be from 1 inch to $1\frac{1}{2}$ long, and made to run true in a lathe. The squares of glass being all of the same size, I cement a number of them together with the marine glue, so as to form a pile of from one inch to two inches high.

Where a *lathe* is used, it is important to face the pile of squares with something that has been already perforated with a hole the size of the drill to enable the latter to enter at once upon its work, and prevent the scratching (and spoiling) the first cell. For this purpose, *brass* perfectly flat, can be used; but a better thing is a square of *plate glass*, one-fourth of an inch thick, already perforated by the same drill, and it should be kept for this purpose alone.

The copper drill is to be charged with fine cutting sand and water, and the block, or pile of glass squares applied to it, and gradually pressed up by the tail spindle of the lathe, while the drill should revolve at a *moderate*, but not a *rapid* rate. When a number of cells are drilled, they can be easily separated from the block by placing the outer cell downwards on the iron plate and applying the spirit lamp: they can be removed one after the other with great rapidity, untill only *undrilled* glasses remain, and to these other squares may be added if necessary. To make an oval cell, two round holes (fig. 5, *a*, *b*,) must be drilled so as to intersect each other; their proximity to be determined by the *length* of the oval required. The pieces that remain on each side, *c*, *d*, can be cut off with the diamond if the glass do not exceed one-eighth of an inch thick, otherwise a disc of copper, the diameter

of a ten cent piece must be applied to the lathe, charged with sand and water on the edge, and it will speedily make the sides of the cell level.

There is another excellent method of cutting either round or oval cells of any size, *provided the glass do not exceed one eighth inch in thickness*, for which I am indebted to Sir Charles Babbage, the inventor of the celebrated calculating machine. Mount a glazier's diamond to cut a circle; this can be done in a variety of ways—I have mounted my diamond as shown in fig. 6. *a* represents a square bar of iron, 8 inches long, with a male screw at the lower end to fit a plate of iron (*i*) tapped to receive it, *b*, *b*, two arms 4½ inches long, clamped to the upright bar by thumb screws, the other extremity drilled and ground to receive *c*, which is a spindle, to the lower end of which is rivetted a screwed rod *d*, 4 inches long from the centre. *e* is a box to receive the glazier's diamond when removed from the handle, and kept in its place by a tightening screw; continuous with this box is a square nut, *f*, perforated with a hole large enough to allow it to swing freely on the screwed arm. *g*, *g*, are square screwed nuts, one on each side of the diamond box.

In adjusting the diamond to cut a disc, say three inches diameter, move it along the screwed arm until the point be coincident with 3 inches as marked upon the scale (*l*) which has before been drawn upon the chamois leather which covers the board K; then bring the nuts close to either side of the diamond box, adjust it to its cutting angle, and then by means of a wrench in each hand *tighten* the nuts simultaneously. The glass to be cut may now be placed upon the board; remove the pin *h*, and lower the spindle (*c*) till the diamond touch the glass: then holding the screwed arm by one hand, turn it steadily round and it will describe a circle of the size to which the diamond has been previously adjusted.

As the spindle has been ground to fit the two collars through which it works, the motion is smooth and steady. Such a machine would be improved by casting the upright bar and the two arms, *b*, *b*, in one piece; the height need not exceed six inches. Having cut a circle on a piece of glass, if the *disc* be wanted, cut

the glass in three or four places *from the circular cut to the outer edge*, and carefully break off the pieces; but if a *cell* be wanted, the *disc must be removed* without injury to the remainder of the glass, which is to form the walls of the required cell.

The cut, already made, gives the exact dimensions of the aperture required; now alter the position of the diamond on the screwed arm and cut a circle *within* the first, adjust again and cut another circle within the last, and so proceed cutting circle within circle as long as the diamond can be adjusted to cut, and the glass will then present a series of concentric circles.

Take a centre punch and screw it point upwards in a bench vice; place the centre of the *innermost circle* on the point of the punch, and get an assistant to hold the glass while the operator takes another centre punch and placing its point *upon* the glass immediately over the point of the lower punch strike the upper punch lightly with a hammer, *not to break*, but to *pound* the glass lying between the two punches. Presently a small hole will be made; now bring the broken edge of the glass upon the lower punch continuing the pounding motion until the hole be sufficiently enlarged to admit of using the "*plane*" of the small and light hammer with which the punch has been struck. At this stage of the process, the use of the upper punch should be discontinued, and the operator holding the glass himself, *keeping an edge of the hole always supported on the lower punch* while he lightly taps it with the plane of the hammer, not attempting to remove the *circles* as cut, but rather tracing the hole from the *centre to the circumference*, (like making a cross in the glass,) and if it have been *cut*, not *scratched*, large pieces will fall out as soon as they have room enough, and the entire operation be finished in less time than it takes to describe it.

In like manner, an oval cell can be made as easily as a round one. The glass should not be cut into shape to form the *outer* dimensions of the cell, until the central hole be made, as it is likely to break. Upon this plan a hole of any size can easily be cut in a plate of glass of any dimensions. Sir Charles Babbage told me that he once communicated this plan to a glazier, who employed it most extensively in punching holes in squares

of glass to form the bottom plates of street gas lamps, for the transmission of the gas pipe.

I have already remarked, that the above plan of cell-making is limited to glass one-eighth of an inch thick; my wants frequently require cells full as deep again; I could cement two or more shallow ones together, and thus build up any required depth, but they look heavy, and I prefer therefore to cut them out of plate glass of the proper thickness. When in London, I could obtain the services of Mr. Dennis to cut anything that I required, at any time; but in this country, at this moment, I am altogether thrown upon my own resources. To meet my wants I have contrived a small, portable, and most efficient apparatus, by means of which any lad can cut cells as well as I can, and with this machine my Son has cut for me the best cells I have ever had.

It consists of *a*, *a*, (fig. 7, A,) an iron support with three arms and a square plate at the lower end of it, cast in one piece. *b*, a vertical bevelled wheel working *c*, a horizontal beveled wheel by means of the crank *d*, the latter wheel, having for its axis a spindle *e*, the lower end of which screws into the brass mount of the copper drill, *f*, whilst its other end passes through a collar in the upper arm of the iron support and is regulated in its action by the wooden lever *g*.

Attached to the under surface of the wooden lever is a strong steel spring, about one inch wide, the profile of which is shown at B, fig. 7. *m* is the spring, to be screwed to the lever by the flat extremities. In the centre of the curved portion there is a slit through which the upper part of the spindle *n*, previously reduced for that purpose, passes, and in which it can freely play; it is kept in its place by the button *o*.

The square plate *i*, (A and C,) is screwed down to the board *j*, by four nut-headed screws. *k*, *k*, are two thick pieces of wood screwed on to the bottom board *j*, their inner edges being under cut to form a bevelled groove in which the two pieces of wood marked *k*, being beveled to the same angle, may freely move.

In the centre of the pieces marked *k* is a slit, through which passes a nut-headed screw to connect them with the lower board

and to admit of their adjustment, their inner extremities are cut to form half a square and are intended to receive the block of glass placed diagonally, to be drilled by the machine. The block of glass, *l*, should be placed exactly in the centre, and it can easily be secured in its position by clamping the side pieces of wood by means of the nut-headed screws and collars.

With this machine I employ superfine emery and water. It cuts best by a *dragging* motion; pulling the crank half round with one hand whilst the lever is kept down with the other; and then lifting up the lever and allowing *the crank to go the other half round without grinding.* By alternately lifting up the lever and pressing it down, the emery works into the cut; whereas if the lever be kept steadily down, either by a weight, or by the hand, and the crank turned constantly round, the drill will be cut away much faster than the glass.

The fixed position of the horizontal beveled wheel, *c*, would seem to preclude the possibility of elevating the spindle, *e*, the required height.

To obviate this difficulty, a notch is made in the wheel *c*, into which a square steel plug 1½ inch long is driven, which works freely in a corresponding slab made in the spindel, *e*.

The same directions apply to cementing the round and oval cells to the slides as those already given; and when finished they appear like fig. 8, where *a* represents the slide, *b* the cell cemented to it, and *c* the *well* formed for the reception of the preparation and the fluid.

Cells cut out of the *very thin glass*, that which is usually employed for covers rarely answer, *for want of flatness* in the glass itself; and yet, a great number of very beautiful and valuable, microscopical preparations, of the animal and vegetable kingdoms can only be preserved *in fluid*, and by the introduction of some medium to defend them from pressure between an upper and lower plate of glass. For this purpose I have long employed the marine glue, as the material from which to construct *very shallow cells.* Subsequently, the same substance has been employed by others for a like purpose, as Vide Quekett's Treatise

on the microscope; but the modus operandi, as therein described, differs so essentially from my plan that I think it expedient to give a description of it.

For neatness sake it is desirable that the cell, whether round or oval, should be placed in the *centre* of the slide, and to ensure accuracy in this respect together with rapidity of action I made a tool described in the accompanying figure, 9.

a Is a flat board of the substance represented in the figure; *b b b*, are three pieces of wood raised upon *a*, and so placed that a space is left, *c*, from front to back; this is intended to fit the bottom glass, on which a cell is to be made, and should fit the short diameter of the glass so well that it can only just pass in and out; in other words, room to admit of lateral motion is objectionable. The space is not intended to be as long as the greater diameter of the glass. *d*, Represents a thinner piece of wood fastened to the sides of the frame *b b*, in the centre of which is a round hole—the size of this hole can only be regulated by the width of the glass. One size slide that I have long used is less than that recommended by the Microscopical Society, and I have drawn the figure to exact dimensions of the apparatus it is intended to represent. The round hole is intended to receive and form a guide for a round copper, brass, or tin tube—copper being the best. The exact position of the perforated cross piece of wood can only be ascertained (as will soon appear) from actual experiment.

Having determined the size of the slip of glass upon which a cell of marine glue is to be formed, it is necessary to determine the diamater of the circle which is to form the *interior* margin of the cell.

Procure a tube of copper, brass, or tin, about three inches long, the *outside* diameter of which corresponds with the *internal* margin above described. Especial care should be taken to have a sufficient space outside the circle and the sides of the glass slide to form a good support for the top glass and for the black cement to secure it.

The hole in the cross piece *e*, must allow the tube to work easily through it, and the cross piece so fitted to the frame, that when the glass slide is placed in the space *c*, the tube placed in the hole in the cross piece will descend upon the *exact centre* of the slide, equi-distant from end to end, equi-distant from side to side. The accurate adjustment of the cross piece is somewhat troublesome; for *circular* cells, it is not very important; but for oval cells it becomes a necessity.

This apparatus being ready, place the glass slide on the iron plate and apply the spirit lamp; put some marine glue on the glass and as it melts distribute it evenly over the glass to an extent *beyond the proposed limits of the cell*; the *quantity* of the glue must depend upon the depth of cell required. If it be wanted *very shallow*, continue the application of heat to procure the necessary hardness of the glue, but in all cases the glue should be made tolerably hard by inspissation. Remove the glass slide from the iron plate to space *c*, in the wooden frame, dip the end of the metal tube in water, pass it through the guide aperture in the cross piece, press it firmly upon the glued surface of the glass slide, turn it half round and remove it—a circle will be cut in the hot glue on the glass, the interior of which can be (now) easily removed, the water from the tube assisting.

It is easy to make an *oval* cell of any dimensions in the *long* diameter commensurate with the size of the apparatus, in this way: cut some pieces of glass the exact width of the space *c*, $\frac{1}{8}$th, $\frac{1}{4}$th, $\frac{3}{8}$ths, &c., wide; place one of these in the frame close up to *f*, now put the glass slide in its place, resting against, not the wood *f*, but the narrow glass placed there; make the round hole in the hot marine glue with this punch, instantly *turn* the slide end for end, and punch again, remove it, and with a knife connect the *axes* of the two circles, clean off the glue from the interior, and the oval is made. If the aperture in the cross pieces be other than exact, the oval will be *diagonal* in respect to the long diameter of your slide.

Now grind the cell upon the emery plate to the required substance—it can be made beautifully flat,—and then by means of a square and knife form the outer lines of the square in which

your round hole is punched, or the ends of your oval cell, as the case may be: my *small* slides are so narrow that the space beyond the circle in the smaller diameter is only enough to give a required margin for the cover, and cement.

A little piece of cotton wool, dipped in liq: potassæ, held in a pair of forceps, and applied to the interior of the cell, will soften the glue, and it can then be easily removed with a scalpel, small chisel (⅛th diameter), or lozenge shaped engravers' tool. When finished, the cell is really a beautiful one; I have never had a preparation in this form of cell *fail*.

In a former publication devoted to a description of my preserving fluids and glass cells, my silence with regard to one particular form of cell, also of my invention, was misconstrued by a gentleman who has, it appears, adopted my cast-off. I allude to a cell made by cutting off a slice of a glass tube.

In the year 1841 being in Edinburgh, I employed the late Mr. Sanderson, Lapidary of the Pleasance, to make sections of glass tubes for my use, but, as he could not make two sides alike or either of them true, they failed. Sections of a tube can only be made true by a compound motion of this kind, i. e., they must not only be ground with a circular motion on a flat surface, but revolve on their own axis simultaneously.

If such a disc be firmly held by the thumb and finger and submitted to the grinding process already described, the *pressure being unequal*, it will cut quicker beneath the finger and thumb than elsewhere, and still more beneath the thumb than the finger: change its position and you have the like result, so that *holes or hollows* must be formed unless the disc can be made to revolve *constantly and uniformly*, subject to the same pressure.

With such a surface, it is hopeless to expect to obtain contact with flat glass, and marine glue.

Apparently you have a joint: but if the preparation be subject to the slightest jar at any time, off comes the slice of tube cell and the preparation is lost. A friend of mine, resident at Albany, being in New York city, purchased 12 injected pre-

parations just arrived from London. Six of these were mounted in the section of tube cell, the remainder in cells drilled out of flat glass—a round hole in the small square plate. On his arrival at home, his children anxious to see all that had arrived from New York, clutched at the table cover on which the rarities were displayed, down went the 12 slides, and when picked up, the six *drilled* holes in the square plates of glass were uninjured, the other six had separated and the *costly* preparations were entirely lost; nothing would induce that man to purchase preparations mounted in cells of that form again. Where it necessary, I could give a great number of like illustrations; but I think it must be obvious, that the four corners of a square glass with a round hole in it must cling with great pertinacity to the lower plate, and that, if it be possible (as it really is) to cement two pieces of glass together so firmly that no mechanical force can separate them, this should give a better chance of success than a thin rim of glass.

I can only add that amidst my incessant wanderings on this side of the Atlantic, I have more than once, twice or thrice, been horrified to see a box containing upwards of 60 dozen of long cherished and much valued preparations *flung out of a waggon*, or hurled to the ground from a baggage waggon in a manner peculiar to railway travelling in the United States, and such as no one who has once seen it can ever forget, and yet, only twice out of 3 years have my preparations been broken, and on *each occasion* the ends, only, of bottom glasses were broken off close to the *outer margin of the cell*; in no one instance has a *cell come off.*

There are many preparations of entire animals no less than dissected portions of them, which can be well displayed only in vessels with flat surfaces, in contradistinction to round or oval bottles; but, from their greater size, they cannot be contained in vessels constructed on the principle of those already described. For all such, I build up a *box* of glass, consisting of four sides, the bottom plate (or slide) and the top, or cover—six pieces in all.

These vessels are confessedly difficult to make; yet they form the most attractive and beautiful exhibitions that can be put into a museum. The trouble I have had with these upright vessels, no less than my great desire to submit them and the preserving fluids, to the only satisfactory test—time, has retarded the publication, on my part, of the several processes herein referred to.

Having settled the length, depth, and width for an upright box, the glass for the sides should be selected of sufficient substance for the bulk and weight of fluid the vessel is destined to contain; and it will frequently happen that the ends (by which I mean the two lengths of *least diameter*, calling the larger and outer portions the sides, as in the annexed figure, 10,) require to be somewhat thicker than the sides to insure sufficient surface for a joint. The glass should be cut as true and square as possible in the first instance; the two side pieces and the two end pieces should be connected together, respectively, with the marine glue, forming two pairs of glasses. First, bevel all the glasses, on the metal or emery plate, as before directed; and then proceed to make perfectly flat the extreme ends of each pair of glasses; this, the most important, is, at the same time the most difficult part of the work, and such as can be accomplished only by practice; the position of the glass in the hand must be frequently changed, for the pressure of two fingers on one side, opposed to the thumb on the other, will have a tendency to incline the glass to an angle of 45°, whilst the operator believes he is holding it perfectly upright. It frequently saves time to grind till a smooth but inclined surface extend from one outer edge of the pair of glasses to the other, and then change its position in the hand—the probability is, that there will be the like tendency to form a similar angle, although reversed, and by carefully watching and measuring, the operation may be suspended at the point where perfect flatness obtains, and just before the inclined plane can be formed in the other direction. A small brass Square will be found of considerable importance in testing the truth of the grinding, but the most severe test is that which I

always resort to namely, to wet the ground surface of glass as lightly as possible and place on it a plate of plate-glass—the sides of which, for all practical purposes, are parallel: if true and flat, the plate-glass will be seen to touch every part of the ground surface and form with it a T. Now wipe all the glass just tested quite dry; *breathe* upon the ground surface, and quickly apply the plate-glass—if true, the moisture of the breath will be equally diffused along its surface, and the contact be so perfect that the ground surface will hang suspended for several seconds from the plate-glass. If the work endure this test, there need be no difficulty in making a permanent joint.

Again use the iron-plate, un-cement and clean the glasses, prepare the ground surfaces of the ends and the flat surfaces of the sides against which the ends are to abut, with the naphtha solution; return all the glass to the iron-plate (if small enough to lie there at the same time) and place marine glue on the painted flat surfaces of the sides; when melted put on the *ends*, which will form three sides of an open square, then quickly place the other side on the ends, and carefully remove it from the plate to a piece of wood, or paper—the former, provided with a straight edge (like the cutting board) is the best. While the glass remains hot enough to keep the glue soft, press together and critically adjust the glued surfaces, taking especial care that the sides coincide with the angles of the brass square; it is most important to remove from the glue in the joints any extraneous particles of dirt. These preliminaries settled, the glass cold, and the glue hard, the operator will have four sides of a box—like a brick-maker's mould—without top or bottom; this he may now proceed to grind upon the metal or emery plate, firstly bevelling the edges, by a circular motion—constantly turning the box in his hand to prepare it for the bottom: after this is accomplished, it must pass the ordeal of the former test next the slide or bottom plate of glass must be heated on the iron plate, (it having been previously prepared with the naphtha solution which must also be applied to the ground surface of the box;) and then after melting the marine glue on the upper surface near the edge for the adhesion of the lower edges of the hollow box, this box is to be

applied to the slide and the whole suffered to remain on the heated plate of iron long enough for the lower portion of the sides of the box to become sufficiently hot to form a joint with the slide, but without melting the joints previously made; if it can be avoided when cold, the upper surface may be ground in the same manner as the bottom, and with like care, and when finished, the vessel will appear like figure 10.

To give additional support and resistance to the joints, Mr. Dennis, suggested to me the application of triangular bars of glass, which he called "angle pieces" cemented into the corners. In my experience of them they fail, for two reasons—one, because they cannot be ground with sufficient accuracy, and the other, that in cementing them the heat is generally so great as to decompose the glue. I have substituted the following plan with more success: I pour melted glue into the corners, and make the angles of the vessel hot enough to keep the glue fluid, while I cause it to run equally from the bottom to the top of the box—this plan has not disappointed my expectation of it in any instance.

I have made several upright vessels some of them of great dimensions (fifteen inches high, six inches wide, and four inches deep) and extremely elegant in their appearance. A preparation of Physalia pelagica (Portuguese Man of War), fig 11, will give an idea of this form of vessel; the original box is eight inches high by two inches wide, five-eighths deep from back to front. *a* represents the front side. *b b*, the end pieces, *c*, a block of polished plate-glass half an inch thick to which the upright box is cemented, *d*, a thinner plate of glass forming with *c* a handsome pedestal and heavy support for the upright vessel.*

The joints of the pedestal, and of the box to the thick upper plate, must be made with Canada balsam or the *chloroform* preparation of marine glue for the sake of transparency; the box must be made as before directed, with the patent marine glue. When

*The mode of grinding a box of the height here mentioned, does not in any respect differ from that which has been described. Here the end pieces are considerably elongated, and must firstly be made true in the direction of their longer axis—the top and bottom is not to be ground untill the hollow box be perfect.

large surfaces of glass are to be cemented together, the iron-plate is insufficient for the purpose and another plan must be had recourse to. I have already remarked that a red-hot soldering iron may be applied to the edges of glass with considerable impunity, and I use such in the manufacture of large upright vessels. Fig. 12 represents the several forms of such irons as I have found desirable. Numbers 1, 2, 3, and 6, are reduced one-half in size; and five is more reduced; 1 and 2 are made of iron rod $\frac{5}{8}$ths inch square, welded to a round iron wire which is inserted into a wooden handle; 2 only differs from 1 in having the iron wire bent as shown in the figure—they are both eleven inches long, inclusive; 3 is intended to apply the marine glue to the inner angles of the boxes and upright vessels; an end view is shown at 4. The wire of this last is differently fixed, and its length, including the wooden handle, is fifteen inches; 5 represents a much heavier tool and is designed to retain the heat for a longer time than either of the former. With a pair of such instruments, large surfaces of glass can be well and expeditiously soldered.

To be concluded in the next number.

*** The Publisher begs to inform the Subscribers of the Upper Canada Medical Journal, that the diagrams referred to in the above communication, are in the hands of the Artist, and will be forthcoming at the conclusion of the article in the next number.

MONTHLY METEOROLOGICAL REGISTER, AT

Latitude, 43 deg. 39.4 min. N. Longitude, 79 deg. 21.5 min. W.

Magt.	Day.	Barometer Corrected. 6, A.M.	2, P.M.	10, P.M.	MEAN.	Temperature of the air. 6, A.M.	2, P.M.	10, P.M.	MEAN.	Tension of Vapour. 6, A.M.	2, P.M.	10, P.M.	MEAN.
						°	°	°	°				
c	1	0.317	0.332	0 281	0.306	2.8	7.9	1.7	4.26	0.147	0.187	0.157	0.167
ld	2	.307	.322	.268	.289	0.1	10.4	1.3	3.81	.139	.206	.154	.167
c	3	.162	.092	.044	.053	5.1	10.7	13.8	9 93	.169	.217	.261	.215
b	4	.221	.323	.328	.307	14.5	10.5	6.5	10.63	.214	.265	.211	.237
c	5	.253	.176	....	....	13.2	10.4	....	....	.236	.241	....	....
c	6	.121	.266	.314	.239	13.3	10.7	17 2	13 83	.238	.256	.290	.256
c	7	.281	.395	.636	.478	18.6	14 3	17.1	18 21	.274	.311	.284	.296
c	8	.379	.461	.07	.193	13.2	8.1	7.5	9.78	.228	.159	.158	.173
bc	9	.161	.237	.180	.186	11.4	7.5	4.8	7 71	.159	.186	.119	.179
.	10	.059	.044	.06	.068	1.7	9.5	3.7	5.01	.135	.179	.161	.158
.	11	.170	.233	.171	.191	6.1	5.5	2.9	5.09	.160	.159	.124	.144
.	12	.071	.019	....	....	4.1	3.2	....	....	.126	.131	....	....
cd	13	.060	.022	.102	.061	1.4	1.0	1 8	0.71	.118	.161	.105	.132
c	14	.336	.389	.318	.343	3 8	0 9	3.1	0 09	.107	.172	.137	.125
b	15	.321	.429	.391	.38	0 9	3 1	1.5	0 13	.107	.150	.120	.126
a	16	.278	.016	.391	.088	2 9	4.1	10.9	9.08	.161	.173	.208	.186
c	17	.641	.52	.422	.520	13.9	0 0	9.4	0.19	.204	.145	.092	.134
c	18	.368	.14	.104	.166	3.7	1.4	0.5	0.91	.093	.106	.105	.107
b	19	.092	.59	....	....	0.0	4.4	....	....	.113	.172	....	....
b	20	3 8	.15	.060	.172	8.4	1.4	7.7	1.22	.121	.116	.088	.109
c	21	.078	.25	.354	.239	10.3	11 5	9.3	10.02	.078	.063	.076	.074
a	22	.462	.567	.547	.525	3 8	3.8	0.3	2 52	.086	.081	.099	.089
cd	23	.250	.0	.145	.010	4.9	3.4	8.8	6.36	.147	.185	.197	.176
c	24	.280	.40	.181	.296	16.0	13 8	9 5	12.41	.224	.232	.175	.208
cd	25	.006	.074	.232	.059	7.5	1 9	6.0	5.79	.149	.148	.160	.137
a	26	.202	.01	....	....	2.5	0.4	....	....	.124	.122	....	....
c	27	.230	.057	.42	.10	3 2	3 4	8 6	4.39	.097	.148	.199	.155
c	28	.681	.411	.07	.367	15.7	0.5	1.8	5.63	.234	.117	.113	.149
ec	29	.026	.129	.177	.111	3.0	1.2	0.1	0 54	.169	.127	.125	.122
a	30	.035	.037	.030	.002	3.4	3 5	0 4	2 44	.119	.171	.130	.134
a	31	.032	.123	.219	.124	1.4	6 1	4.1	2.82	.125	.122	.112	.119
Means {	Normal.	29.650	29.621	29.640	29.643	21.65	30 35	26 57	27.01	0.172	0 170	0.156	0.159
	Observ'd.	29.614	29.579	29.591	29 593	30 83	35 16	31.69	32.27				

Highest Barometer .....30.210, at 4 p. m. on 2nd } Monthly range
Lowest Barometer.....28.966, at 6 a.m. on 28th } 1.244 inch.

High'st observed temp.... 51°0, at 2 p.m. on 7th } Monthly range
Lowest registered " 13 2, at a.m. on 21st. } 37°8

Mean. high'st obs'v'd tem. 36°54, } Mean. daily range:
Mean. registered minimum 26 58, } 9°96

Greatest daily range, 22°2, from 6 a.m. to 10 p.m. on 17th:

Warmest day, 7th, mean temperature 47°65 } Difference,
Coldest day, 21st, mean temperature 16 72 } 30°93

The "Means" are derived from six observations daily, viz:—at 6 and 8, a.m.; and 2, 4, and 10, p.m.

The column headed "Magnet" is an attempt to distinguish the character of each day as regards the frequency or extent of the fluctuations of the Magnetic declination, indicated by the self-registering instruments at Toronto. The classification is to some extent arbitrary, and may require future modification, but has been found tolerably definite as far as applied. It is as follows:

(*a*) A marked absence of Magnetical disturbance.
(*b*) Unimportant movements,—not to be called disturbance.
(*c*) Marked disturbance,—whether shewn by frequency or amount of deviation from the normal curve,—but of no great importance.
(*d*) A greater degree of disturbance,—lasting more or less the whole day.
(*e*) Considerable disturbance of the first class.

The day is reckoned from noon to noon. If two letters are placed, the first applies to the earlier the latter to the later part of the trace. Although the declination is particularly referred to, it rarely happens that the same terms are not applicable to the changes of the horizontal force also.

Toronto, January, 1852.

M. M. Magnetical, Observatory Toronto, C. W.—DECEMBER, 1852.

Elevation above Lake Ontario, 108 feet.

Humidity of Air.				Wind.			Rain: Inches	Snow: Inches	Weather.
6 AM	2 PM	10	Mn.	6, A.M.	2, P.M.	10 P.M.			
.82	.70	.82	.80	SW b W	S b W	SW b S	.....	.....	Auror. light 4&5 a.m: lt'l cl'ds dur. day.
89	71	8.	83	SW b S	SSW	NE b E	....	.....	Cl'dy at 6&8 a.m: day clear and fine.
89	75	90	85	NNE	NE b E	ENE	1.020	.....	Densely overcast: rain'g fr. 9h.50m. pm.
91	93	93	93	E b N	ENE	N b E	0.060	.....	Rain'g sl'tly till 2 pm: den. cldd. all day.
91	87	..	..	WNW	W b S	WSW	inapp.	.....	Densely overcast; drizz. rain at 11 pm.
90	91	9.	92	S b E	E b S	SSW	inapp.	.....	Overcast all day; sl't rain 1 to 3 pm.
93	94	88	90	SW b S	ENE	SSE	0.160	.....	Dens-ly overcast; rain'g fr. 2h.40m. pm.
96	63	74	75	SW	WSW	SW b W	.....	.....	Clouded; a few clear spaces occasionally.
84	77	86	83	ENE	ESE	ESE	.....	.....	Clouded till 4 pm: auroral light fr. 10pm.
88	70	87	84	SW b W	S w b W	Calm	.....	.....	Morn'g clear; cloudy fr. 2 pm.
90	72	70	76	N	N b W	W	.....	.....	Overcast; very dull, dark day.
87	65	..	..	W b N	WSW	SW b W	.....	0.1	Morn. clear: aft'n cl'dy, sl't snow d. night
89	92	75	83	W	N	NW b W	.....	0.5	Clouded, snowing occasionally dur'g day.
89	97	79	84	NW b N	S b E	SE b E	.....	.....	Generally clouded; dull.
81	80	83	82	S	S b W	S b W	....	....	Clear save a few clds. rd. horizon; fine.
89	88	91	88	E b S	ESE	E b S	0.745	0.2	Snow fr. 11 am: turned to rain at 1pm.
84	85	94	85	SW	WSW	WSW	.....	0 5	Raining till 5 am; auroral light fr 10pm.
81	67	71	75	WSW	W	W bN	.....	inapp.	Densely cl'd: d, sl't snow occas. windy.
84	86	..	..	WSW	SW b W	W b S	.....	.....	Densely overcast; very dark dull day.
67	74	83	78	NW	NNW	N	... .	0.8	Overcast, mild; sl't snow fr. 3 to 10 p m.
89	59	74	76	N b W	NNW	Calm	.....	.....	Lt'tly overcast 6&8 am; occas. sl't showers
75	55	6.	67	Calm	SW b S	SE	.....	.....	VM cl'ded; gen. clr. N. to 4pm; nl't ov'ct
92	93	91	95	ESE	ESE	E	0.600	inapp.	Overcast; sl't snow at 8am; foggy.
91	85	84	86	NE b N	W b S	WSW	0.010	.. ..	Rain'g sl'tly 6&8am: foggy; occ. showers
84	83	88	86	NW b W	Calm	E b S	.....	6.5	Overcast, constant snow from 8am.
87	75	..	..	NW b N	WSW	WSW	.....	.....	Overcast all day; part. of snow occas.
91	78	96	84	NW	E b S	E b S	1.100	1.0	Snow g fr. N; turned to rain at 5h45m.pm
97	70	74	79	S b E	WSW	W b S	.. ..	inapp.	Ceased rain'g at 4am; auror. lt't at 10 pm.
74	80	85	81	SW	E b S	WSW	.....	0.5	Clouded dull day; part. of snow occas.
79	91	88	83	WSW	S b W	ENE	inapp.	2.0	Snow'g and very sl't rain most of the day.
91	94	89	92	NE b E	NE b E	N b E	.....	8 0	Snowing constantly and heavily all day.
.86	.80	.83	.83	Miles: 5. 60	Miles: 8.10	Miles: 6.47	3.995	20.1	

Sum of the Atmospheric Current in Miles, resolved into the four Cardinal Directions:

North.	West.	South.	East.
1096.30	2228.39	1970.31	1516.54

Mean velocity of the wind—6.51 miles per hour.
Max. velocity—21.3 miles per hour, from 1 to 2 p.m. on 28th.

Most windy day—17th: mean velocity—11.14 miles per hour.
Least windy day—10th: mean velocity—2.77 ditto.

Hour of greatest mean velocity—noon: mean velocity—8.37 do.
Hour of least " —9 p.m.: do. —5.26 do.
Mean diurnal variation—3.11 miles.

COMPARATIVE TABLE FOR DECEMBER.

Year	Temperature.				Rain.		Snow.		Wind.
	Mean	Max.	Min	Range	Days	Inches	Days	Inches.	Mean velocity.
1840	23 28	41.0	4.4	45.4	3	vapp	18	not re-	Miles.
1841	29.67	45.5	2 4	43.1	7	6.600	5	gistered.	
1842	25.35	40.3	3 8	36.5	3	0 880	17		
1843	30.59	41.1	2.7	38.4	6	1.019	8	8.1	
1844	28.78	48.9	— 0.8	49.7	6	Imp't	6	4.2	
1845	21.49	37.6	2 7	10.3	2	inapp	12	4.7	
1846	27 74	49.2	3.7	45.5	5	1 215	9	6.0	
1847	30.58	50.0	6 6	43.4	7	1.185	8	6 8	4.55
1848	29.64	49.1	0.6	48 5	7	2.750	7	2.5	5.44
1849	26 92	41.3	— 5.2	46.5	5	0.840	12	6.6	6.33
1850	22.55	48.3	— 9.7	58.0	2	0.190	18	1.5	7.40
1851	21.59	43.8	—10.3	54.3	6	1.075	15	.7	7.37
1852	32 27	51.0	13.9	37.1	7	3.995	10	0.1	6,54
Mean	27.03	45.16	0.03	45.13	5.1	1.648	11.2	11.6	6.23

MONTHLY METEOROLOGICAL REGISTER

Latitude, 43 deg. 39.4 min. N. Longitude, 79 deg. 21.5 min. W

Magt.	Day	Barom. at tem. of 32 deg.				Temperature of the Air.				Tension of Vapour.			
		6, A M.	2, P M.	10 P.M.	MEAN	6, A.M	2, P.M	10P. M.	MEAN	6, A M	2, P.M	10P.M	MEA
						°	°	°	°				
bc	1	29.723	29.634	29.784	29.691	23 2	26 9	21.3	23.47	0 110	0 129	0 110	0.114
b	2	.776	.685	.....	..	23.4	31 4	.....	.....	109	140	...	....
bc	3	.660	.697	.882	.768	26 9	25 1	18 3	22 95	12	117	08	.106
a	4	.915	.914	.9 7	.921	11 6	18 3	10 9	11 45	068	072	066	.073
c	5	.900	.792	.656	.769	7 1	23.3	18.7	16 03	05*	114	081	.086
c	6	.602	.447	.445	.501	25.8	31.2	21.6	27.57	12	149	101	.132
c	7	.482	.615	.768	.6 7	29 8	38.4	27 2	30 95	14	170	130	.164
cd	8	.794	.766	.694	.696	21.6	36 0	30.0	31.77	104	198	182	.160
cd	9	.578	.723	.....	..	36.3	40.9	.....	....	197	178	....	....
c	10	.690	.734	.813	.727	24.1	37.4	32.4	32 33	113	177	171	.153
a	11	.892	.930	.976	.914	34.5	33.8	30.7	32 55	169	141	153	.146
c	12	.971	.922	.928	.938	22 3	24 1	17.7	21.03	111	059	088	.086
c	13	.893	.865	.872	.879	20 2	26.9	23 7	23.47	100	084	116	.105
c	14	.855	.819	.816	.830	25.6	38.0	27.3	26 52	116	158	153	.127
a	15	.741	.676	.803	.752	29.1	31 2	10.1	21.40	142	117	058	.117
b	16	.945	.963	.....	.....	—66	4.7	.....	.....	024	050	.....	.....
b	17	.910	.807	.637	.784	5 0	18.0	13.9	12.38	0*8	082	070	.067
c	18	.658	.673	.798	.710	14 4	20.2	18 3	17 62	079	081	091	.084
a	19	.851	.899	.849	.866	18.0	21 9	25 9	21.12	084	102	113	.103
ac	20	.694	.462	.528	.557	21 4	32.9	2* 8	27 97	113	132	118	.121
b	21	.632	.634	.596	.621	18.7	34.1	27 9	25.67	087	161	135	.122
a	22	.523	.428	.324	.411	19 7	37.0	28.7	28.05	091	167	143	.134
b	23	.050	29.788	.....	....	30.9	41.	.....	.....	157	125	...	....
b	24	28 665	28.830	29 160	28 922	33.2	29.7	19 4	27.08	167	112	077	.120
b	25	28.889	28 991	29.174	29 046	27.5	13.6	9.7	16.27	145	061	062	.096
a	26	29.550	29.789	29.876	29 765	9.3	4.0	7.5	3 72	035	046	044	.042
ac	27	30.050	30.149	30.315	30.187	12.7	19.7	10.5	13.72	065	076	057	.065
b	28	30.293	30.220	30.136	30.211	10.0	7.9	21 0	18.25	062	104	091	.086
a	29	30 066	29.630	29 421	29 657	14.1	15.7	33.0	27.47	074	170	171	.140
c	30	29 496	29 687	.....	.....	35 9	29 5	.....	.....	146	094	...	...
b	31	29 660	29 562	29 826	29 666	22.6	38.6	28.4	31.13	105	179	116	.137
	M	29.707	29.691	29.757	29.7121	19.83	27.55	21.72	22 98	0.102	0.120	0.107	0.111

Highest Barometer 30 315, at 10 P. M on 27th } Monthly range
Lowest Barometer 28.653, at 4 a.m. on 24th } 1.662

Highest Temperature, 40.9, at 2 p. m. on 9th. } Monthly range,
Lowest reg. Temperature, —9.7, at a.m. on 16 } 59.6

Mean highest observed temp. 29 °04. } Mean daily range:
Mean registered minimum 11 .89 } 14 °.16

Greatest daily range, 40 °.9, from 2 p.m. on 15th to a.m. of 16th.

Warmest day, 11th. Mean temperature, 32 °.55 } Difference.
Coldest day, 26th Mean temperature, .72 } 28 °.83

12th, 8h. 23m., p.m., brilliant Meteor in South—time of flight fully 2 seconds.

The "Means" are derived from six observations daily, viz:—at 6 and 8, a.m.; and 2, 4, 10, and 12 p. m.

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(*c*) Marked disturbance,—whether shewn by frequency or amount of deviation from the normal curve,—but of no great importance.

(*d*) A greater degree of disturbance,—but not of long continuance.

(*e*) Considerable disturbance,—lasting more or less the whole day.

(*f*) A magnetical disturbance of the first class.

The day is reckoned from noon to noon. If two letters are placed, the first applies to the earlier the latter to the later part of the trace. Although the declination is particularly referred to, it rarely happens that the same terms are not applicable to the changes of the horizontal force also.

Toronto, February, 1853.

M. M. Magnetical Observatory, Toronto, C. W.—JANUARY, 1853.

Elevation above Lake Ontario, 108 feet.

Humidity of air			Wind				Snow, inches in the	Rain	Weather.
6 A.M	2 P.M	10	Min.	6 A. M	2 P.M.	10 P.M			
.85	87	.94	89	N	NW b N	WW bN	.....	.....	Densely overcast dull and comj day,
84	79	..	.	W b S	N E	E b S	.....	.....	Overcast—Mild.
87	84	82	83	N E b N	N b E	N b E	...	.....	Densely overcast. Dull day.
93	70	86	82	N b E	N b W	N N W	.....	.....	Uncloud d. a.m. Overcast from 8 p.m.
93	88	78	86	N	S W	S W b W	.....	.....	Uncl ouded a.m Afternoon overcast dul.d'rk
85	81	81	84	S W bW	S W	S S W	.....	.....	L'gt pas cl'ds. F'nt Auroral l t from md'nt.
87	73	84	81	S W b S	S W b S	S W	.....	.....	Almost unclouded, few passing clouds.
81	93	9..	94	N E b S	E b N	E N E	.....	*0.294	Overcast with light haze, foggy and mild.
93	90	..	..	SW b W	W S W	Calm	.....	.....	Densely clouded all day.
85	75	94	89	W S W	S S W	S S W	.....	inapp.	Densely overcast, drizzling rain at 4 p.m.
95	6.	89	79	N b W	N E	N E	.....	.....	Overcast, mild, dull and dark.
88	45	86	75	N E	E N E	N E	0.1	.....	Densely clouded, snowing slightly from10pm
80	59	88	81	N E	E N E	N E	0 5	.....	Densely cl ouded, slight snow or sleet all dy.
83	87	88	87	N E b N	N b E	N b E	0.4	.....	Densely clouded, slight snow till 3 p.m.
84	67	78	79	S W b W	N W b N	N Wb N	inapp.	.....	Clouded till 4 p.m. part snow occa'ly n't cl'r
85	85	..	..	N W b N	N W b N	N N W	.. ..	.....	A m. clear & cold. few detached cl'ds fr 2pm
81	84	80	80	W b N	N W	N b E	.....	.....	Few f't detached clouds pas. fine clear day.
89	72	83	81	N b E	N W b N	N b W	.....	.....	Mostly clear 4 pm. afterwards densely cl ded
86	75	79	8	N Wb W	N W b W	S W b W	.....	.....	Densely overcast all day, hazy.
84	74	82	79	W S W	S W	W b S	.. ..	.....	D.t'chd cl'ds a.m. clear & uncl'd from 6 pm.
82	81	88	84	W b S	S S W	S S W	.....	.....	Uncl'd t'l 4pm aft'rwds hazy hallo r'd moon
85	77	90	85	S S W	S S W	S S W	.....	.....	Gen'ly cl'd few cl'r spa's occas'ly [at midn't.
92	95	..	.	N N E	E N E	N	4 0	inapp.	Overcast, sleet & snow f'm noon till 9 p.m.
88	67	71	78	N b W	N W bN	NW bW	2 0	.....	Sno. t'l 9a.m af. part'ly cl'r w'd hi'h & sq'al
96	71	85	82	S S W	N Wb N	N W	0 2	.....	Sl't snow& hsa d'ft t'l 9 am hallo rn.d moon.
76	81	66	7.	N W	W N W	SW b W	.....	.....	Clouded a.m. n't clear & unc'ld. [fm 11pm.
79	70	77	75	SW b W	W S W	Calm	.....	.....	Lightly overcast, hazy.
83	67	81	79	NW bW	S W	S W b S	.....	.....	Clear & uncl'd, fine day and night.
97	8.	91	89	S W b S	S b W	W S W	.....	.....	L't cl'ds am, cl'r& uncl'd pm [dis.fr 6.56pm
*69	57	..	..	NW bW	N W	Calm	.....	.....	Gen cl'r, f't auro'l l't 7a.m.Zodiacal l't very
84	77	73	77	Calm	S b W	E N E	.....	.....	Generally clouded, cl'r spaces occasionally.
86	75	84	82	Miles N 5.69	Mile 7.75	Miles, 5.83	7.5	0.290	*A great portion of this was melted snow.

Sum of the Atmospheric Current in miles, resolved into the four Cardinal Directions:

North.	West.	South.	East.
2472 69	1911.13	809.84	1064.17

Mean velocity of the wind—6.34 miles per hour.
Max. velocity—25.3 miles per hour, from 11a.m. to noon on 25th.

Most windy day, 24th. mean velocity—13.58 miles per hour.
Least windy day—10th: mean velocity—1.40 ditto.

Most windy hour—noon: mean velocity—8.37 ditto
Least windy hour—9 p.m: mean velocity—5.20 ditto.

Mean diurnal variation—3.17 miles.

COMPARATIVE TABLE FOR JANUARY.

Year	Temperature. Mean.	Max.	Min	Range	Rain. days	inches	Snow. d'ys	inches	Wind. M'n velocity
									Miles.
1840	17.02	40 6	—13.8	54.1	4	1.395	11		
1841	25.11	41.7	— 4.1	45.8	2	2.150	14	not Reg.	
1842	27.54	45 8	1 3	44.5	5	2.170	9		
1843	22.46	54 4	1.5	52.9	6	4.295	12	11.2	
1844	19.95	45.6	— 7.7	54.3	7	3 005	11	24.9	
1845	26 26	43 0	— 3 4	46.4	5	Imper'ft	9	22 7	
1846	26.14	41 2	0.3	40.9	5	2 335	10	6.0	
1847	22.88	42.6	— 2.2	44 8	7	2.135	5	7.5	
1848	27 87	51.5	—1.6	63.5	7	2.245	8	7.1	5.82
1849	18 49	40 1	—15.2	55.3	4	1.175	10	9.2	6.71
1850	29.14	46 3	10 6	35.7	5	1.250	8	5 2	5.80
1851	25.62	43.2	—12.8	56.0	4	1.275	10	7.8	7.69
1852	18.54	37.3	- 7.0	44.3	0	0.000	19	30 9	7.67
1853	22.98	40.9	— 6 6	47.5	1	0.290	6	7.5	6.34
M'n	23.99	43.67	—5.08	48.95	4.4	1.825	10	13.0	6.67

THE TORONTO GENERAL HOSPITAL.

Many rumours having declared the intention of the Government to make some changes in the management of the Toronto Hospital; it behoves us to watch over this movement, to see that the interests of the Medical profession of this Province do not suffer—and now it appears to us, would be a fitting opportunity to consider what may be the rights, privileges and duties of the Medical profession in regard to it. In adverting to the subject we trust to be guided by a genuine spirit of liberality and progress—while we are firmly resolved to eschew all personalities or criminations, and in seeking the public good, to merge all other feelings.

In considering this matter, the subject would naturally appear to divide itself into several heads.—1st. as to the condition of the present Hospital,—2nd as to the duties of the Hospital Trust,—3rd as regards the appointment of Medical Officers,—4th as respects the duties of the Medical Officers,—5th as to the care and treatment of the Patients,—and 6th as regards the position of the of the Medical Students who resort to Toronto to obtain a knowledge of the profession.

1st. The condition of the present Hospital building, is extremely bad, both old and inefficient; and although capable of containing 75 beds, it is entirely without due and necessary ventilation, while there is no chance of properly heating the building; it is likewise without any of those accommodations considered necessary during sickness—to say nothing of the modern appliances of comfort and utility, now recognized as indispensable in every well regulated Hospital. The building is so ill arranged that it is with difficulty any thing like a proper separation of the patients can be made, and as it is, the women are obliged to occupy the general corridor or passage as a ward, besides which the building leaks extremely, and the toute ensemble is a perfect picture of ruin and decay, that ill accords with the rapid progress of the good city of Toronto. There is not so much as an operating theatre, so that if any capital operation has to be

performed, it must be done in one of the wards, perhaps greatly to the pain, and possibly to the injury of the other patients.

Again, the bad ventilation and injudicious heating of the rooms, engenders and maintains a foulness of the air which amounts almost to positive destruction of the patients enclosed within it, and is extremely dangerous to the Medical Officers, Students, attendants and others whose duty it is frequently to visit the wards. We would ask how in the name of common sense could it be otherwise, with perhaps a dozen persons taking medicines, with nothing but a closed stool to resort to, placed at their bed sides, and at the same time only a common box stove in the room, perhaps heated to a temperature of 120 degrees, without any possibility of that effluvia escaping, except by opening the windows, which of course cannot be permitted in winter—it must be sure to engender a bad malaria, and render it deadly powerful; and such is surely the case, for in many of the wards, with the best efforts of ventilation, the odour which remains is but too perceptible to the senses, the very walls being corrupted by it. It is almost impossible for a patient to escape this noxious influence, as is shown by the constant attacks of the Erisipelas, and other irritating constitutional complaints that indiscriminately effect them, after residing a short time in the Hospital, but to end this disagreeable matter,—there is no doubt but that the building occupied as a General Hospital in Toronto, is a snare and a delusion, alike to the patient, the Physician, and the medical student. To the poor patient it is a woeful deception, for when admitted perhaps for some surgical complaint, and having obtained all the relief which the present advanced state of science could afford him—he suddenly finds that he has contracted a far more deadly constitutional complaint from the Hospital air, and which is even more likely to destroy him; such is almost invariably the case, for the most simple variety of disease admitted into that building, is more or less modified by that deadly miasm. To the Medical officer it is a constant source of annoyance, perhaps he has treated the patient with the most consummate skill and science, and he naturally expects to find the legitimate result of his appropriate treatment, but as the deadly poison received into the patient's constitution so modifies the result, that it is any thing but what it was intended, and instead of obtaining a cure, he has to combat a far worse malady at an enormous disadvantage. To the Student doubtless it is an extreme delusion—he pays his money in good faith to the Hospital trust, to gain a knowledge of the rudiments of the art of healing; he sees his Preceptor adopting a certain line of practice, a practice doubtless the result of a profound knowledge of the principles of his profession, he is naturally led to expect certain results, but how is he deceived and disap-

pointed, when he beholds the nature of the complaint itself changed by a deadly invisible agent; in despair he fancies that study and knowledge are valueless, as regards the prosecution of his profession, for here he cannot gain a true knowledge of disease, hence he is very liable to give up his mind to some of the popular medical illusions of the day, or in despair to yield himself to pleasure and dissipation, and to waste those opportunities which in after life he will prize beyond all value. He certainly has one advantage in the present Toronto Hospital, he may study the laws of that poisonous miasma, and we doubt not, that on the diligent Student it will make an impression that will never be forgotten. It is thus clear that the whole establishment is a marvellous indication of the want of progress and improvement—it is a disgrace to the city, and ill accords with the march of the other public departments—the very appearance of the old building, set at an acute angle with the main street, plainly indicates the antiquated ideas of the dear old ladies who superintended the erection of the building, and who from some peculiar notions, which it far exceeds our finite comprehension to understand, placed the building north and south, instead of locating it in a line with its neighbours. Then again all the necessary outhouses and conveniences about the hospital, are shabby in the extreme, incomplete, and far below the advanced character of the age we live in. If these facts are true, and we are convinced that no individual can make a visit to the place without being struck with their reality, a new hospital is certainly demanded, the interests of suffering humanity requires that the present pestilential building shall be razed from the ground, and a better and more appropiate one erected in its stead. The numerous Medical Students, have a right to require from the fees that they pay, for a better opportunity of gaining a true knowledge of the profession, while the city of Toronto, nay the Province of Canada generally loudly demand at this important seat of medical instruction, that the progress of the Medical Profession should be onwards, which is impossible, without an hospital building, and all the necessary appliances attached to it, in perfect keeping with all the various improvements of the age. That such will be speedily demanded by all classes of the community we have not a shadow of doubt, and are convinced that the hospital trust will be forced onwards in the march of public improvement, although it may be contrary to their careful and tardy disposition.

2nd. This naturally leads us to consider the duties of the Hospital Trust,—this trust as at present constituted, consists of the Mayor of Toronto, and the President of the Board of Trade ex-Offico, C. Gamble Esq., John King, M. D., and the Rev. H. J. Grasett. M. A., appointed by Government; the Hon. C. Widmer,

M.D., and Lucius O'Brien M. D., appointed by the Corporation of Toronto, these exercise an unlimited control over every department of the Hospital, they not only regulate the expenditure agreeable to the supply, but also have the appointment of all the medical officers and servants in the establishment. That the Hospital Trust should have the power over all the Hospital Funds, and regulate carefully and judiciously the expenditure of the establishment, holding a wholesome check over servants and medical officers, we are willing to believe—but that they should have the power to appoint the medical officers, we most strenuously deny; such power should not rest with any corporate body, for it is sure to beget a family compact, which is diametrically opposed to the true interest of the Medical Profession, and is very liable to have a baneful influence over the welfare of the patients, for without some necessary check, incompetency and ignorance will, by favour, often stand in the place of sterling merit, and undoubted talent.

With regard to the Hospital Funds, if the reports we have heard be true, there is little doubt but that the Trust has exhibited great care, and judicious management of the funds and estate belonging to the Toronto Hospital, we believe it could not be otherwise under the watchful eyes of the careful and indefatigable veteran Dr. Widmer, and the judicious management of Mr. Brent who have as we are given to understand, so carefully husbanded the resources that they have some £5,000 on hand. This would be fully sufficient to commence a new Hospital, which we have shown to be absolutely necessary—nay was this fact untrue, the ground on which the old establishment now stands, if properly laid out in building lots, might be made to yield a quit rent of £1000 a year, putting it at the lowest notch. The new Hospital might occupy but a small portion of the land, or might be built upon the block of 6 acres owned by the Trust, situated a little further to the westward, these with the other extensive estate owned by the trust in this city, would surely be sufficient to commence an Hospital consonant with the present condition, and future requirements of the good city of Toronto; besides which it is pretty certain that the Government would aid and assist in so noble a work, the Corporation of the City of Toronto also would not be behind in finding funds, while the good citizens themselves, never deaf to the calls of suffering humanity, would be ready to add their mite, especially when they were convinced that the whole establishment would be an honour to their city, and a striking indication of their philanthropy. The Hospital Trust must wake up, and a new building be erected, for it must never be said that the Roman Catholic portion, are the only progressive part of the community in this respect, and that they, unaided, will build a magnificent Ho-

tel Dieu, which we are assured is at present in contemplation, the land having been already purchased, and the plans marked out for that purpose.

3rd. Another of the duties appertaining to the Hospital Trust, and which we most unhesitatingly condemn, is the appointment of the Medical Officers by the Board, as leading to favouritism, and inefficiency. We think it is our duty in the name of the whole of the Medical Profession of this Province, solemnly to protest against it, as a decided infringement of their just rights. What right has any man, or corporate body, to sit in judgment upon the Licensed practitioners of this Province. If any properly educated Medical man, being a British subject, has been found fit and capable of practising the profession in this Province, and having passed the Medical Board, has fulfilled the law, and in the eye of the law is perfectly on a par with every licentiate in the Province, except with respect to the date of his License, if so what right has any Board of Trustees to place their ban upon him, and for want of favour and interest, set him without the pale of the only honours and advantages to which the profession is accessible. It is as unjust as it is deliterious to the public good. It is certainly adverse to that liberality and progress which the present Government have long professed to advocate and honour; therefore we have some hopes to see it altered. Rather let every licentiate in his turn have an opportunity of gaining all the honour and improvement which a public Hospital can alone afford. It should be his indisputable right after he has passed the Medical Board, in his turn, when a vacancy should occur, (but in no way interfering with the present incumbents,) to enjoy the privilege if he is willing and able to accept it.—It should be a law, that when a vacancy shall occur among the Medical Officers of the Hospital, an offer of the appointment should be made to the oldest upon the list of licentiates, should he refuse to accept it, the next in his turn should have the offer, until one is found willing to take the onerous duties upon himself; this would be a far fairer method than the present—should it chance that an incapable should obtain the situation, a due and proper system of checks, would soon expose his incapacity, and cause his dismissal or resignation. Should inattention, incapacity or inhumanity be exhibited by the Medical Officer in the performance of his duties, he should be called before the Board of the Hospital Trust, and either admonished or declared unworthy of the situation. For although we would deny the Trust the power of the appointments, as adverse to the public interest, still we would maintain that they should hold the necessary checks, and have the power to dismiss or reprimand, for every dereliction of duty. Let it be

remembered, that except the honour of this gratuitous attendance upon the poor, the Medical Officer of the Hospital receives nothing for his mental and bodily labour—to the public it must be a constant source of wonder and surprise, that so much rancour and bad feeling should be displayed between antagonistic medical men, while striving for the honour. There is positively no other reward attached to this appointment, than a facile opportunity of studying his profession, and to do this which is his only gain, every facility which it is in the power of a well b "t, and well regulated Hospital to afford him, should be at his command properly guarded by a due system of checks, this would alike be beneficial to the poor patient and advantageous to the medical student—while the Medical Officer's progress must be onwards, he must learn from the opportunities placed at his command, or he will soon be compelled to resign, and make room for others more inclined to profit by the advantages of their position.

4th. These facts necessarily bring us to the consideration of the duties of the medical officer; these are preeminently two-fold: to render all the aid in his power to the poor sufferer committed to his charge, and with kindness and attention to communicate all the practical knowledge of disease he is able to the Medical Students, who have entered to the Hospital for instruction. That these duties may be done with facility, regularity and order, too many medical men should not be attached to the Hospital. Four Medical Officers and two assistants with the house surgeon, in our judgment would be found amply sufficient for an Hospital with one hundred beds. The Medical Officers might divide their duties as Surgeon and Physician. The duties of these medical officers should be to attend the Hospital punctually at the hour of 12 o'clock, according to their turns of duty. Each should be obliged to inscribe his name and the hour of his arrival, in a book kept for the purpose, at the Hospital, and this should be laid before the Board at their monthly meeting; if the Hospital rule has not been punctually attended to, and no sufficient excuse offered for absence or delay, due punishment should surely follow; for it must be remembered that time is of vital importance to the Medical Student, when he has numerous lectures to attend to. The Medical Officer should visit and prescribe for the patients which have been allotted to him in the several wards of the Hospital, he should take or cause to be taken, a full and accurate record of the case of each patient daily, in a book the property of the Hospital, which should be preserved in the Library of the Institution, to be considered public property, and to be freely open to every Licentiate of the Province, or any Medical Student belonging to the Hospital; who may study or transcribe the case

at their pleasure, and at the termination of every case, the result should be duly recorded, and when death shall have unfortunately occured—it should be the duty of the Medical Officer or of one of the assistants to make a decent morbid examination of the body, so as to ascertain the true cause of death, and this should be truly recorded in the same book, while proper drawings and preparations of the diseased parts should be carefully and scientifically preserved, so as to aid in the description; and these should be placed in a museum attached to the Hospital. If these means were fully and properly carried out, the public would soon be able to discriminate between the truly industrious and talented Physician, and the man who only fills the office to keep his better out. It would ensure a careful and judicious attention to the patient, and give the best promise to the diligent Student, that his precious time would not be wasted in delay—while the opportunity of study here afforded him, if duly appreciated, would open to him, an inestimable fund of practical knowledge. It should be the duty of the assistant Medical Officers, to see all the out patients and to prescribe for them, taking down their cases in a book kept for the purpose, and when any present themselves, that require admission into the Hospital, it should be their duty to admit them to the charge of the Medical Officers of the week, and should it happen that the Medical Officer, from accident or sickness, does not arrive at the time appointed, the assistant should be required to go round the wards, visit and prescribe for his patients, in his absence, so that no neglect or delay should occur to the patients. The assistant Medical Officers should also be required to make all chemical and microscopic examinations the cases should demand, and these should be recorded in the case book, and demonstrated to the Students. It should be the duty of the house Surgeon to dispense all medicine according to the recorded prescriptions, have full power over all stewards, nurses, labourers, and others belonging to the Hospital, and to see to the internal order and arrangement of the whole establishment; it should be his duty to visit the patients twice or more daily, according to the requirements of their complaint, and to administer every assistance in his power, during the adsence of the Medical Officers. He should see to the diet of the patients, the due execution of all orders, and the careful attention of the nurses in the administration of the medicines, &c.

5th. With regard to the proper care and treatment of the patients admitted into the Hospital.—The Hospital is intended as a public charity, to afford gratuitous medical assistance to the poor during sickness, so that the moment they enter the establishment they are the recipients of public bounty, and to a certain extent must be considered as public property; they must cheerfully sub-

mit to all the rules and ordinances of the Hospital, or be liable to instant removal, it is certain that if a proper spirit be impressed upon these regulations, such only will be ordained as shall be for the patient's good, and the public advantage. If the patient should not be treated with due consideration and attention,—have any fault to find with the medical attendants or any of the pupils or servants, a proper representation of the matter should be made to the Board, who should be empowered to redress all such grievances. A certain number of patients might be admitted who could pay in proportion to their circumstances, but they must in every case be obliged to submit to the rules and regulations of the Hospital; and were not the funds of the Hospital found able to relieve all the claimants for assistance, the wealthy citizens and merchants of Toronto might be solicited to purchase the admission to one or more beds, by an annual payment of a certain sum of money; when by their direction so many patients might be admitted, and treated in the Hospital should their cases require it.

6th. As to the position of the Student.—As soon as an individual has paid his fee for admittance to the Hospital practice, he should have all the advantages the establishment could afford him, in the study of disease and the acquisition of the rudiments of his profession. It should be considered that the State, by means of this public Hospital has a duty to perform in affording the Medical Students every legitimate means of properly acquiring a sufficient knowledge of his profession. The patients should be to him a living book, in which he should be permitted to study under the guidance of his Preceptor, and with due regulations, every phase of injury and disease to which man is liable. The Student should have an opportunity of performing all the minor operations under proper instructors, and should be prepared to assist, as far as is consonant with his amount of knowledge, in the duties of the Hospital. It should be obligatory upon every one of the medical officers to the best of their ability, to explain the several cases to the Students, either at the bed-side of the patient or at some subsequent period, and it should be required that the Students pay all due attention to their preceptors, and strictly conform to all the rules and orders of the Hospital, for if they controvene any of them they should be called up before the Board, and be reprimanded or dismissed, according to the magnitude of the offence.

Such then to our mind are the conditions which would render the Toronto Hospital a source of great public benefit, an undoubted blessing to this community, and to the Province generally. It would afford an establishment an honour to the city of Toronto and worthy the philantrhopy of her citizens. It would do justice to the Medical profession, affording them a just and unprejudiced

opportunity of distinguishing themselves—placing them in a noble field for the study of their profession, and the exercise of their humanity. It would prevent the possibility of the minister of the day, making a trade of the calamities and misfortunes of the poor patients, by selling the office of medical attendants for parliamentary influence, or family affection, whereby it must be often yielded up to ignorance and incapacity, a fact alike contrary to the interest of the profession, the benefit of the Student, and the advantages of the patient; while it would freely afford to the poorest person in the community, a certainty of the best medical and surgical advice and assistance, it would form a sure refuge for the poor during the severity of disease, when totally incapable of caring for himself, and would produce a noble field for the education of the Professional Students, and training him up to be alike an honour to the profession, and a benefit to the Province.

Having said thus much with regard to the Toronto Hospital, we do not see why all the hospitals in the province intended for the admittance and treatment of the poor, and receiving Provincial assistance, should not be placed under similar arrangements, and would also think the law should embrace the possibility of the Municipal Council of each county establishing a Public Dispensary or Hospital in each of their chief towns. In all towns and villages in Canada there are many poor who suffer for the want of medical assistance, or have to throw themselves on the charity of the medical profession, it would be a noble trait in our Municipal institutions that they were careful of human life, willing to relieve the miseries, ready to hold out the hand of charity to our suffering fellow creatures, and not let the burden fall entirely upon one class of the community—the Medical Practitioner.

DR. GOADBY'S PAPER ON THE PRESERVATION OF ANIMAL SUBSTANCES. &c. &c.

Agreeable with the promise of the former proprietor of the Upper Canada Journal, we have the pleasure to present the subscribers with a part of Dr. Goadby's manuscript on the mode of preparing anatomical and physiological preparations, which we believe will be found extremely interesting and worthy the attention of the amature as well as the Physician.—We extremely regret that the whole of the promised production will not be forthcoming in consequence of Dr. Goadby refusing to afford the remainder of the matter, a circumstance that placed the former proprietor of the Journal in a most unpleasant position, forcing him to seek the pro-

tection of a Court of Law, so as to prove to the subscribers that the default in the matter did not rest with himself.

It would appear, that some slight explanation of the matter was due to the subscribers on this occasion. As noticed in a previous number of this Journal, Dr. Goadby in a series of microscopic demonstrations, had greatly interested the Medical Profession in Toronto, several of whom were anxious to possess in print, the Dr's. mode of making and injecting anatomical preparations, also his instruction for the use of the microscope in these matters; the fact was mentioned to Dr. Goadby, and Mr. Plees the printer of the Upper Canada Journal was introduced to him, when Dr. Goadby made the following agreement with Mr. Plees.—Dr. Goadby was to afford matter, and illustrations for 150 pages, and Mr. Plees was to find all material, print and bind the book, giving to Dr. Goadby 100 copies.—Mr. Plees having the privilege to insert the matter in the pages of the Medical Journal, as an original communication. Mr. Plees commenced the work, and printed some 750 copies of a part of it, when Dr. Goadby was assured by some person that he would never receive the promised copies of the work, whereupon Dr. Goadby refused to supply any more matter, and the work could not be proceeded with, so the subscribers to the Medical Journal will thereby be disappointed of a considerable portion of the promised communication.

In offering this explanation, there is no desire to open anew the spirit of private and political rancor which exhibited itself on that occasion: and while we regret that the impulsive character of Dr. Goadby should have been so easily misled, we cannot conclude without declaring that notwithstanding the misunderstanding here evident, we are perfectly ready to acknowledge Dr. Goadby's talent and ability in the peculiar department of science in which he stands preeminent, and to declare from our love to that science, should a similar opportunity offer, we should serve the Dr. with the same disinterested zeal which marked our effort during his residence in Toronto.

We have to acknowledge the receipt of a pamphlet from Dr. Hall, of Montreal, being a series of strictures upon the New Medical Bill introduced into the present session of the Provincial Parliament by Dr. La Terriere, the report of the Committee on the said bill, and a series of questions addressed to Medical Practitioners in Canada East and West. The spirit of the Bill is evidently an attack upon the present incorporation of the Profession in Canada East, and will oblige every Medical Practitioner, whatsoever may be his diplomas or qualifications, to undergo an examination before

the Medical Board, prior to obtaining a License to practice his profession in the Province. There appears something very illiberal, and retrogressive in this attempt, and will doubtless have the effect of degrading many a Medical man who has thoroughly studied his profession in other countries, while it places in the hands of the Medical Board a power to annoy and degrade persons, who may be far their superiors in education, and Medical qualifications; so it will tend to prevent the settlement among us of many superiorly qualified Medical practitioners; at the same time we are fully alive to the difficulties which this Bill is intended to remedy, and feel that the indiscriminate admission of Medical practitioners, upon the simple production of a diploma, without due and sufficient guards, is liable to serious objections and susceptible of great abuse.

The following is the proposed Bill.

BILL.

An Act to amend the Law relative to the practice of Physic, Surgery and Midwifery in Lower Canada.

WHEREAS it is inexpedient that any person should obtain a license to practice Physic, Surgery or Midwifery in Lower Canada, without undergoing an examination before the Provincial Medical Board: Be it therefore enacted, &c.,

That the seventh section of the Act passed in the tenth and eleventh years of Her Majesty's Reign, and intituled.—

"*An Act to incorporate the members of the Medical Profession in Lower Canada, and to regulate the study and practice of Physic and Surgery therein*" shall be, and is hereby repealed.

And be it enacted, That for and notwithstanding anything in the said Act, or in the Act amending the same, passed in the twelfth year of Her Majesty's Reign, and intituled, "*An Act to amend the Act to incorporate the Medical Profession in Lower Canada, and to regulate the study and practice of Physic and Surgery therein*," no person shall, after the passing of this Act, receive a license from the Provincial Medical Board to practice Physic, Surgery or Midwifery in Lower Canada, unless he shall have undergone an examination before the said Board, and obtained a certificate of qualification from the said Board; Provided always, that nothing in this Act shall apply to females practising Midwifery in Lower Canada under the provisions of the Act first above cited; provided also that any person who shall have served in Her Majesty's Army or Navy, being on half pay, and producing his Diploma or Commission in the Service as such to the Provincial Medical Board, may obtain a License to practice Physic and Surgery without being bound to undergo an examination.

At an early period we propose to return to this subject, and at the present time should feel thankful to any of our readers, if they would inform us what has become of the proposed act of incorporation of the Medical Profession in Canada West; at the present moment, from what with the destruction of the Law and Medical classes of the Toronto University, and the general want of professional confidence; the medical profession is in a state of chaos that certainly requires some basis on which to found a better aspect of affairs. We would respectfully press upon our Professional brethren a greater unity of action, and more cordial conventional agreement, than at present appears to exist among them, for without such, we cannot expect any beneficial or satisfactory enactments.

SELECTED MATTER.

MEDICINE.

CLINICAL ILLUSTRATIONS OF SUB-ACUTE OVARITIS, WITH REMARKS ON THE DIAGONOSIS OF THAT DISEASE.

After reading ten cases of sub-acute ovaritis, Dr. Tilt gave a summary, in which he showed how far they threw light on the causes, symptoms, and terminations of the complaint. He then treated more fully of its diagnosis, and observed, in *limine*, that pain, however intense and well localized in the ovarian region, was not a sufficient ground to admit ovarian inflammation, because the pain might depend on uterine inflammation, or on what Dr. Fleetwood Churchill describes as ovarian irritation, considered by Dr. Tilt to be the same disease as the French pathologists term lumbo-abdominal neuralgia. With regard to the diagnosis of sub-acute ovaritis, Dr. Tilt remarked that it was rendered difficult by the similarity of the seat of pain in both complaints; and that, however probable it might seem from the absence of uterine disease, the fixed pain, the appearance of fever, and the tensions or swelling in the ovarian regions; still, a rectal examination could alone give certainty to the diagnosis. Those (Dr. Tilt adds) of a nervous temperament are most liable to lumbo-abdominal neuralgia, not brunettes, of a sanguine constitution, as in most of my cases. Pain exists in all, but while, in sub-acute ovaritis, it is more fixed, continues with the same intensity, without regular exacerbation, and is exasperated by any kind of pressure, in lumbo-abdominal neuralgia it is quite the contrary; for although there may be at all times a dull, aching sensation, this is not invariably the case, pain sometimes occurring by repeated attacks, and is relieved by wide or even by continued pressure with the united tips of the fingers. Dr. Tilt agrees with Dr. F. Churchill, that ovarian irritation is characterized by a kind of nervous tenderness, which shrinks from the weight of the finger as much as from severe pressure, and not by the *positive* pain mentioned in Dr. Tilt's cases. There is also, in lumbo-abdominal neuralgia, no swelling, no heat, no pain of the ovaries when these organs are subjected to a rectal examination, whereas there is heat, swelling and pain, in sub-acute ovaritis. The pain is unaccompanied by any sympathetic pain of the breasts, or fever, in lumbo-abdominal neuralgia; nor so in sub-acute ovaritis. The former is so frequent an accompaniment of uterine disease, that many pathologists, both at home and abroad, consider pain in the inguinal region as almost pathognomonic of uterine disease, while sub-acute ovaritis is not so frequently induced by uterine disease. With regard to the treatment, repeated blisters, and opium, are of most use in lumbo-abdominal neuralgia; but such remedies, valuable in the latter stages of the disease, require to be employed after leeches, emolients, &c., in sub-anute ovaritis. Dr.

Tilt then established the greater frequency of young females to idiopathic peritonitis, and to bridles of lymph in the vicinity of the ovaries, and concluded by observing—It seems urgent on us carefully to bear in mind the frequency of inflammatory products in or about the ovaries; the frequency of intense suffering in the ovarian regions at the menstrual periods; and the great probability of both facts standing one to the other in the relation of cause to effect. We should also remember the greater liability of young women to idiopathic peritonitis, and incaration from bridles of inflammatory lymph, at the very age when I have shown that even the sub-acute inflammation of the ovaries is most frequent, and therefore the imperative necessity of watching over the first stages of a complaint, which being too often left to nature, is as frequently productive of serious mischief. Lastly, that sub-acute ovaritis can be distinguished from uterine affections, as well as from lumbo-abdominal neuralgia; and that at all events no harm can ensue, from the treatment recommended.

A discussion took place, in which Messrs. Hird, Canton, Dendy, as well as Drs. Murphy, Crisp, and Ogier Ward, took part; and all admitted the difficulty and interest of the subject. It having been stated by Mr. Canton, that whereas he had frequently been able to ascertain on the dead body the possibility of investigating the condition of the ovaries through the rectum, he did not find that in the narmal anatomical condition of the human body the ovaries were susceptible of being mediately examined by pressure to the inguinal region. Dr. Tilt replied, that while admitting the force of Mr. Canton's assertion, when increased to double or triple their usual size by inflammatory congestion, the ovaries were so pushed forward, that by careful pressure in the inguinal region, a small tumour might sometimes be felt, and the diagnosis was susceptible of being rested by a rectal examination. In answer to Drs. Murphy and Dendy's doubts as to whether the cases read by Dr. Tilt were not cases of physiological irritation or erethism of the ovaries, and whether he could establish the difference between congestion and inflammation of the ovaries. Dr. Tilt said, that unable to do more than general pathologists, he could not fix the precise boundary between ovarian congestion and inflammation, but that when he met with cases where the ovaries were enlarged painful with increase of temperature, and a tendency to fever, and this totally independent of menstruation, he considered this state to be one of inflammation, and as having nothing to do with the physiological action of the ovaries. He added, that as numerous observers had met with such cases, it was fair to infer that the same might be still more likely to happen during menstruation, as, in fact, occurred in some of his cases. Dr. Tilt admitted that the greater liability of women to incarceration of the ilium by bands of lymph, might be left as a reserved question, although, with Dr. F. Renaud and others, he believed it to be the case, and he referred Dr. Crisp to the work of Dr. Negrier for a case of death by peritonitis, from the bursting of a very small ovarian cyst. To Dr. Ogier Ward's inquiry, relative to the constitutional symptoms of sub-acute ovaritis, Dr. Tilt said that they were not of a severe nature, varying according to the patient's constitution—slight fever in some, hysteria in others, or pseudo-narcotism or derangement of the biliary function.

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* We have given but a short abstract of Dr. Tilt's interesting paper, as it will shortly be published entire in *The Lancet*.

CASES OF ACUTE RHEUMATISM; TREATMENT BY LEMON-JUICE; RAPID CURE.

(Under the charge of Mr. Hancock.)

Mr. Hancock has lately been trying the lemon-juice in acute rheumatism, with two patients, according to Dr. Owen Rees's plan, and both recovered in about a week. The first patient is a servant girl, of twenty-three years, who was admitted, Dec. 30, 1851, with acute rheumatic pain in the right wrist, which flew, two days afterwards, to the left, the joints in both cases being red and swollen. The shoulders were subsequently involved, and the parspiration was profuse. The patient took, at first, calomel and Dover's powder, and cooling alkaline draughts. On the second day, she was ordered one ounce of lemon-juice every fourth hour, and went on taking these doses, with an occasional anodyne at night, for ten days, when all the above-mentioned symptoms had disappeared. Mr. Hancock now prescribed quinine and quassia, the patient only complaining of weakness and want of appetite.

The second case refers to a man of forty years, who was admitted June 6, 1852, with a non-rheumatic affection of the knee. While under treatment, he was suddenly attacked with pain, swelling and redness of the left wrist. Mr. Hancock ordered half an ounce of lemon-juice three times a day. This dose was taken for a week, when we were kindly shown the patient, who had regained the full use of the wrist. There were no heart complication in either case.

SURGERY.

VESICAL CALCULUS IN A LITTLE GIRL, TWO YEARS AND A HALF OLD; ENLARGEMENT OF THE URETHRA BY INCISION; LITHOTRITY.

(Under the care of Mr. Poland.)

The hospitals of a large metropolis must necessarily present a great many stone cases in children, these being principally of the male sex; and as such patients are known to bear lithotomy perfectly well, we have frequent opportunities of witnessing operations of this kind. Lithotrity is but seldom resorted to with boys; still, we recollect one or two cases, where the results were pretty favourable. As to little girls, it would appear, if we can trust our own statistics, that they are considerably less liable to calculous concretions than boys, for it happens but very seldom that very young patients of the female sex, thus affected, are brought into our hospitals. We have, however, of late, noticed two cases of this kind, one treated by Mr. Poland, at Guy's Hospital, and the other by Mr. Ward, at the London Hospital. In both instances it was found necessary to crush the stones; but the preliminary steps were somewhat different as will appear from the account below.

Before entering into details, we would just direct the attention to the fact, (to which we have alluded in a former "Mirror,") that the children of the labouring classes are much more liable to stone in the bladder than the offspring of middle or upper sections of society. This circumstance is well known, and it is very natural that we should inquire into the causes of this difference. Herein we shall be greatly assisted by the investigations of Sir Benjamin Brodie on this subject, the results of which are consigned in his Lectures on the Urinary Diseases. Sir Benjamin, after pointing out that when the urine con-

tains a superabundant acid, the red or lithic-acid sand is precipitated, and that in this manner the lithic-acid diathesis is established, goes on to say, "In what are called the better classes of society, you will find the deposition of red sand to exist chiefly in adult persons; but in the lower classes, you will see it among children. These circumstances are easily explained. Adult persons who are possessed of much property, for the most part lead a more luxurious and indolent life than their children; while among those of lower condition, the diet of the children is frequently unwholesome, and comparatively little attention is paid to the various derangements of the digestive organs to which they are liable."

The contrast between the upper and lower classes, with respect to calculous concretions, is thus satisfactorily explained; though it still remains to be shown, why one sex, among poor children, should be more frequently affected than the other. With regard to this fact, it might perhaps be stated, that the "red sand" escapes more readily with girls than with boys.

We now turn to Mr. Poland's case, the details of which, as collected by Mr. W. H. Moon, run as follow:—

Emma R——, aged two years and a half, was admitted into Guy's Hospital in October, 1851, under the care of Dr. Golding Bird. She is a strumous, stout, florid-looking child; her parents and seven brothers and sisters are quite healthy. It appears that this little patient enjoyed good health until she was twelve months old, when she began to suffer from partial incontinence of urine and pain in micturition. These symptoms increased in frequency and severity up to three months before admission, when she became an out-patient under the physician. At that period the signs of disease were incontinence, much mucous and phosphatic deposit in the urine, and great pain on passing water; she also had excoriation of the genitals. The child had been under treatment with little or no benefit until her reception into the hospital.

She now had an attack of varicella, unaccompanied by fever or any particular constitutional derangement, and was also still suffering from her other complaints. Under mild saline medicines the varicella partly subsided, when Mr. Poland was requested to see her respecting the urinary symptoms. She had then constant pain in the bladder, at times excruciating, causing her to scream and pull herself about; she could not hold her urine, which was constantly dribbling from her; when passing her motions she suffered greatly, and alvine evacuations were always accompanied with more or less prolapsus. There was much thick mucous deposit in the urine.

Some chloroform was administered, and the child sounded, when a calculus was distinctly felt. It was determined not to dilate the urethra, so as to risk the evil of a permanent incontinence, but to extract the stone by incising the urethra upwards and to the left side, so as to allow the calculus to pass, if of small dimensions, and make it practicable to draw it through the neck of the bladder. Should these measures fail, it was proposed to crush the stone with a strong pair of forceps at one or more sittings; and if the calculus were found to be of too hard a consistence, it was determined to use a female lithotrite made by Mr. Bigg.*

* This instrument is similar to the common male lithotrite, with its shaft shortened to six inches, and the angles of the jaws set at 140 deg., instead of the usual angle, so as to facilitate its introduction into the female bladder. There is a slight modification in the beak.

On October 31st the child was put under the influence of chloroform, and held in the position for lithotomy. There was no water in the bladder, and some little prolapsus of the rectum ensued. A short grooved sound was passed into the urethra, and that canal slit upwards and to the left side, when a strong pair of dressing forceps were rapidly introduced, and a calculus, about the size of a bean, seized without difficulty. Extraction was attempted with some degree of force, and persisted in for a short time, as the size of the stone seemed to warrant this course, but these efforts were unsuccessful. The stone was now with some force crushed, and the fragments broken up by successive applications of the forceps. The debris were extracted in large quantities by the instrument, which was introduced seven or eight times. Mr. Poland then passed an elastic catheter into the bladder, and the latter was thoroughly washed out. A great deal of the calculous matter being collected, it was found to consist of uric acid, urate of ammonia, and the phosphate of lime.

The child had a comfortable night, and did not seem to suffer in the least; no untoward symptom arose, and all her former complaints subsided.

Six days after the operation, the patient was again put under the influence of chloroform, and sounded with the dressing forceps, when a small fragment was detected, crushed, and removed.

From this time she perfectly recovered, suffered no pain whatever, and obtained complete control over her urine, which she sometimes held for more than six hours, the renal secretion becoming clear and natural.

A fortnight after the second crushing, the child was again sounded, and the bladder found perfectly free from any fragments.

The patient was presented cured at the end of four weeks; two months after her discharge, she was in good health, and had not the slightest urinary difficulties.

Mr. Ward's patient is considerably older than the last, being about ten years of age. The symptoms were here well marked, and Mr. Ward detected a rather large stone by sounding the child whilst she was under the influence of chloroform. It was now resolved gradually to dilate the urethra to an extent sufficient for the passage of the stone. This plan was persevered in for three weeks, with the assistance of chloroform; but it was found impossible, when the dilatation had been carried as far as was thought safe, to extract the calculus. Mr. Ward was therefore obliged to have recourse to lithotrity, which operation was performed several times with the aid of chloroform and the common polypus forceps. The crushing proved now and then a little troublesome, as the urine would suddenly escape on the introduction of the instrument. The bladder was several times washed out, and a few fragments passed with the urine, but the greater portion was removed with the forceps. It was now clear that the stone must have been very large, as the fragments on being collected weighed no less than five drachms. They were principally composed of phosphate of lime, and being very light, almost filled a flat ounce phial. This little patient suffered from incontinence for a few days; but this symptom soon disappeared, and she can now retain her urine very satisfactorily.

It is evident, from these cases, that lithotrity is by far preferable to forcible dilatation and extraction, when the stone is of large size. A small stone may be easily removed without crushing; but it then becomes a question, whether it is more advisable to incise the meatus, than make gradual dilatation. That

course ought certainly to be chosen which is least likely to produce incontinence, and here the incision would perhaps have the preference; but it should be recollected that the wound is sometimes long in healing, from the irritation of the urine. However this may be, it is satisfactory to notice how favourably both the preceding cases have ended, sufficient data being thus obtained regarding lithotrity in young subjects of the female sex.

REMOVAL OF A NÆVUS BY THE PLATINUM WIRE, HEATED BY A GALVANIC CURRENT.

(*Under the charge of Mr. Hilton.*)

Our readers probably remember the cases of fistula in ano and hæmorrhoids, successfully treated by Mr. Marshall, at University College Hospital, with the platinum wire, made red hot by a galvanic battery. We perceive that Mr. Hilton has been trying this plan of cutting and searing at the same time upon a nævus of a flat kind, situated in front of the ear of a child two months old. The operation was performed with Cruikshank's battery and a very thin wire, which it was first intended to tie around half the tumour, which was about the size of a crown piece. But the wire ran so easily through it, that the whole was completely removed, and the parts are now fast cicatrizing. This is rather a quicker measure than the ligature, and just as secure, since hæmorrhage is so rare.

AXILLARY ANEURISM; TYING OF THE SUBCLAVIAN ARTERY; RECOVERY.

(*Under the charge of Mr. Barnard Holt.*)

Mr. Holt has just discharged from his wards a patient who was admitted several months ago with axillary aneurism. The case had been sent from the country, with some doubts concerning the nature of the disease. Mr. Holt tied the subclavian artery on the right side, which measure caused the pulsation to cease, though the fluidity of the contents of the sac persisted for several months. Consolidation and absorption at length ensued, and the patient has just been discharged with the full use of his arm, and the almost complete disappearance of the tumour.

PUNCTURING THE BLADDER THROUGH THE RECTUM FOR RETENTION OF URINE OR STRICTURE, WHEN NO INSTRUMENT CAN BE PASSED ALONG THE URETHRA.

(*Under the charge of Mr. Cock.*)

Mr. Cock has been very successful in re-establishing the permeability of the urethra in complete retention or guttatim micturition, by puncturing the bladder through the rectum when no instrument could be passed along the urethra. Within the last three weeks two such cases were thus treated, the urethra being afterwards gradually dilated, so as to admit Nos. 8 and 10 catheters.

The urine flowed for a few days through the rectum, the urethra thus remaining completely at rest. Micturition was generally satisfactory after ten or twelve days' treatment. We noticed a few days ago a third patient similarly affected; the man is forty-two years of age, a fellmonger by trade, and has had stricture of the urethra for the last four years. He was admitted January 16th, 1852, with partial retention of urine, only a few drops escaping at a time. As no instrument could be passed, the bladder was punctured through the rectum, and great relief afforded. After the canula had been left for a few days, an instrument was introduced into the urethra, and the process of dilatation is now going on, micturition becoming at the same time more and more easy. Mr. Cock has had constructed, by Mr. Bigg, of St. Thomas's street, a double canula, with cranks so fixed to the upper part of the outer one, that the instrument cannot slip out of the bladder and rectum—an accident which should always be avoided, as re-introduction is extremely difficult. Mr. Cock has now operated with success on a great number of cases.

MIDWIFERY.

ON FISSURING AND LACERATION OF THE STRUCTURES OF THE PERINEUM AND CERVIX UTERI IN NATURAL LABOUR.

As the result of a long series of observations, Dr. Simpson has drawn the following conclusions:—

I. Fissuring and laceration of the cervix uteri and perineum are not, as is generally conceived, rare lesions during labour; on the contrary, they are very common occurrences; especially in primiparous labours.

II. These lesions are not, as has been often alledged, necessarily the result of mismanagement, but they occur constantly in practice, despite every modification of management; and in cases also in which no kind of management has been adopted.

III. Evidence of the great frequency of laceration of the anterior stuctures of the perineum is furnished by,—1. Almost every careful autopsy of women after delivery, whether assisted or not assisted during their labour; 2. By the contracted or shortened state in which the perineum is almost always found, when vaginal examinations are made for uterine disease in women who have borne a family; and, 3. By the fissuring or laceration itself being usually traceable (under careful tactile examination), particularly in the first labours, when that examination is instituted in the interval of pain, immediately before the passage of the child's head, or after its birth.

IV. Lacerations of the perineum may be often felt beginning in the form of slight roughish rents or fissures upon the mucous surface of the perineum, and these may extend either backwards or forwards; and if they extend forwards, they at last run over the edge of the perineum, and along its cutaneous surface; the mucous and cutaneous structures of the perineum being thus sometimes lacerated, while its middle cellular, and fascial tissues are comparatively entire, or at least not so deeply and extensively injured.

V. The proper management and support of the perineum no doubt modifies

and diminishes this form of perineal lesson; but it fails far more frequently than is generally supposed in entirely preventing it.

VI. The evidence of the frequency of fissuring of the os and of the lower or vaginal portion of the cervix uteri is the same in character, and consists principally—1. In the frequency with which slight laceration of the edges of the os, and of the mucous and middle coat of the cervix, is detected in autopsies after after natural labours, and particularly with first children; and, 2. In the permanent marks of its previous occurrence, as exhibited in those cicatrices and irregularities of the cervix uteri, which anatomists have long empirically, but correctly, laid down as proofs that they, in whose bodies they are found, have been previously mothers.

VII. Fissures and lacerations of the vaginal portion of the cervix uteri not unfrequently occur to a very considerable extent, in cases in which the tissues of the cervix have been rendered rigid by previous inflamation, by carcinoma, or by other morbid causes; and, in such cases, this fissuring or laceration, if limited to the lower or vaginal portion of the cervix, seems to be accompanied with little or no danger.—*Monthly Journal of Med. Science.—London Journal of Medicine, September*, 1851

OF THE MANAGEMENT OF WOMEN AFTER THE CESSATION OF MENSTRUATION.

By Dr. E. J. Tilt.

[The superabundance of blood and nervous energy after the cessation of the menstrual flow may be safely and effectually kept down by the habitual use of small doses of purgatives; and, as they may have to be continued for some length of time, it is best to consult the patient as to what medicine would be best tolerated. The purgative to be used depends upon the constitution of the patient. Perhaps the best is some mild purgative which has been found to agree with the patient. Dr. Tilt continues:]

I frequently prescribe the soap-and-aloes pill of the Edinburgh Pharmacopœia, ordering five or ten grains to be taken with the first mouthful of food at dinner. Hæmorrhoidal affections I have never seen *caused* by this frequent use of aloes, but I have seen them *relieved* by it; and as I read in Giacomini's 'Treatise of Materia Medica' my own experience on this point is confirmed by that of Avicenne, Stahl, Cullen, and his own, so I think there must be some exaggeration as to the extraordinary property generally ascribed to this valuable drug, which can be associated with hyoscyamus, and is thus said to be less liable to induce piles. Kemp and Hufeland recommend the following powder to be given to those who are advanced in years, and who complain of a tendency to vertigo:—Guaiacum resin, cream of tartar, of each half a drachm, to be taken at night. This, no doubt, will sometimes be found a useful laxative; so will the popular remedy called the Chelsea Pensioner, of which Dr. Paris has given the following formula in his excellent Pharmacologia:—Of guaiacum resin, one drachm; of powdered rhubarb, two drachms; of cream of tartar and of flour of sulphur, an ounce of each; one nutmeg finely powdered, and the whole made into an electuary with one pound of clarified honey; a large spoonful to be taken at night. I generally administer the flour of sulphur alone, or else to

each ounce of it I add a drachm of sesquicarbonate or biborate of soda, and sometimes from five to to ten grains of ipecacuanha powder. One to two scruples of these powders taken at night in a little milk, is generally sufficient to act mildly on the bowels, and I consider such combinations as very valuable when a continued action is required.

I feel obliged to class sulphur amongst purgative remedies because such is its visible action, but I believe that it owes its chief value, in diseases of cessation, to another action, much more difficult to understand, and which has long rendered it so valuable both in hæmorrhoidal affections, where there is an undue activity of the intestinal capillaries, and in skin diseases marked by a morbid activity of the cutaneous capillaries. Whether sulphur cures by acting on the nerves or on the blood-vessels, or by modifying the composition of the blood itself, is difficult to tell, but it does certainly cure the diseases I have enumerated. It forms part of many popular remedies for the infirmities of old age, was recommended by Hufeland, and is lauded by Dr. Day in his work 'On the Diseases of Old Age;' but its utility is not generally known in all derangements of the menstrual function, at whatever period of life they may occur, and particularly at the change of life, where, if required, its action may be continued with impunity for months and years.—*Provincial Med. and Sugr. Journal, October* 1, 1851.

THERAPEUTICS.

ON THE THERAPEUTIC USES OF INDIAN HEMP.

By Dr. Alexander Christison.

[Indian hemp, in spite of the strenuous recommendations of Dr. O'Shaughnessy, has been but little used in this country hitherto.]

Dr. O'Shaughnessy used it in the following diseases:—In three cases of rheumatism he found it apparently beneficial. In one of these, it seemed to produce great insensibility, and a state resembling catalepsy; but on this state passing off, the man was found to be thoroughly restored to health. In an epidemic of cholera, it was thought serviceable; but although it seemed to stimulate the circulation, and check diarrhœa, it is doubtful whether any ultimate good resulted. In a case of hydrophobia, a soothing effect, with diminution of the spasms, and greater facility in drinking was kept up for four days; but the patient died. Several cases of tetanus were also treated by him in this way, with apparent success. In one, ascribed to cauterization of the hand by a quack mixture of incandescent charcoal and tobacco, a state of intoxication was excited by large doses of the extract of hemp, and the spasms were gradually put an end to; but death ensued in the end from mortification of the hand. Another patient consumed 134 grains of the extract, and was ultimately discharged from the hospital cured. A third case, with similar results, is detailed. At the Native Hospital at Calcutta, Mr. O'Brien treated seven cases of tetanus in this way, and in four of them he employed ten-grain doses. The result was, almost immediate relaxation of the muscles, and interruption of the convulsive tendency. Four of these cases recovered. A case in the practice of Mr. Richard O'Shaughnessy is also detailed, were the disease was connected with suppurating

of the scrotum. The hemp had no effect for four days, and then the patient became tranquil, with fewer paroxysms, and the appetite good. When the hemp was intermitted, the symptoms became aggravated; latterly, the hemp caused much excitement, and was therefore discontinued. The last case was one of infantile convulsions, where very large doses were given, and where the narcotic action greatly relieved the symptoms. The child recovered. This gentleman is confident that the resin is capable of arresting the progress of tetanus, and that, in a large proportion of cases, it will cure the disease.

It would certainly appear from the above facts, that Indian hemp has proved of service in the treatment of tetanus, as it occurs in India. How far this result has been obtained in Europe, I shall now describe.

That I may not extend my observations to too great a length, I shall limit my remarks to the treatment of tetanus, as observed it cases in private practice, and in the Edinburgh Royal Infirmary.

Professor Miller has provided me with the following remarks:—

"My own experience speaks loudly in favour of the hemp. I can now record three fortunate cases under its use—all traumatic tetanus—and a case which proved fatal, but where great alleviation of suffering was produced.

"The first of these was a girl, aged seven, admitted to the Royal Infirmary, October 18, 1844. She had received an extensive injury of the middle finger of the right hand a fortnight previously. Inflammatory swelling and pain became intense, and there was a tendency to spasmodic flexion of the fingers and wrist. On the 23rd she was observed by the nurse to take a 'kind of fit,' becoming rigid, having difficulty in opening the mouth, and swallowing, and complaining of pain in the jaws. At visit she seemed perfectly well. A brisk purge was ordered, and, lest the case should prove tetanus, ten drops of tincture of hemp were prescribed to be taken every four hours. Next day the symptoms were well marked without any influence from the hemp. The finger was then removed, and the simplest dressing applied to the wound. The dose of hemp was increased to 20 drops, and after five doses she slept; but the following day the symptoms were aggravated. Turpentine enema was ordered, and ice to the spine—30 drops of cannabis to be given hourly. In the evening there was rigidity, but no spasm; the hemp to be given every half-hour; after which she became drowsy, and at twelve next day she was much improved. Aconite was now substituted; but as the spasmotic attacks became more severe, hemp was again given, with the effect of producing sleep. She continued to improve till the 25th November, the dose of hemp being gradually reduced; producing, when given, drowsiness, or calm sleep; it was soon discontinued, as it then seemed to excite the circulation. Throughout the whole period of its use, its effect on the appetite was obvious, the craving for food being at times absolutely voracious. After this no more medicine was given, and recovery was complete.

"The second case, occurring in private practice, was that of a boy, about the same age, who had simple fracture of the thigh, with compound and comminuted fracture of the great toe. The treatment and result were the same.

"The third was a boy, rather older, who had compound fracture of the bones of the arm. Treatment again resulted in cure.

"In these cases a few doses generally induced sleep, with marked mitigation of the spasms. The period of narcotism did not exceed two or three hours; the sleep was deep and unbroken, and seemed to be refreshing. It was followed by no headache, or other apparent inconvenience. The most remarkable effect

observed, was the tolerance of the remedy, whereby a girl, aged seven, took every half hour, and sometimes many hours in succession, doses of hemp sufficient to narcotise an adult."

In these cases Mr. Miller is inclined to give the hemp credit for a chief share in the cure.

In 1846 the virtues of hemp were tested in a case of tetanus in the Royal Infirmary, in the wards of Dr. Duncan. In 1847 another case presented itself, where hemp was administered. At that time sulphuric ether was much used as an anæsthetic, and it was thought probable that it would be of service in this case. The patient inhaled it at frequent intervals during a whole afternoon, with decided, but only temporary relief. After this canabis was given, without its physiological action being attained by nearly an ounce and a half of the tincture; it was not persevered with. Ether was again tried, and also opiates with some benefit. The patient died on the 13th day.

The first of these cases was very accurately observed, and the following report of the case from the journal will be found to have considerable interest:—

James Mackay, a railway labourer, was admitted under the care of Dr. Duncan, October 20th 1846. He had received a slight lacerated wound of the hand a week before, and tetanus had commenced on his admission. The wound appeared to be healing. He complained of great general uneasiness, particularly about the neck and spine, of some rigidity of the jaws, which could only be separated three quarters of an inch, of inability to protrude the tongue, and of commencing spasm of the neck and upper part of the back. He complained also of "a burning about the heart." His expression was anxious, with but little "risus." His thirst was great, but swallowing difficult. He perspired profusely. The spasms, of short duration, recurred once or twice every minute; pulse, 115 to 120, soft. Opening medicine was ordered, and at eleven o'clock tincture of hemp was given, repeated in doses of fifteen or twenty drops with appreciable effect. On the 21st the bowels were not opened, though a turpentine enema was administered. The spasms were more violent and general, and a touch caused general spasm. He had not slept; 120 to 140 drops had no effect. The doses were increased to sixty or eighty drops every three quarters of an hour, and croton-oil was given, producing free action on the bowels; and in the evening the spasms abated, but the hemp caused only slight dozing at intervals. The tincture was ordered to be continued, and strong beef-tea to be drunk.

On the 22nd swallowing was easier, the spasms less violent, but not less frequent; 100 drops were given at half-past eleven, and continued about every half hour till four o'clock, when drowsiness was quite decided; he was not readily roused, even by the spasms which, though as frequent, were not so intense. At nine o'clock drowsiness passed off; copious stools, coloured as by the medicine, were brought away by injection; 130 drops were given, and repeated at midnight, at which time he was much relieved, but suffered from cough. On the 23d the spasms were again gaining strength, no help having been given for nine hours. A drachm of the tincture was given and repeated at eleven, when he became quiet. The doses were continued till evening, when he took mince-collops and beef-tea without difficulty, and the bowels were copiously relieved.

On the 24th, at visit, the spasms were absent, but the chest symptoms were worse, with general mucous râle, and frothy sputa mixed with blood. Drowsiness had been kept up by doses of a drachm to a drachm and a half. In the

evening he was much weaker, but quite sensible, with a desire for food. On the 25th he was perfectly free from spasm, but was evidently dying from accumulation of mucus in the chest. Very little hemp was given. He died at eight p.m.

In this case six ounces of O'Shaughnessy's tincture of Indian hemp were given in all, being equal to 144 grains of the extract. The extract for the tincture was reputed the best in Edinburgh. The doses at first were evidently too small. The examination of the body was not permitted.

It is a safe conclusion, from these facts, that Indian hemp deserves further trial in the tetanus of Europe, as well as in that of hot climates. I would particularly urge, however, the necessity, in all such trials, of making certain, by experiment on healthy persons, that the preparation to be used is good. For the present there is no other satisfactory test of quality.

As to the use of hemp as a calmative and hypnotic in diseases in general, I may mention that, while acting as clinical clerk in the Royal Infirmary, in 1849, I had several opportunities of administering hemp in different diseases as a hypnotic. The object was in general attained, and no evil results followed. I regret there is no record of these cases, as at that time I did not pay particular attention to the subject. Hemp is frequently given in other wards of the infirmary for a like purpose. In cases of phthisis and other lingering diseases, where opiates have for a long time been administered, but have ceased to produce sleep, Indian hemp may often be given with advantage; thus, in one case of advanced phthisis, doses of five or ten drops of the tincture were successful in procuring sleep when other means had failed.

Dr. Christison has administered hemp in many instances. He gives the following account of two of them:—

A gentleman had suffered from palpitation of the heart for twenty-one years, and at night the attacks were generally most severe. He had used one medicine after another with the hope of relief, but he did not derive any benefit. Dr. Christison advised him to try Indian hemp. The patient's wife states that he passed the the night on taking it without suffering from the palpitation, though still he was perfectly conscious of its presence; and that the attack left him entirely at 8 a.m., instead of continuing twenty-four hours, as it previously did.

In the other case, a gentleman was afflicted with a severe eczema over the whole body, with intense itching. A large dose of solution of the muriate of morphia caused extreme sleepiness, but so much increased the itching that he was kept awake by the necessity of scratching. Twenty-five drops of cannabis tincture gave him six hours' sleep, and he continued to enjoy sleep from four to six hours every night for six weeks without increasing the dose, until the eruption was nearly removed: during all this time the itchiness continued as before when he was awake.

Dr. Christison has observed that, in the generality of cases, hemp has had the effect of causing sleep without disturbing the function of the stomach or bowels. Given where morphia and hyoscyamus had failed, it has also repeatedly failed to cause sleep; but in one or two cases he has found it to succeed where morphia and opium disagreed.

An interesting series of cases by Mr. Donovan will be found in the 'Dublin Journal of Medical and Chemical Science' for 1845. This gentleman was convinced of the beneficial effects of hemp, particularly in cases of neuralgia. Mr. Donovan had himself suffered occasionally since early life from neuralgic

pain of different parts of the foot, lasting one or two days, or sometimes a week. Immersion in cold water gave entire relief, but no other treatment did so, till he took five drachms of weak tincture of hemp; in twenty minutes the pain was gone; at the same time, "he had hardly any consciousness of the motion of his limbs when walking—they appeared not to belong to him." On another occasion, he took six drachms without effect; but on the third night, after taking twelve grains of weak extract, he was free from pain, and slept four hours; and in several other attacks he derived similar benefit.

A gentleman was attacked on going to bed with excruciating pain in the left upper jaw, which kept him awake till morning; after a short sleep, he awoke in torture, and in the evening, upon taking his third dose of fifteen minims of weak tincture of hemp, he slept profoundly till eight next morning, when the pain was much abated. At night he repeated the remedy, with similar results, and next evening he took twenty minims, which deadened the pain; but it soon became as bad as ever. Embrocations of laudanum and camphor spirit were then tried, with another dose of twenty minims, and he immediately fell asleep; in the morning the pain was nearly gone, and it soon disappeared.

Another gentleman had excruciating sciatica for thirteen weeks; his sufferings caused groans, cries, and tears, and he passed sleepless nights. The only relief he obtained was from firm pressure on the hips, and, for a short time, from laudanum. Two doses of hemp, at short intervals, produced sound sleep for eight hours, and on awaking he was perfectly relieved. Five doses more so completely subdued the pain that it gave little farther trouble. He experienced a slight but transitory return on entering a cold room.

A number of other cases will be found in Mr. Donovan's paper, in which the hemp, if it did not effect a cure, yet was of great service in the treatment of the complaints to which he alludes; but in several cases no good followed, and, on the contrary, unpleasant effects were produced. Thus, a lady suffering from neuralgia of various parts of the body was ordered five drops of strong tincture at night; next morning she was giddy and weak, and, without authority, took five drops more. She became faint and universally cold, had some apprehensions of death, and remained disagreeably affected during the whole day; the pain was not relieved, and the effects of the hemp reappeared at intervals for two or three days.

Another patient, who was accustomed to take hemp, on one occasion had alarming depressing symptoms; he sat, greatly agitated, with his eyes open, and his head reclined on his chest. The respiration was tremulous, with interruptions of sobbing; his whole frame was in an indescribable shudder, and he seemed to shiver with cold. The pulse was good all the time, and in half an hour he recovered.

Indian hemp, in different forms, has been recommended, principally by the older writers, for several other purposes, as in the treatment of diarrhœa, gonorrhœa, and locally as an anodyne lotion, or in the form of poultice for hemorrhoids. For these purposes, I am not aware that it is now used; but there is one affection where it has late been applied with advantage—viz., uterine hemorrhage. Dr. Churchill says ("Diseases peculiar to women," Ed., 1849)—We possess two remedies for these excessive discharges, at the time of the menses going off, which were not known to Fothergill—ergot of rye, and tincture of Indian hemp. The former has been long known to possess the power of restraining uterine hemorrhage after delivery, &c., but the property of

hemp of restraining uterine hemorrhage has only been known to the profession a year or two. It was accidentally discovered by my friend, Dr. Maguire, of Castleknock, and since then it has been extensively tried by different medical men in Dublin, and by myself with considerable success. The tincture of the resin is the most efficacious preparation, and it may be given in doses of from five to fifteen or twenty drops three times a day, in water Its effects, in many cases, are very marked, often instantaneous, but generally complete after three or four doses. In some few cases of ulceration, in which I have tried it on account of the hemorrhage, it seemed to be equally beneficial.

These effects seem to me to be allied to the action of hemp on uterine contraction during labour, to the consideration of which subject I shall next proceed.

[The following interesting cases are given by Dr. Christison of the powers of Indian hemp on the contractions of the uterus.]

One woman, in her first confinement, had forty minims of the tincture of cannabis one hour before the birth of the child. The os uteri was then the size of a shilling, the parts very tender, with indurations around the os uteri. The pains quickly became very strong; so much so as to burst the membranes, and project the liquor amnii to some distance, and soon the head was born. The uterus subsequently contracted well.

Another, in her first confinement, had one drachm of the tincture, when the os uteri was rigid, and of the size of a half-crown; from this the labour became very rapid.

Another, in her first confinement, had also one drachm of the tincture, when the os uteri was of the size of a half-crown. Labour advanced very rapidly, and the child was born in an hour and a half. There were severe after pains.

A fourth had ℥ iij. of the tincture, in divided doses, which much accelerated and increased the pains. She had then chloroform for six hours. I have since been informed, that the severity of the pains was so great as to cause some alarm, and chloroform became necessary to produce insensibility.

Case 1.—Was a natural labour and eighth pregnancy. The first stage was not completed till twenty-four hours after the woman was seized. Hemp was given four hours before its completion. After the first dose of eight drops, little effect was observed; but after the second of twelve drops, the duration of the pains was increased, and the interval shortened; and it was very obvious that the intensity of the pains, counting from the second pain after the hemp was given, was increased; by the fourth or fifth pain the effect wore off, and hemp was not again given.

Case 2.—This was a second pregnancy. Seventeen drops of hemp were given in the second stage. The second pain, after the hemp was taken, was lengthened, and the interval shortened; this was not the case with the third pain; but the intensity of the pains was much increased and the woman was speedily delivered.

Case 3.—First pregnancy. Hemp was given in the second stage of labour and the chief fact observed was increased intensity of the pains; the duration of the pains was slightly increased, and the intervals decidedly shortened, after the second dose of hemp. Twenty drops were first given, and after twenty minutes thirty drops more. Twenty-four hours after, twelve drops were given, and after pains were induced, which the woman said were "quite as bad as when she took in labour first."

Case 4.—First pregnancy. Twenty-five drops of the tincture were given at the completion of the first stage; after this, both the pains and the intervals were shortened, and the intensity of the pains increased. After one or two pains the effect wore off, and thirty drops more were given at the end of half an hour. The third pain after this became very intense; and pain succeeded pain without intermission for several hours. As there was deformity of the pelvis, chloroform was administered, and delivery accomplished by the forceps.

Case 5.—First pregnancy. Hemp was given during the second stage, first thirty drops, and then thirty-five drops after half an hour, and the patient was delivered during its action. The effect of the first dose was chiefly shortening the interval at first, and prolongation of the pain; but the effect on the interval was more marked after the second dose. The pains were described by the patient as more intense, and by examination it was ascertained that the head of the fœtus was more forcibly propelled.

Case 6.—First pregnancy. Thirty drops of hemp were given in the second stage, and the effect was very decided. Previous to the administration of the hemp there had been no progress for an hour, the patient was nervous and excited, and though she complained much of the pains, the contractions of the uterus were felt to be feeble, and the child's head did not move; but on the second pain after the cannabis the contractions became very strong, forcing down the head, and the child was expelled ten minutes after the hemp was given. At the same time there was no decided effect on the duration of the pains and intervals.

Case 7.—Sixth pregnancy, with the first stage not completed. Thirty-two drops were given, and the action was well marked; the woman said, the second pain after it was the strongest she ever had. After an hour and a half, forty drops were given, but there was no action on the pains; they became irregular, and the intervals were very long; the case was then allowed to proceed naturally. There seemed to be a tolerance of the remedy; for though 120 drops had been taken, no physiological effect of any kind was induced.

In these cases, then, it does not appear that the duration of the pains or of the intervals was materially affected in all; but in cases 1, 2, 4, prolongation of the pain and shortening of the interval were most obvious; while in case 5, a shortening of the interval corresponding to each dose of hemp was observed. Shortening of the interval was in general a more conspicuous phenomenon than prolongation of the pain. Upon the whole, however, I am not inclined to lay much weight upon these results. But there can be no doubt that the *intensity* of the pains was greatly augmented by the hemp, except in the last case, where, after the effects of the first dose passed off, no action followed the repetition of it. This case was an exception to all the others.

It is worthy of remark, that in none of these cases were the ordinary physiological effects produced; there was no excitement or intoxicating action, and there did not seem to be the least tendency to sleep in any of them.

In conclusion I may state what appears to be the most obvious difference between the action of ergot of rye, and that of Indian hemp. First,—While the effect of ergot does not come on for some considerable time, that of hemp, if it is to appear, is observed within two or three minutes. Secondly,—The action of ergot is of a lasting character, that of hemp is confined to a few pains shortly after its administration. Thirdly,—The action of hemp is more energetic, and perhaps more certainly induced, than that of ergot.

There appears little doubt, then, that Indian hemp may often prove of essential service in promoting utering contraction in tedious labours.

More extended experience will show how far these effects may be depended on, and to what cases hemp is most applicable.

Mode of Administration.—Indian hemp may be administered in several ways. The extract, in the form of pill, produces the most gradual effect, and the disagreeable taste of the solution is avoided; but its action in this form is very uncertain. The following emulsion has been recommended:—A scruple of the extract rubbed in a warm mortar with a drachm of olive oil, to which are added half an ounce of mucilage, and seven ounces and a half of distilled water (Bromfield). But the simplest method is to use the tincture, which should be dropped into a little water, and immediately swallowed. The water may be sweetened with sugar; or an aromatic, as compound tincture of cardamom, may be added. The usual strength of the tincture is three grains of the extract to a drachm of rectified spirit.

The extract may be given in doses of one to six grains; the tincture in doses of ten to thirty drops, for ordinary purposes. Less than thirty drops is of little service in promoting uterine contractions; and greatly larger doses, as much as one or two drachms repeatedly, must be used in the treatment of tetanus, in which disease there is very great tolerance.—*Monthly Journal of Med. Science, July & Aug.* 1851.

TANNIN, EMPLOYMENT OF.

Dr. Cummings states, as the result of several years' experience, that he has found tannin the most valuable of *astringents.* Thus, whenever, in dysentery, medicines of this class are indicated, it acts admirably, either given alone or combined with opium. He says, he could refer to more than a thousand cases of dysentery, diarrhœa, cholera infantum, &c., in which he has employed it, never with regret, and almost always with advantage; while other practitioners, with whom he has communicated concerning it, express similar opinions. In the sweating, or last stage of phthisis, or low continued typhus, and even in the worst cases, this accompaniment of diseases of debility has been entirely or in part relieved. It is useful in almost all forms of hemorrhage, and most remarkably so in hœmoptysis; and when combined with opium and ipecacuan, it forms a medicament very preferable to acetate of lead and other similar substances. Among other forms of hemorrhage, over which it exerts great power, is that from the bowels resulting from dysentery, and that which occurs in threatened abortion. In hæmorrhoids, it is of great use as an outward wash. In epistaxis, it may be snuffed up or blown through a quill, and will almost always arrest the bleeding. No article in the whole class of astringents acts like it in severe salivation. In aphthæ and other diseases of the mouth, in which there are spongy or bleeding gums, it possesses no equal. Used as a gargle in relaxed uvula and tonsils, its efficacy is great. As an *antiseptic*, for cleaning old foul ulcers, the author has extensively used in the form of a powder, especially when there is disposition to hemorrhage. As an estringent collyrium, it is, in his opinion, preferable to all other substances in the purulent opthalmia of infants. He administers it internally in two-grain doses.—*Boston Med. Journal; and Brit. and For. Medico-Chirurgical Review, Oct.*, 1851.

SELECTED MATTER.

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PHYSIOLOGY.

ON THE PHYSIOLOGY AND PATHOLOGY OF THE PHOSPHATE AND OXALATE OF LIME, AND THEIR RELATION TO THE FORMATION OF CELLS.

By Dr. William Benees, Resident Physician in the German Hospital, Dalston.

[In a small work lately published in Germany, the author has established the following results :]

1st. Just as in plants and inferior animals, the phosphate of lime is indispensably necessary in man for the formation of cells; this formation does not only depend upon the presence of albumen and fat, but likewise upon the presence of phosphate of lime.

2ndly. The want of phosphate of lime, either in plants or animals or men, causes a deficient formation of cells; and a great many pathological states of the system really seem to depend upon a deficiency of phosphate of lime.

3rdly. In accordance with these general laws, we must suppose that we are enabled to cure, or at least to alleviate, by the internal administration of phosphate of lime, diseases marked by emaciation, formation of ulcers, in one word, by a deficient formation of cells.

4thly I have shown by my experiments, that such really is the effect of the administration of phosphate of lime;—that is to say, I have produced by the internal administration of phosphate of lime, an undoubted increase of the cell formation in diseases evidently showing a diminished formation of cells.

5thly. As diseases or affections of this kind, which have come under my observation, I have to mention:—

a. Ulcerations of any part of the system, which are based upon general dyscrasia, such as scrofula, and which are not merely local affections.

b. Infantile atrophy, especially the well known atrophic state of children suffering from rickets, and its accompanying symptoms, as diarrhœa, &c.

c. Tuberculous disease, more especially of the lungs, in its earliest stages.

6thly. There seems to be a remarkable connection between scrofula and deficiency of phosphate of lime. But as it generally ought to be mentioned, that we shall never be able to produce an increase of the formation of cells, unless we administer a wholesome, preferably nitrogenous diet; so it must be considered that the deficiency of phosphate of lime is only a constituent part of these diseases;

and by its mere use we are very well enabled to remove symptoms, which depend on its deficiency, but by no means shall we cure thereby the dyscrasia *in toto*.

The chief point of the facts on which these conclusions are founded is the great importance of inorganic substances in the formation of organic compounds. Liebig pointed out this fact with regard to the organic compounds of plants, these depending in some measure upon the presence of the inorganic constituents of the soil. And hence we must be induced to suppose that a similar relation exists in animals, and even in men. Dr. Bencke even supposes that in these organisations the inorganic are quite as essential as the organic compounds.

In Plants the azotic compounds are not produced without the co-operation of the phosphates. The produce of cells increases proportionately to the power and quantity of the manure afforded to the soil, and that this power particularly depends upon the presence of phosphates, the other salts really being of no great importance in this respect.

But there are different sorts of phosphates contained in the soil, such as phosphate of soda, phosphate of lime, phosphate of magnesia, and phosphate of iron. Is there any reason to believe one of these several substances to be more important for the production of nitrogenous substances and cells than the others? Facts are not wanting which afford an affirmative answer to this question. The single experience that we may considerably increase the produce of nitrogenous substances and cells by manuring the land with ashes of bones, is quite sufficient to prove that the phosphate of lime is of the greatest importance in this respect. A great many analyses of ashes of bones, communicated in the "Annalen für Pharmacie und Chemie von Liebig und Wohler," by Enderlin, Fresenius, and Will, and the analysis of bones by Berzelius, evidently show that the phosphate of lime is always present in ashes in a certain proportion, according to the nutritive power of the plants and the soil from which they are taken; and that, on the other hand, the proportion of phosphate of magnesia and soda in bones themselves is too small to partake of the influence exerted upon the soil by manuring it with bones. Berzelius found in 100 parts of dry bones, 53.04 phosphate of lime; 1.16 phosphate (carbonate ?) of magnesia, and 1.20 soda, with a small quantity of chloride of sodium.

On these facts, then, I have founded the conclusion, that the phosphate of lime is indispensably necessary for the formation of cells in plants.

With regard to the inferior animals, and the part which is performed in their economy by the phosphate of lime, I have only to mention an excellent paper by Dr. Carl Schmidt, "Zur vergleichenden Physologie der wirbellosen Thiere Braunchweig, 1845." In this paper Dr. Schmidt communicates most interesting experiments, from which it becomes evident, that the phosphate of lime has an intimate relation to the formation of cells. Dr. Schmidt ascertained beyond a doubt, that in the articulata the quantity of phosphate of lime increases or decreases proportionately to the quantity of chitin, a sort of colourless, transparent tissue, which is not soluble in water, alcohol, ether, and liquor potassæ, and forms the principal constituent part of the skeleton of all the invertebrata. Now, this tissue is the result of an active formation of cells during the period of changing the integuments in these animals, and so it results that the quantity of cells formed is proportionate to the quantity of phosphate of lime present.

Dr. Schmidt himself says:—These observations really are so striking as fully to

confirm the opinion before advanced; and he further adds as his firm belief, that a certain combination of albumen and phosphate of lime, or better, that a solution of albumen, which is saturated with the phosphate of lime, is particularly enabled to coagulate by the contact of heterogeneous substances, and to form membranes around them—that is to say, walls of primary cells.

Having become acquainted with these remarkable facts, I put forward the question, whether the phosphate of lime might not have the same relation to the formation of cells in the higher classes of animals, and even in men, as it has been shown to have in inferior animals and plants? I have been fortunate enough to obtain satisfactory and affirmative results.

The way in which I tried to solve the question was a double one—first, I had to prove that wherever we find a formation of cells, the phosphate of lime is present, and *vice versâ*, that the phosphate of lime is wanting where no formation of cells takes place; secondly, I had to show that the phosphate of lime is indispensably necessary for, and that it really influences the formation of cells.

First, I examined the serum, which was drawn by blisters. It is rather difficult to detect the phosphate in a single drop of the unaltered serum, the quantity of lime really being a very small one. However, in a single drop I detected crystals of the smallest size, by the addition of sulphuric acid, and by continued examinations I found that the crystals presented themselves the more quickly and well marked the sooner the formation of pus-globules took place in the serum, which was left beneath the skin. If I slightly evaporated the serum in a hot-water bath and now mixed a drop of the evaporated serum with sulphuric acid, a rapid formation of crystals generally took place, which undoubtedly showed the phosphate of lime to be present in large quantity. I then examined exudation-matter of wounds and ulcers, and these observations really afforded the greatest interest; they decidedly proved the relation of the phosphate of lime to the formation of cells. It will be well known to every accurate observer, that during the time of cicatrization of wounds and ulcers, two different sorts of exudation generally take place. First, an exudation appears, which I should like to call "spurious exudation," and which really exhibits nothing but a natural cover for the part affected or wounded, being far different from what we call "spurious granulations;" this exudation is subsequently thrown off; afterwards beneath this covering the real blastema is produced, affording the materials for the tissue which is to be formed, and undergoing the well-known changes to cells, tissue, &c. Well, then, if we examine microscopically what I have called the spurious exudation, we shall observe it to consist of amorphous, structureless masses; no cells are to be detected; it only seems to consist of molecules; no organization takes place. And even in these masses, by the addition of sulphuric acid, I have never observed the formation of sulphate-of-lime crystals, and consequently no phosphate of lime could be present. If, on the contrary, I examined in the same way the blastema produced beneath the spurious exudation, after the lapse of about twenty-four hours, I not only met with beautiful exudation-cells and pus-globules, but also, by adding a drop of sulphuric acid, could observe a rapid formation of crystals, so as to be led to the conclusion that the phosphate of lime is present in a large quantity, where cells are produced, and that it is wanting where we find nothing but amorphous masses. Lastly, with respect to this point, I have to draw the attention to the muscular tissue itself; and it will become evident, from my observations, how small

a quantity of phosphate of lime we are able to detect by the kind of examination alluded to. The muscular tissue is well known to contain a certain quantity of phosphate of lime; it was the result of Liebig's inquiries, that when the formation of muscular tissue from the constituents of the blood takes place, nearly the whole quantity of alkaline phosphates returns in the blood, and that at the same time a certain quantity of phosphate of lime becomes chemically fixed in the organs themselves. This quantity, then, however small it may be in a muscular fibre, which is so fine, as, by microscopical examination, to show the transverse stripes, I have detected, in the above described way, in a few muscular fibres which I had submitted to the action of sulphuric acid for about twenty-four hours. I observed, after this time, by the microscope, crystals of sulphate of lime—of course only in very small quantity, but beautifully formed. Especially in this kind of preparation the different stages of crystal-formation are to be well observed.

After these experiences, the other question remained—whether the phosphate of lime really influences and increases the produce of cells? I have tried to solve this question in a double way, first by experiments, and on the other hand, by practical inquiry.

With respect to the experiments, it is my firm belief that I succeeded in artificially producing cells, which did not show any distinction from pus-globules and what we call exudation-cells.

First, I tried the phosphate of lime in patients who suffered from chronic ulcers, resulting from the scrofulous diathesis, and exhibiting a want of formation of cells in the highest degree. These patients had been for a long time under medical treatment, inclusive of my own, but all remedies had been fruitless, such as cod-liver oil, ointments of lead and zinc, lotions of nitrate of silver, &c. Being myself quite sure, that no influence of the former kind of treatment could be still remaining in operation, and even after having left off all treatment for a long time, I then ordered the phosphate of lime to about four or eight, to twenty grains per diem, and after a few days the ulcers evidently showed themselves in another state. The suppuration improved; instead of an ichorous secretion, a pus bonum et laudabile was produced, and after a few days longer the cicatrization began. In children, in particular, I obtained very striking results, and there was not the least doubt, that the effect must be ascribed to the phosphate of lime.

In syphilis I had also tried the phosphate of lime, and even in persons who had for a long time already suffered from secondary ulcers, ulcers of the bones, &c., and became emaciated and extremely weak during that time. These cases likewise showed a most beneficial effect of the phosphate on the formation of cells. It ought to be mentioned, that besides the phosphates, the iodide of mercury was administered, but I have never met with such a rapid cicatrization of syphilitic ulcers, as was the case in these persons, and I could not help thinking that the cure was promoted in a remarkable degree by the internal exhibition of the phosphate.

As to other affections, in which I have tried the phosphate of lime, I have to mention rickets, caries, inflammations, and consecutive abundant suppurations of the cellular tissue, and also fractures of the bones. In all these cases, the administration proved most beneficial, and I would strongly advocate its further experimental use. With respect to fractures of the bones, I have to state in particular, that the consolidation of the callus took place in a much shorter period than is

generally the case ; however, too large doses of phosphate of lime must be shunned in those cases, as I have observed an abundant callus, causing a deformity of the bones, produced by the daily administration of twenty-four grains of the phosphate for a fortnight.

Having stated the principal facts, affording a proof of the relation of the phosphate of lime to the formation of cells, Dr. Beneke proceeds to add, that as the formation of cells is increased by the administration of the phosphate of lime, we may ask—in the diseases shown to be most beneficially influenced by the administration of phosphate of lime—is the phosphate present in the system in a smaller quantity than it ought to be in the normal state ? But it is impossible to determine directly the phosphate of lime which is present in the body. One might suppose the question might be solved by making accurate analysis of the blood, but he says :—We cannot avail ourselves of the analysis of the blood in order to solve the above question ; but there is another way which will enable us to do so, and this way is afforded by continued analysis of the urine and the feces. Thereby we shall know what quantity of phosphates is thrown out of the system, and by comparing this quantity with the average quantity of phosphates taken with the food, we shall be very well enabled to judge of a general increase or decrease of the earthy phosphates in the system. However, as it is very likely that the greatest part of the phosphates contained in the excretions of the bowels originate directly from ingesta, and as it is certain, on the contrary, that the greatest part of the earthy phosphates contained in the urine originate from the wear and tear of the tissues and bones of the system, I have in the first place only directed my attention to the latter, and I have found the analysis of the urine to give a satisfactory answer to the question proposed.

The following now are the results of the analyses which I have performed, and which we may depend upon the more as most of them have been twice repeated. First, the urine which I marked by 0, always contains a very small quantity of phosphates; this quantity, however, cannot be detected in the above-described way, but it never seems to exceed the quantity of 0.2000 grain in one ounce of urine. There are many steps between no phosphates at all and of 0.2000 grain; they require a more accurate study in order to show the preternatural decrease of the quantity of earthy phosphates in the urine. But as we shall consider here only the hypernormal increase in the quantity of phosphates which are excreted in the urine in diseases, I am compelled to waive this discussion at present. I have only to state that in every case we may consider a urine marked by 0, as containing 0.2000 grain of earthy phosphates, or less; never more than this in one ounce. With respect to the other descriptions of urine, I should really far exceed the limits of this paper by adducing the results of the single analysis; they can be seen in a pamphlet which I have lately published in Germany, entitled, "Zur Physiologie und Pathologie des Phosphorsauren und Oxalsauren Kalkes," Goettingen, 1850. Here I only beg to state the general results as follows :—

A urine marked by $\frac{1}{2}$, contained nearly 0.250—0.300 grain of earthy phosphates in one ounce; a urine marked by 1,0.400—0.450 grain; a urine marked by $1\frac{1}{2}$, 0.550—0,600 grains; a urine marked by 2, 0.700—0.750 grain; a urine marked by 3, 1.000—1.050 grains; and lastly, a urine marked by 3—4, 1.000—1.300 and more grains of earthy phosphates. By referring to these numbers we may easily

approximately calculate the quantity of earthy phosphates voided in twenty-four hours, and I am sure we shall never be far from the truth.

After these explanations I have to speak of the different quantities of phosphates which I met with in the urine in different diseases. My observations of course do not extend to all diseases; it would scarcely be possible to give such accounts in a large number of years, notwithstanding I observed a sufficient number of cases with respect to this point for the deduction of some general results.

First, I have to remark generally, that scarcely any disease occurs, in the course of which we should not sometimes find an increased quantity of phosphates; that at any rate there exists no disease which does not admit of some hypernormal excretion of phosphates at some one of its periods; on the other hand, we meet at different periods of disease with quite different quantities of phosphates, as, for instance, it often happens that in the first stages of disease we do not find an increased quantity of phosphates at all, and that at a later period a large quantity is excreted. With respect to this point, and in order to obtain results which can be depended upon, it is therefore indispensably necessary to examine the amount of phosphates almost every day; we shall never arrive at correct views if we do not attend to this rule.

Secondly, it must be stated as a general result, that the quantity of phosphates excreted does not depend as well upon the nature of the disease itself, as upon the individual afflicted; and if in one case of rheumatism we find, for instance, a large amount of phosphates in the urine, we do not detect any increase at all, perhaps, in another case. This point really is a very important one; it affords the best proof of the general fact, that we are always wrong in speaking of certain diseases as of individuals, or as of well-defined and marked never-varying alterations of the physiological state of the body, and that we shall never succeed in obtaining positive results, if we do not direct the most accurate attention to the previous history and the former state of the individual who has become afflicted with any disease—in other words, if we do not individualize disease.

It may be concluded from these short remarks, that it is very difficult to give an account of the excretion of phosphates, generally applicable and absolutely right in almost every case. However, we meet with some pathological states which, generally speaking, very rarely show an increase of the excretion of phosphates; with other affections which always show an increased quantity of phosphates in the urine and even a most anomalous quantity; and with others which are generally distinguished by a slighter, but continued loss of phosphates. To these states and affections I shall now draw attention, and I scarcely know how to give a better explanation of them, than by referring to the numbers above alluded to.

I have met then with urine containing only such a quantity of phosphates as I have marked by 0 or $\frac{1}{2}$—

1st. In persons who, always showing a good state of health, a normal complexion and colour, and a strong constitution, have accidentally become afflicted with disease or injury, as, for instance, with syphilis, wounds, contusions, &c. As I really considered these persons as nearly healthy, at least for so long a time as the affection remained a local one, I am inclined to view the quantity of 0.1000—0.2000 grains of earthy phosphates in one ounce of urine as nearly the normal quantity. It must, however, be remembered, that it is always extremely difficult to speak of

normal states of health, a precise distinction between health and disease being incompatible with our present amount of knowledge, if conceivable at all.

2nd. In the first stages of acute diseases, as, for instance, of acute rheumatism, pneumonia, pleuritis, peritonitis, &c. When these diseases happened in persons who did not exhibit any other signs of diseased constitution, and had never been ill before, I did not, either during the whole course of the disease or upon recovery, find an increased quantity of phosphates in the urine. But it was seldom I met with such persons. When, on the other hand, persons were afflicted with acute diseases who never enjoyed good health before, or suffered from dyscrasia of the blood, I almost always met with an abnormal quantity of phosphates in the urine after the acute stage having ceased; there appeared now all the symptoms of the original dyscrasia, and either the reconvalescence was a very slow one, or emaciation, general weakness, &c., was still increasing; instead of acute tuberculosis, the symptoms of chronic affection of the lungs appeared; instead of acute rheumatism, chronic rheumatism remained. From these very remarkable differences I concluded with certainty, that it was not the disease itself which caused a decrease of the excretion of phosphates, but that this decrease was exclusively dependent upon the acuteness of symptoms, that is to say, the feverish action—a circumstance which I shall refer to in the following parts of this paper. It must also be mentioned, that in some cases of acute disease I met with a quantity of phosphates as marked by $\frac{3}{4}$ or 1, even during the first periods; in these cases, however, the quantity of phosphates was very considerable after the acute symptoms having ceased.

3rd. In the first stage of typhus fever. Here I always met with a decrease of the quantity of earthy phosphates in the urine, a result which, after a great many analyses, seems to be a characteristic one. With respect to the later periods of typhus, however, the same refers to them as I have stated on acute diseases generally.

4th. In some cases of Bright's disease, as well as in some persons who suffered from stenosis of the orifices of the heart, or from insufficiency of their valvules. But some cases also occurred where an absolute increase of phosphates was met with; however, in these cases complications or affections of other organs could be observed; and I am inclined to suppose that the dyscrasia of the blood, which leads to the well-known degeneration of the kidneys, as well as the dyscrasia which results from the above-mentioned diseases of the heart, does not cause by itself any increase of the earthy phosphates in the urine.

5th. In the first stages of carcinoma (but only in these); an observation the more interesting, that Rokitanski alludes to the preternatural development of the bones, or the proportion of phosphate of lime in persons afflicted with cancer. I myself, found in a post-mortem examination of an individual who died from carcinoma of the lungs, all the cartilages of the ribs ossified; which was never the case in persons who during life passed for a long time increased quantities of phosphates, and had been afflicted, for instance, with tuberculosis.

I have to add, generally, that in all cases where I observed no increase of the phosphates in the urine during the *whole course* of a disease, I likewise never observed emaciation; that is, want of formation of cells; these persons altogether were of strong constitution, and showed a remarkable development of the muscles

In these persons blistered surfaces healed far more speedily than was the case in persons who passed a hypernormal quantity of phosphates, the average time being three or four days. Of course, in acute diseases emaciation was observed, though no abnormal loss of phosphates could be detected; the same was the case with Bright's disease. In these affections, however, many circumstances concur, which sufficiently account for the waste of tissues—circumstances which do not require any further explanation.

To sum up now the results of the first and second part of these communications, I have shown in the first that, supposing a sufficient quantity of albumen and fat to be present, the produce of cells evidently increases by the administration of phosphate of lime; that, on the other hand, by this administration we may promote the cure of diseases which show a deficiency of the formation of cells; and that especially in scrofulous affections the administration of phosphate of lime has often proved most beneficial. On the other hand, in the second part, I have established the fact, that in nearly all chronic diseases, where we observe a loss of flesh, emaciation, and general weakness, a hypernormal quantity of phosphates is always excreted from the economy by the urine, and more especially in those cases where the administration of phosphate of lime proved most beneficial. Perhaps it might be supposed that these quantities had been increased by the phosphate of lime taken as a remedy; but this is by no means the case; on the contrary, my observations prove, that even during the administration of phosphate of lime, the quantity of earthy phosphates in the urine often decreases, supposing a proper treatment in other respects to be employed. Well, then, the harmony of the results of the above two parts is so striking, that we can scarcely admit of any doubt in their truth, and the physiological as well as pathological importance of the points alluded to is so apparent, that it does not require any further explanation. We know that the phosphate of lime is indispensably necessary for the production of cells; we know that in a great many diseases the phosphates are excreted from the economy in very abundant quantities by the urine; and we know even that in these diseases the formation of cells is deficient. Shall we have any doubt that by substituting the quantity of phosphates excreted by the urine, or by removing the cause of their excretion itself, we must afford a great benefit to persons who are afflicted with the diseases alluded to.

There remains one difficult point, which I have to refer to. In the way which I relied upon in determining the quantities of phosphates in the urine, I precipitated the phosphate of magnesia, as well as the phosphate of lime. This having been shown microscopically and chemically, the question arose, whether the results which I spoke of with respect to the phosphate of lime would not require an amendment? It is true the proportions between the phosphate of lime and phosphate of magnesia are very different in different urines; however, I rarely observed the quantity of magnesia to exceed the quantity of lime; on the contrary, it was oftener found less. I therefore concede, without any hesitation, that the exact quantity of phosphate of lime could not be ascertained in the manner which I have adopted; but generally we shall not be far from the truth in supposing half the quantity of earthy phosphates present to consist of phosphate of lime, this being generally below and very rarely beyond the real quantity. I must repeat with respect to this point the above given remark, that I have only looked for approximate results, and that I believe them sufficient for the conclusions which I have drawn. All these

relations demand a very accurate revision; nor can I refrain observing that the proportions between phosphate of lime and phosphate of magnesia in different diseased persons are highly interesting, so much so as to recommend their most accurate study and exact analysis. In expressing this opinion, I refer, for instance, to a communication in the "Annales de Cihmie et de Physique, Juin, 1849, tom xxvi., 3e série," entitled "Recherches sur les Causes du Goitre et du Cretinisme, par M. T. Grange," but I am compelled to waive the discussion for the present, it being rich enough in itself to form the subject of a special treatise.—*Lancet, April* 19, *June* 21, *and June* 28, 1851, *pp.* 434, 668, 699.

MEDICINE.

EPILEPSY AND INSANITY IN A LITTLE BOY; RECOVERY.

(Under the care of Dr. BABINGTON.)

It is very natural that in our visits to the hospitals of this metropolis, our attention should be especially directed to the diseases whose nature is but imperfectly known, the cure of which is uncertain, and where the pathological manifestations are of a very changeable character Epilepsy is of this number; and we have more than once had an opportunity of recording cases of this disorder which strongly illustrated some interesting point respecting its nature or treatment, or which tended to arrest attention upon phenomena not frequently observed. We alluded some time ago to the maniacal violence which sometimes accompanies the fits, it is now our intention to dwell for a few moments on a case wherein insanity was manifested, after epileptic seizures, in a very young subject.

It may safely be asserted, that with children epilepsy is more centric than eccentric, or, in other words, that it depends more on irritation conveyed to the brain from some portion of the body, as the liver or intestine, than from any derangement of the encephalon itself. It is not difficult to agree in this respect with the authors who have written on the disease; but when insanity occurs in a very young epileptic patient, we find existing theories insufficient; for it is generally held that the mental derangement is the consequence of repeated attacks through a series of years, that permanent congestion of the brain, and that fatuity, followed by complete aberration of mind, gradually sets in.

These gradual changes are thus described by Dr. Watson:—

"Doubtless, a single paroxysm does often leave the patient in a worse condition than that in which it found him; but this does not become perceptible to ordinary observer until after the alteration has been rendered apparent by repeated fits and repeated small additions to the permanent injury. The friends of the patient remark that his memory is enfeebled in proportion to the number of the attacks; that his mental power and intelligence

decline. His features even assume, by degrees, a peculiar character, and too often he sinks into hopeless fatuity, or confirmed imbecility or insanity. It is this tendency which render epilepsy so sad and fearful a disease."

Being acquainted with these views, we were not a little astonished in seeing in Dr. Babington's wards, a little boy giving evident marks of insanity, and whose derangement was ascribed to epilepsy. It is well known that the latter complaint may attack patients of any age, but among the most common periods, Dr. Bright mentions seven or eight years, being the time of the second dentition; and this is just about the age of our little patient. But besides the irritation of the teeth, other sources of disturbance may also exist with young patients, as stones in the bladder, worms, &c., so that very careful inquiries respecting these circumstances should always be instituted when we are desired to prescribe for children suffering from epilepsy. The case under consideration runs as follows:—

Thomas C——, aged eight years, a remarkably handsome, stout, healthy-looking, and apparently intelligent child, was admitted into Job ward in March, 1851, under the care of Dr. Babington. He is one of four children, who are all in good health; the patient himself has hardly ever been ill, but his father states that about four months before admission the child suffered from a very severe attack of whooping-cough, and swelling of the cervical glands. He recovered, however, in a short time, and during his convalescence he had an epileptic fit while in bed. This fit commenced with violent screaming; he then jerked his legs up and down, lost his consciousness, &c. At first, he had but one or two attacks per night, and one in the day; but the frequency of these had gone on increasing, and just before his admission he had twenty or thirty fits in one night, but not more than four or five in the day. These numerous fits probably belonged to that class which has by the French been called "petit mal;" they had latterly, however, increased in severity, and were accompanied by twitching of the right side of the face.

When the patient was first taken by these fits, he was quite sensible after each attack; but as they increased in number and strength, he lost all power of reasoning, and his intellectual faculties gradually went astray. He occasionally complained of pain in his stomach, but this usually disappeared in a very short time. As the boy continued to get worse, he was brought to town, and during a space of six days, which he spent in London before entering the hospital, he had but very few fits, which circumstance would tend to imply that the change of scene and the journey had acted favorably on the nervous system.

On admission he was found a well-formed boy, with large development of head, especially on the posterior part; dark hair and eyes, and of a very handsome and interesting countenance. He is constantly jumping out of bed, eluding the nurse's watching, and runs up and down the ward. He is always in some mischief, ever talking concerning subjects unfit for his age, and using shocking language, which forms an unpleasant contrast with the regularity of

his features and the beaming intelligence of his looks. He is very quick at answering, and puts to the physician and those who accompany him the strangest questions, which he repeats with great rapidity, until attention is paid to them. All the functions are normally performed, and there is no evidence of the existence of calculus in the bladder or intestinal worms.

The boy was at first well purged with jalap and rhubarb, and Dr. Babington then commenced a tonic treatment by the agency of sulphate of zinc, the dose for the beginning being two grains three times a day in mint water. On the next day the nurse reported that he had four fits in the night, which she thus describes: He first sits up in bed, then gives utterance to a kind of bark, the legs and arms become rigid, and subsequently convulsed, and after about ten minutes' struggle he falls into a profound sleep.

The doses of sulpate of zinc were gradually increased up to six grains three times a day; the fits did, however, not vary much in number or intensity for several weeks, but after that period, the health being still very good, they gradually diminished, the propensity for talking and mischief became less, the intelligence cleared up, and about three months after admission there were no more fits, and the little patient left the hospital well in mind and body.

The subject of the preceding case did certainly not look scrofulous, (epilepsy has by some authors been coupled with the scrofulous diathesis,) nor could the convulsive affection be traced to heredity; we may, therefore, suppose that the patient's naturally very excitable nervous system had been somewhat affected by the severe attack of whooping-cough, soon after the disappearance of which the epileptic seizures had begun. The fact of alienation of mind having subsequently taken place in so young a patient is, however, difficult of explanation. The sulphate of zinc can of course not be regarded as a specific, but the success which accompanied its use in this case will serve as an additional proof that the mineral tonics act very favourably where any portion of the cerebro-spinal axis is suffering from debility or derangement.

SURGERY.

ULCERATED CANCER OF THE BREAST; TREATED BY DR. JAMES ARNOTT'S FREEZING PROCESS.

(Under the charge of Mr. SHAW.)

Our attention was attracted, a short time age, towards a patient in the cancer ward, who suffers from an ulcerated carcinoma of the breast. This poor woman is far advanced in age, but the breast was attacked only one year ago. The disease has, however, made such rapid progress, that the scirrhous tumour is now deeply ulcerated, and at times extremely painful. It appears that Dr. James Arnott's plan of producing insensibility, by freezing the part

with a mixture of ice and common salt, was tried in this case, in order to allay the agonizing pain which the poor woman was suffering. This trial was attended with very satisfactory results, and the patient was so much relieved by the proceeding, that she soon afterwards requested to have the ice applied again, and she expresses herself as very grateful for the temporary removal of the severe pain she experienced.

Dr. Tyler Smith likewise succeeded, some time ago, at St. Mary's Hospital, in relieving excruciating pain by the freezing mixture, in case of cancer of the uterus. It is plain, however, that the apparatus must be somewhat more complicated when the disease has attacked the organ. The fact that the pain accompanying the carcinoma of the womb was thus allayed, should certainly not be lost sight of.

Most of our readers probably know, that Dr. James Arnott advises equal quantities of ice and common salt to be mixed together, (the former being well pounded,) and then placed into a gauze bag, the margins of which are attached to a gutta-percha ring. By gently touching the part to be rendered insensible, with the bottom of the bag, for a minute or two, the surface becomes suddenly frozen, insensibility follows, and the pain of course disappears. To obviate the tingling sensation which is apt to ensue upon the return of sensibility, ice without salt is to be used, and thus no uneasiness whatever is experienced. We have seen portions of the human frame thus frozen, and always noticed that the insensibility became very great.

Dr. Arnott has proposed that his process should, in certain operations, take the place of chloroform; but it is plain that the insensibility can hardly be carried deep enough for the generality of operative purposes. Where, however, a thin stratum only is to be implicated, it might certainly be used with advantage. To relieve the pain of cancer, it seems, from the preceding case, and others which have been recorded, to deserve attention at the hands of those who have to prescribe the palliatives which are so indispensable in that melancholy affection.

MATERIA MEDICA.

APERIENT, *Mild.*—The employment of the cortex of the rhamnus frangula, or alder buckthorn, has been recommended by Dr. Gumprecht, of Hamburgh, as a valuable and cheap substitute for some of the aperients now in use. "The fresh cortex is not to be used, as being uncertain or violent in its effects, that which has been kept, at least a year being preferable. If given in infusion it sometimes causes vomiting; and the decoction has been found the best and simplest way of preparing it, although the tincture and watery extract are very efficient preparations. The decoction may be prepared with ℥ss to ℥i. of the cortex to ℥xii. of water, boiled down to ℥vj., the strength depending upon the condition of the patient, duration of the disease, &c. So, too, the dose of a tablespoonful may be given every two or three hours, or two or three times a day, according to the state of the bowels and system in general.—*Med. Times, Nov.* 1, 1851, *p.* 469.

FORENSIC MEDICINE.

HORSES' AND HUMAN BLOOD.

At the inquest held upon Emma Styles, who was brutally and mysteriously murdered at St. John's Wood, Dr. Arthur Hill Hassall, who was appointed by the coroner to ascertain whether the stains upon the husband's clothes were those of human blood, stated that he closely examined with the microscope two of the stains upon the husband's shirt and trowers; and in order to arrive at an unerring and satisfactory conclusion, had scraped from a wall some horses' blood, and compared it with the blood-stains. Having detected a distinct and positive difference between the stains, he then examined the two stains upon the husband's clothes, and found that they were those of horses, and not of human blood. In answer to the coroner, he said he felt no hesitation in arriving at that conclusion. A verdict of "wilful murder against some person or persons unknown" was ultimately returned. The jury, in a written addendum to their verdict, declared the identity of the prisoner could not be proved, and that the ends of justice were defeated by the prisoner not having been brought before the coroner.

CHEMISTRY.

ON A NEW METHOD OF OBTAINING HIPPURIC ACID IN CONSIDERABLE QUANTITY WITHOUT EVAPORATION OF THE URINE.

The method described for obtaining hippuric acid, consists in adding to fresh cow's urine some hydrochloric acid, in the proportion of half a fluid ounce to a pint of urine. The hippuric acid being very slightly soluble in this acid liquor, is deposited together with some colouring matter, from which it is subsequently purified by dissolving it in water with excess of lime, adding animal charcoal, and heating the mixture for half an hour or more. On adding hydrochloric acid to the filtered liquor, and allowing the mixture to cool slowly, the hippuric acid is deposited in large prismatic crystals. It sometimes requires to be further purified by repeating this process. The acid on being thus prepared, on being analyzed, gave results agreeing with the established formula for hippuric acid, namely, $C_{18}\,H^8\,N\,O_5 + H\,O$.

ON THE COMPOUNDS OF COTTON WITH THE ALKALIES.

The author first described the process of Mr. Mercer, by which the beautiful fabrics made known to the public through the Great Exhibition, are produced. When cotton, or an article made of that material, is immersed in strong caustic soda in the cold, a certain combination is effected—which is again destroyed by pure water; but the "Mercerized" cotton thus produced is permanently contracted, and rendered more susceptible of dyes. This was illustrated by a number of specimens, much shrunk, so that they assumed an appearance of extraordinary fineness, others

puckered in patterns by partial "Mercerization," and others again printed with colours which surpassed in depth and brilliancy those produced by the same means on the calico in its original state. Dr. Gladstone proceeded to detail experiments by which he had succeeded in obtaining the compound of cotton and soda free from adhering alkali, through the agency of strong, sometimes absolute alcohol. He found that the proportion of soda which combined with the lignine varied with the strength of the solution employed, but under no circumstances exceeded one at m, the formula of the "sodaed" cotton, being C_{24} H_{20} O_{20}, Na O. There was a varying amount of combined water. Some properties of this compound were discussed, and the author then proceeded to state his conviction that there was no sufficient ground for viewing the "Mercerized" cotton as chemically different from the original lignine It is identical in composition, and the change of properties may be accounted for by the change in its physical condition. When viewed under the microscope, the fibres in their ordinary condition appear as flattened twisted ribands; but the moment they are touched by the alkaline ley they untwist themselves, contract in length, and swell out, assuming a rounded solid form; and this circular appearance they retain after the soda is removed by water. This not only explains the shrinking, but the cause of a larger quantity of dye being absorbed, as the substance of the fibre itself is porous. Potash has a similar action to that of soda, giving rise to a compound, the formula for which is C_{24} H_{20} O_{20}, K O.

SURGERY.

INJURY TO THE PELVIS, BLADDER, AND URETHRA; DEATH; AUTOPSY.

(Under the charge of Mr. Fergusson.)

Among the numerous kinds of injury which come before the hospital surgeon, there are few which present more danger than lesions about the abdomen or pelvis. Such cases, when the accident is severe, almost always terminate fatally; and the line of treatment must necessarily be surrounded with some uncertainty, as it is often extremely difficult to distinguish the actual nature of the lesion which the patient has suffered. Injuries of the urethra are generally followed by obstinate stricture; of this we have seen several examples, the latest being a boy, under the care of Mr. Coulson, at St. Mary's Hospital, who was for several years the subject of stricture and occasional retention, this state of things being the result of a laceration of the uretha. The boy has finally died of chorea, and we shall probably at no distant period offer a detailed report of the case.

But when the bladder is either contused or ruptured, matters proceed at a more rapid rate, and death soon ensues; but there is a great difference in the number of days during which patients live, after the occurrence of a rent in the bladder and extravasation of urine in the abdomen. Some die in a few hours, and others have been known to survive ten days, a fortnight or more. We shall just adduce some of the cases which have of late come to our cognizance. In the first, the injury was of a very serious character. The facts are these :—

William P——, a carman, about thirty years of age, was brought into the hospital, Dec. 24, 1851, having been run over two hours previously. It appears that while driving his cart through a narrow road, the patient found himself wedged between the wall and the vehicle, the horses having taken fright. The poor man had not time to rescue himself, but was knocked down, the wheel (the tire of which was six inches wide) passing between his legs, along the perinæum, to the upper part of the right thigh, and by a sudden turn crossing over the abdomen. At the time of the accident, the bladder seems to have been nearly empty, as the patient had passed urine an hour before.

On his admission, he was very low, and complained of great pain over the region of the bladder, and in the loins, especially on the left side, the course of the wheel being distinctly marked by strong ecchymosis. The patient was immediately placed into a warm bath; and as he had not passed any urine since the accident, an attempt was made to introduce the catheter. The uretha was found much lacerated, and the catheter seemed to deviate from the straight course just before it arrived at the triangular ligament. With the finger passed into the rectum, the point of the instrument could be felt gliding between the gut and the bladder. Mr. Davis, the house-surgeon, finding his efforts fruitless, sent for Mr. Fergusson, at 11 o'clock, P M.

After having carefully examined the patient, Mr. Fergusson endeavoured for a short time to introduce the instrument, but without success. He gave it at the same time as his opinion, that there was but little, if any, urine in the bladder, and that the right ramus of the ischium was broken at its junction with the pubis. Ordered fomentations.

On the next day, there was not much change, and the patient did not seem to suffer much pain, except over the immediate vicinity of the bladder, which viscus did not appear to be distended; the tongue was moist, the pulse quick and feeble. Calomel, eight grains; opium, one grain—to be taken at once. Another attempt was made to pass the instrument, but without success.

On the third day, the patient was found to have had a very bad night; he complained of severe pain over the bladder, and felt a great inclination to pass urine. It was thought that he might perhaps succeed in the warm bath, but he obtained no relief from it; nor were further attempts with the instrument successful. The abdomen now became tympanitic, but the pain remained located in the region of the bladder.

In the afternoon, as the patient was in much pain, Mr. Fergusson was again sent for, and the catheter tried, but without success; and Mr. Fergusson stated that the bladder was not distended, for he had found that on passing his finger into the rectum, he could feel no fulness about the bladder, the latter seeming free and capacious. Extravasation of urine was now suspected. Two incisions were made on the obdomen, and another in the left groin; but no urinous fluid escaped. The man continued in this state until about half-past two, A. M., on the fifth day, when the pain became excessive, and a sudden change took place. The nurse called up the house-surgeon; but before the latter reached the ward, the patient was dead.

The post mortem examination revealed the following lesions:—The intestines were slightly, if at all, inflamed, but the true pelvis was filled with urine, so that when the viscera were pressed downwards, the fluid welled up considerably. When

the opaque straw-coloured liquid had been taken out, no marks of peritonitis came into view. The bladder was now carefully examined. It was quite empty and collapsed, and on its posterior portion a ragged opening was observed, which might have admitted the index finger. The symphysis pubis was separated for the space of about one inch, and the ramus of the pubis fractured at its junction with that of the ischium. There was likewise a fracture of the ilium running into the acetabulum, and the sæcum was broken and separated from the bone. The urethra had suffered an extensive laceration in front of the membranous portion; from this latter fact it was now clear that the instrument, during the attempts at catheterism, had passed into the perinæum.

ON THE IODIDE OF POTASSIUM IN SYPHILIS.

[The following remarks upon this subject are made by the Reviewer in the "British and Foreign Medico-Chirurgical Review."]

Dr. Williams was the real discoverer of this influence, perhaps the greatest therapeutical discovery of the age, after that of the anæsthetic effects of ether and chloroform. His paper was read at the College of Physicians in 1834, five years before Ricord began his experiments; and so far from giving it indiscriminately in all cases, he took the greater pains to investigate its real powers, and pointed out where it was efficacious and where useless; not with hesitation, but with all the open candour of his nature. In his "Elements of Medicine," while showing the marvellous certainty of its action in rupia and the hard periosteal node, he showed that its power was much less in roseola, purpura, and ecthyma, but still it was better than mercury; while in lichen, lepra, psoriasis, and iritis, he proved with equal clearness, that mercury, either locally or generally, had more beneficial influence than the iodide. He pointed out the curious fact, that while the action of the iodide on *hard* periosteal node was as certain and evident as that of quinine in ague, when once suppuration had commenced, sarsaparilla was the remedy, the iodide being useless. In soft node and prurigo, he showed the true power of sarsaparilla; and in syphilitic angina and rupia, the invariably good effects of combining local mercurial applications with the internal administration of the iodide.

We witnessed many of his experiments, and for the last twelve years have been guided by his results, without ever having had cause to regret it; and after tolerably extensive opportunities of treating secondary symptoms, the only modification we have learnt to make in his practice, is the occasional use of the protoiodide of mercury in lichen and in some of the affections of ligaments and synovial membranes. We almost always give the dose recommended by Dr. Williams, eight grains three times a day in water or camphor mixture; and when using the protoiodide of mercury, begin with one grain daily in divided doses, increasing gradually to three or four grains in the day, made into pills with liquorice, or with catechu, if it acts on the bowels. Opium appears to destroy its power altogether. We never saw any good done by giving a mercurial course before the iodide, as many recommend, but often much harm. On this point and on the relative powers of iodide of potassium and mercury in syphilis, we would refer to a work in which the investigation has been made in the true spirit of science by Dr. Hassing, of Copenhagen. A notice of this book, and an abstract of some of the important results, will be found in Vol. XX, 1845, pp. 482–6.—*Brit. and For. Medico-Chirurg. Review, July*, 1851, *p.* 201.

SELECTED MATTER.

ANATOMY AND PHYSIOLOGY.

ON THE IMPREGNATION OF THE OVUM IN THE AMPHIBIA (SECOND SERIES), AND ON THE NATURE OF THE IMPREGNATING INFLUENECE.

By George Newport, F.R.S., F.L.S.

The author commences his paper by stating, that having given direct proof, in his former paper (see British and Foreign Medico-Chirurgical Review, vol. viii. p. 253,) that the spermatozoon is the impregnating agent, and also that the *liquor seminis* does not effect impregnation, he now proposes to detail some new experiments which bear on the views he then advanced; and especially with respect to the nature of the impregnating influence.

He first details some additional experiments with *solution of carmine*, with the object to show, that the result of one experiment mentioned in his former paper, in which he detected a small granule of carmine within the vitellary membrane, was attributable to the cause he then assigned—accidental injury to the egg; and he states that the results of his present investigations confirm him in the view then held—that no natural perforation or fissure exists in the envelopes of the egg, either of the Frog or of the Newt, before or at the time of impregnation; and that the spermatozoon does not penetrate into, but only lies in contact with, the envelopes.

He next gives the results of some experiments with solution of potass, in confirmation of his former observations; and further shows the effect produced on the egg by immersion in solutions of potass and soda, with different proportions of the salts; and afterwards details the results of other experiments made to test some of the more remarkable ones by Spallanzani with regard to the effect of very minute quantities of the impregnating fluid. In these trials the author has proceeded by the mode of direct application of the fluid, and not by immersion of the eggs in large quantities of water with small proportions of seminal fluid, the mode followed by Spalianzani. The result of the direct application through contact, *once only* with each egg, with the point of a pin wetted with the fluid, was, that this was sometimes sufficient to effect the commencement of segmentation, and consequent *partial impregnation*; while, if the fluid was allowed to drain off the pin, by continuing the contact for a few seconds, then complete segmentation and full impregnation followed, and, other circumstances being favourable, an embryo was produced; and when the head of a pin was employed to apply the fluid, then the usual result was full impregnation; so that these results confirm those by Spallanzani. The author further states that it appeared to be of no consequence as to which surface of the egg was touched, the dark surface, light surface, or the side,—the result was the same.

He next proceeds to show, that when the egg is immersed in *pure* seminal fluid a *directly opposite result ensues.* Segmentation then seldom occurs, and

the embryo is but rarely produced; and further, that the effect then produced on the egg is very similar in appearance to that of the *chemical action* of solution of caustic potass; the yolk becomes shrivelled and decays. These results he thinks are not explained by the views at present entertained respecting the nature of impregnation. The author then refers to the observations made by himself, and also by M. Quartrefages, which tend to show that no impregnation is effected, even by the contact of the spermatozoon with the egg, when all motion in the spermatozoon has entirely ceased; and he conceives that this fact, when considered with the results now obtained, leads to a new view of the subject.

The author then applies the view of the 'Correlation of Forces' to the investigation of the function of impregnation. He thinks that impregnation is commenced if not entirely completed, by what may possibly prove to be a new condition of force, in, and *peculiar* to, the impregnating agent, the spermatozoon, which he designates *sperm force*, and distinguishes from the force of growth and development in cells, through which the spermatozoon is produced. He further distinguishes it from the force of *muscular contractility*, and from that of the *nervous system*, and states that he regards the whole only as modifications of one common force, and as having correlations with the physical forces. In support of this view the author enters into details, and refers to some late analysis by Dr. Frerichs, to show that the spermatozoa, like muscle and nerve, not only have a definite structure, but also a definite chemical composition, and that this composition appears to be the same in different classes of animals. He thinks that the spermatozoa may thence be regarded as organs of a special modification of force, and that motion is the visible exponent of this form of force, since the spermatozoa are quite inefficient to impregnate when their motion has entirely ceased. The author further thinks that it is only by the adoption of views of this kind that the apparently contradictory results obtained are likely to be explained.

In the course of his observations the author states a remarkable fact, which he has repeatedly verified, and which he thinks is of importance—namely, that the *first cleavage or division of the yolk, in the egg of the Frog or Toad, corresponds in its line of direction to the longitudinal axis of the body of the embryo* of those animals; and this he proposes to show more particularly hereafter.—*Proceedings of the Royal Society*, June 19, 1851.

ON THE REPRODUCTION OF THE ASCARIS MYSTAX.

By Henry Nelson, M.D.

The author commences with a brief anatomical description of the *Ascaris Mystax*, found in the intestinal canal of the domestic cat; with more especial reference to the organs of generation in the two sexes. He traces the gradual formation of the semen; originally thrown off as seminal particles by the cæcal extremity of the tubular testicle, the exterior of each solid particle enlarges to constitute a cell, while the interior retains its consistency and forms a nucleus. The cell then acquires a granular protecting envelope, and in this state is introduced into the female. The solution of the protective envelope and the great enlargement of the seminal cell follow, and its nucleus is now seen to present a gradual structure. The external granules of the nucleus coalesce to form a membrane, at first exactly resembling a watch-glass in shape, but by the contraction of its margin ultimately forming a curved cæcal tube. This is the true spermatic particle or spermatozoon, and is set free by the rupture of the seminal cell.

The generative apparatus of the female, commencing also in cæcal extremeties, is next treated of, and the author draws particular attention to a transpa-

rent, narrow contractile portion, the oviduct intervening between the ovary and uterus, as the part in which the ovule encounters the spermatic particles, and is by them fecundated. The cæcal end of the ovary likewise throws off a solid particle, which enlarging forms a germinal vesicle and spot. As the germinal vesicle travels slowly down the tubular ovary, it acquires a thick granular investment or yolk, secreted by the ovarian walls. The ovules now present a flattened triangular shape, are placed side by side, and form one solid mass. At the commencement of the oviduct, however, they become detached, separated from each other, and propelled singly along its interior. Here the gelatinous ovule meets the tubular spermatic particles, and is surrounded on all sides by them. They are at first seen to be merely applied against the ovule; but by degrees the margin of the latter presents a rupture, some of the vitelline granules are displaced, and the spermatic particles become imbedded in the substance of the yolk itself.

While the penetration of the spermatic particles is going on, a chorion, secreted by the oviduct, surrounds the ovule, forming a spherical envelope, within which the germinal vesicle, the granular yolk, and the imbedded spermatozoa, are enclosed. The spermatic particles after penetration are seen to swell, become transparent, and ultimately to dissolve. The vitelline granules likewise disappear altogether, or are transformed into others of a different color; and lastly, the germinal vesicle is destroyed.

By tracing the changes of the ovule in unfecundated females of the same species, the author finds the appearance of the vitelline granules to be dependent upon, while the formation of the chorion is wholly independent of the influence exerted by the spermatic particles on the ovule.

As soon as the vitelline granules and germinal vesicle have disappeared, the whole interior of the chorion is filled with a clear fluid, in which a few granules and the germinal spot are seen to remain. By swelling up, this constitutes the embryonic vesicle and spot. A membrane separates from the interior of the chorion, and contracting on the granules forms a spherical yolk, in the centre of which is the embryonic vesicle. This is the perfect ovum. The subsequent divisions of the embryonic spot, vesicle, and yolk, are described; the author particularly pointing out the gyrations of the embryonic vesicle immediately after division. As soon as the whole interior of the egg has been filled by the subdivisions of the yolk, the external granules coalesce, and form a continuous membrane internal to the chorion, which by gradual depression on one of its sides forms first a fleshy cup, and then, as the membrane of its concavity touches that of its convex surface, acquires the form of a ring. The ring divides at some point of its circumference, the extremities become pointed, and thus the young Ascaris receives its characteristic shape. The author has frequently repeated his observations with a view to their verification, and has employed the camera lucida to render the illustrative figures as accurate as possible.—*Proceedings of the Royal Society.*

[This paper is of peculiar interest, especially when taken in connexion with the preceding. In the first place, the very early stage of development at which the spermatic cells are transferred from the body of the male to that of the female, is a fact which proves, with regard to the Entozoa, as did the similar observations of Mr. H. Goodsir on the Decapod Crustacea, how completely independent is the vitality of these cells, provided that they are supplied with the conditions necessary for their development. The point of greatest novelty, however, is *the entrance of the spermatic particles into the interior of the ovo,* of which the author speaks with the greatest confidence; while Mr. Newport asserts with equal confidence that no such entrance occurs in the case of the Frog. These two facts—for we are much disposed to place confidence in the accuracy of both observers—do not seem to us so discrepant as might at first appear. Our readers will recollect that a similar difference presents itself in the mode in which the contents of the "sperm-cell" and the

"germ-cell" come into relation in plants; for a complete intermixture takes place by the rupture and emptying of both these cells in the lowest cellular Cryptogamia, and a direct passage of the contents of the sperm-cell *into the cavity* of the germ-cell, in those a little higher; whilst in all the superior Cryptogamia, and in the Phanerogamia generally, the spermatic influence (communicated by the *phytozoaire* in the former, and by the pollen-tube in the latter) appears to be applied merely to the *exterior* of the germ-cell.]

ON THE BLOOD, IN ITS RELATION TO THE EXCRETIONS.

By Dr. [H. Bence Jones, F.R.S., &c.

One of the most interesting substances, and certainly the most important one, is carbonic acid, which passes out by respiration. By a very easy and beautiful experiment I can show you its presence in the blood. I have here an aparatus which will produce hydrogen. I have a tube full of caustic potash, which will stop any trace of carbonic acid which can possibly exist. Sulphuric acid is made to act upon zinc so as to produce hydrogen; this hydrogen passes through the solution of caustic potash; it then passes into another vessel, into which, when filled with the hydrogen, some healthy blood is put; the hydrogen bubbling through this, passes through some lime-water in other vessels, and if it carries with it any carbonic acid, the lime-water will of course become turbid. You see how rapidly this turbidity is produced. Carbonic acid, then, is a substance which exists in the blood, and is passing out each moment by respiration. The proportion of carbonic acid to oxygen, in arterial blood, is as 16 of the former to 6 of the latter; and, in venous blood, 16 carbonic acid to 4 oxygen. This proportion was determined for us by the German chemist, Magnus. He found that the quantity of nitrogen was the same in both kinds of blood. M. Majendie states, that in venous blood, in every hundred volumes, there are seventy-eight volumes of carbonic acid gass, and in arterial blood 66 per cent.

Other substances can be obtained from the blood which are constantly passing out of the body in the urine. The most interesting of these are uric acid and urea, substances which form the peculiar characteristic constituents of the urine. These can be found in small quantities in healthy blood. I have here a beautiful specimen in long crystals of urea obtained from the healty blood of an ox, for which I am indebted to M. Verdeil. It is obtained by drying the serum of the blood, reducing it to the finest powder, mixing it with alcohol, and then pouring off the alcoholic solution, which, in health, always contains small quantities of urea. In some diseases the quantity of urea in the blood is considerable—as for instance in Bright's disease. In this disease the blood-globules are exceedingly diminished—the albumen is constantly passing out of the urine; and it is always found that urea is one of the constituents of the serum. It may be obtained thus:—Here is the serum of a patient who was bled in St. George's Hospital. Here is a portion evaporated to dryness; a part of the dry residue is treated with absolute alcohol, the alcoholic solution is evaporated in vacuo to dryness; and the dry residue is disolved in a little water; on the addition of nitric acid, nitrate of urea, as you see, immediately crystallises.

Uric acid is also found in the blood in health and in disease, combined with soda. It was discovered by Dr. Garrod, of University College; he states that it exists in increased quantity in the blood of gouty subjects; and, from my own experiments, I can confirm the truth of his statement. Dr. Garrod also says, that he found in Bright's Disease urate of soda in excess in the blood. In that disease the kidney is prevented from performing its proper functions; the ingredients of the urine are not separated as they should be, and thus urea and uric acid accumulate in the blood. Uric acid, like urea, can be easily

detected, by taking the serum, or the blood as a whole, evaporating it to dryness, reducing it to the finest powder, and treating it with boiling water; urate of soda will thus be obtained in solution. The liquid is filtered off from the insoluble albumen, and the clear fluid is mixed with strong acetic acid, and set aside to crystallise. The uric acid adheres to the sides and bottom of the glass. It may be collected, and will give the characteristic reactions with nitric acid and ammonia.

Kreatin, which I formerly mentioned as one of the constituents of the flesh, probably exists in the blood. It exists certainly in the urine, as I shall have to show you. Hippuric acid, also, which exists in the urine, especially in graminivorous animals, has been found in the blood. It was detected in the blood of an ox, by M. Verdeil Lastly, Dr. Garrod considers that he has found oxalic acid in the blood of a patient in University College Hospital.

Thus, then, there exist in the blood, not only the substances which pass into the body as food, but the substances which pass out in the excretions. I have said that the great peculiarity of the blood is, that it contains fibrin and the red globules; these substances cause the blood to differ from all other fluids. The spontaneous coagulation and the red colour are caused by the globules and the fibrin; neither of which exists ready formed in the food, nor are they ever found in the healthy excretions. If it were not for these substances, it might almost be said that the blood was nothing but a solution of food passing in, and of substances passing out of the body; it is then by the formation of the fibrin and blood-globules that the blood is made a peculiar substance,—an organized liquid, which may live and die like the more solid organs of which we are composed.—*Medical Times.*

MEDICINE.

ON THE NATURE AND TREATMENT OF EPILEPSY.

[The following interesting discussion took place at a recent meeting of the Medical Society of London.]

Dr. Radcliffe then read a paper on this subject. He first drew attention to the *temperament* of epileptics, and showed that this was distinguished by unequivocal marks of weakness and depression: signs of scrofula or some other cachectic disposition, of depressed and feeble circulation, of defective nervous activity, of muscular feebleness, might always be detected, but never the signs of true plethora or of hyper-activity in the nervous or any other system. When epilepsy had shown itself in persons distinguished by their genius and talent, it was in the state of exhaustion induced by the exercise of that genius or talent; when it was associated with insanity, the convulsive disorder coincided with the intervals of depression, and never with the periods of quasi-excitement. After describing the phenomena of epilepsy, he proceeded to point out the continuance of the some signs of depression and exhaustion, and to show that the change which had taken place was always one of aggravated depression and exhaustion. This he did by a special examination of the condition of the vascular and nervous system. Immediately before and after the fit the pulse was shown to be weak and collapsed, and often irregular and slow, and in the fit itself little or no blood was found to be propelled into the vessels. This condition of the circulating system entailed a corresponding failure in the activity of the several nervous centress. He argued, also, that the brain was inactive, because the epileptic was silent, sad, moody, and generally still, before his seizure; completely bereft of sensibility, consciousness, and volition in his seizure; and stupid, confused, and exhausted afterwards. He argued also from the true appearances found after death. He noticed the views of Dr. Davey

and Henry Monro in connexion with insanity, as corroborating this conclusion. He advanced arguments to show that the medulla oblongata, spinal cord, and smaller ganglionic centres, were in a corresponding state of inactivity. Dr. Radcliffe then insisted upon the absence of any local disorder as a cause of epilepsy, and said that the only way in which any such disorder had to do with the matter, was in aggravating the general dibility and prostration of the system. Under this head he went on to notice the views of Dr. Marshall Hall. He contended that in epilepsy there was no proof whatever of any increased irritation in the spinal cord, any more than in the medulla oblongata and brain, but that there were abundance of proofs of a directly opposite condition. He doubted that trachelismus and laryngismus, with the consequent cranial and cervical engorgement, had any necessary connexion with epilepsy. He did this because there were distinct contractions in the limbs and elsewhere, before the occurrence of the spasmodic tightenings of the muscles of the neck and larynx, and because the fit ceases when the congestion was at its height—so that he conceived Dr. Hall's theory had two insuperable difficulties to contend with, the one that the fit had actually begun before it ought, (that is to say, before the congestion had showed itself,) the other that it ceased when it ought to have been most violent, (that is, when the congestion was at its height.) He (Dr. Radcliffe) argued also against the hypothesis of trachelismus and laryngismus, from its non-applicability to very many cases of epilepsy, in which cases, and in many other convulsive disorders, no such phenomena could be detected. He said further that this hypothesis did not account for the insensibility of epilepsy, for, in his opinion, this insensibility (which was much more frequently of the nature of syncope than coma) was, as a general rule, due to a syncopal condition of the circulation rather than to any venous congestion in the vessels of the brain produced by the spasmodic tightening of the muscles of the neck. The violence of the muscular contractions or convulsions in epilepsy, Dr. Radcliffe said, was no objection to the existence of the most positive prostration and depression; on the contrary, this very phenomenon was the best proof of the existence of that state. Muscular contraction, physiologically as well as pathologically, was always (he asserted) the sign of some withdrawal of the nervous and other stimuli which appertain to the muscles, and never the result of the communication or importation of these stimuli; and for the confirmation of this opinion he referred to his published views on muscular physiology and pathology, and to the facts which had just been stated in connexion with epilepsy. Upon the treatment, he argued at some length against low diet, and in favor of the most nutritious food, with stimulant and corroborative drinks, and against over-exercise in favor of rest *rest*. Citing many other arguments, he conceived that the non-existence of vascular or nervous excitement, and the existence of a directly opposite condition, was itself an insuperable objection to bleeding and purging in this malady, and an argument for the necessity of stimulants and tonics, and all means which could corroborate the system. Narcotics, counter-irritants, and emetics were condemned. The convulsion-exciting properties of strychnia were stated to be an argument against rather than in favour of that drug. He objected also to the tracheotomy in the cure of epilepsy, on the ground that there were many cases of that malady in which the larynx was not sensibly affected, and in which the impediment to the respiration was rather owing to irregular action or spasmodic fixation in the thoracic muscles and diaphragm, than to mere closure of the larynx.

Dr. Davey concurred in the views advanced by Dr. Radcliffe, and mentioned, that in the Asylum at Colney Hatch, epileptics, who were usually admitted in a low state vitality, were best treated by tonics and a judicious and discriminating diet. He related several cases to show that this treatment had been attended with the best results. In some cases wine and porter were added to nutritious diet. He expressed his belief that in the treatment of all nervous disorders, practitioners had gone too far generally on the antiphlogistic system, by which he was sure many cases had been rendered incurable. Kind

treatment, the avoidance of mechanical restraint, added to proper diet and regimen, had been found the best improvers of the mind and health, of the great majority of those who came under his care at the Coney Hatch Asylum.

Mr. Richardson agreed with the author of the paper, that the attempt to localize the seat of epilepsy, especially in the brain, had been a failure: and mentioned a number of cases in proof. He differed with Dr. Radcliffe as to depression generally producing the epileptic seizure, and mentioned a case in particular where the fit came on during exertion, which had been carried to fatigue. He differed also in thinking that epilepsy in talented persons usually came on after the brain began to fail in power. With respect to remdies, he thought, as a rule, that spirituous liquors did harm, and porter sometimes brought the epilepsy on. He eulogized the employment of tartar-emetic and valerian, and the use of issues and counter-irritants. Small bloodlettings were also sometimes admissable.

Mr. Dendy thought Dr. Radcliffe's treatment opposed to his theory. He (Dr. Dendy) suggested a combination of remedies as useful in some cases; such as the abstraction of blood to remove congestion, which might exist locally, as in cholera, even in otherwise healthy states of the system, and then to give tonics and support immediately. He thought that in all cases of epilepsy some lesion of the nervous system must exist. He complained that hallucinations, insanity, and other subjects had been mixed up in the discussion with the with epilepsy.

Dr. Webster agreed with the author in considering epilepsy as generally a disease of exhaustion, and most frequently it affected persons of debilitated, broken-down constitutions. The complaint was also more apt to occur in parties endued with a scrotulous diathesis, especially if their parents had also suffered from the same affection. Indeed, hereditary tendency exerted considerable influence, and he considered epilepsy very liable to be transmitted to offsprings, like some other maladies of that character. According to his (Dr. Webster's) experience, it was more frequent amongst the lower than the upper ranks, both in this country and in France; whilst he would further say, it often attacked males than females. This was certainly the case in many French asylums which he had recently inspected, where male epileptics predominated considerably. Respecting the causes often producing epilepsy, he considered terror as one of the most powerful; of which a very striking example some time ago came under his observation. It was that of a young woman, who was frightened by a fellow-servant disguised as a ghost, with a light in his hand, when he suddenly appeared before her at the end of a dark passage. She became so alarmed as to fall down in a fit of epilepsy, which afterwards frequently returned: and in one of these violent seizures Dr. Webster attended the patient. This disorder he considered almost incurable during the latter periods of life, or even in adults, especially when complicated with insanity. Instances of recovery might be occasionally reported; but they were so rare as to render the prognosis always unfavourable. In early age, or before purberty, the prospect of recovery was much greater, and he might refer to several cases proving this inference, but it seemed unnecessary, as the fact must be well known to practitioners. Dr. Radcliffe's observations relative to the treatment of this often terrible disease coincided very much with the principles he (Dr. Webster) would recommend. Respecting bleeding there could not prevail two opinions, and to use the lancet was most objectionable. Even the topical abstraction of blood in youn plethoric subjects, required great caution, and then only to relieve local congestion. With the author Dr. Webster entirely agreed regarding the use of purgatives, although he would not employ drastic cathartics, as similar remedies occasioned too much debility. Allusions having been made to various mineral preparations at one time enjoyed considerable reputation in epilepsy, but now seldom reputed efficacious, he (Dr. Webster) must mention one recently employed by a friend of his own—namely, Dr. Fornasari, physician to the

Fams lunatic asylum in France, which he had visited last autum. The remedy war valerianate of zinc, given in doses from half a grain to one, night and morning, which might be increased to three grains per day. Occasionally purgatives were also prescribed, and frequent baths, the diet being also carefully regulated. Dr. Fornasari speaks favourably of the benefits produced; and several cases then in the asylum had derived so much relief, that fits which at first recurred ever three, six, or eight days, had not supervened for more than three months. Supported by the above authority in favour of valerianate of zinc, Dr. Webster administered it lately to a patient labouring under epilepsy, and apparently with such advantage as would induce him to recommend employing the same mineral in other examples. Although nutritious diet and generous regimen were often escential for epileptic patients, he thought indigestible food frequently acted in an injurious manner. Indeed, indeed a full meal of improper substances often proved an exciting cause; and he could quote one case which came under his own observation, where a person having eaten freely of fried bacon and eggs at supper, was seized with so severe a fit, about three o'clock next morning, that death followed in consequence. Notwithstanding wine and malt liquors, even in large quantities, had been recommended by several fellows, such stimulating beverages might be taken too freely; and he must remark, unless under special circumstances, much porter or ale was by no means so useful as wine diluted with water, where stimulants were really required. Great caution, therefore, became necessary when adopting that kind of treatment. Before sitting down, Dr. Webster oeserved, although he coincided with Mr. Richardson in opinon that many lesions of the brain and nervous system did not produce epileptic seizures, still these affections generally depended upon or indicated organic changes of structure within the cranium; at least, his individual experience warranted such conclusions respecting the pathology of epilepsy.

Dr. Radcliffe, in answer, said, that the very extended practice of Dr. Davey as to the necessity of good diet with wine and beer in epilepsy, was a strong argument in favour of the view he had advocated. He said any one would be sensible of the advantages of such a course, who remembering the appearance of epileptics in our own or in foreign hospitals a few years ago, now paid a visit to Colney Hatch or Hanwell. He would at leat learn that good food and wine and beer did no harm. In answer to Mr. Richardson's objection that the epileptic was not always depressed before the fit, he called up Mr. Richardson's own admission that he had not watched that point particularly. To another objection from the same gentleman, that Mohomet was epileptic during the most vigorous period of his life, he answered that Mohomet saw visions in his fits, and that on that account those fits could not be epileptic, inasmuch as the consciousness is suspended in epilepsy. He thought it better to reason from recent cases, the particulars of which were better known, and from the general history of the disease; which being done, he (Dr. Radcliffe,) though Mr. Richardson would be obliged to admit that the system of the epileptic was always marked by prostration, and most of all, so marked in the fit itself. In reply to Mr. Dendy's defence of bleeding, he thought the utter absence of plethoric excitement and of nervour hyper-activity, and the presence of signs directly opposite to these in their nature, together with the absence of any ill effect from the generous treatment pursued at Colney Hatch and elsewhere, were insuperable objections to bleeding in any form. If Mr. Dendy took exception to Dr. Davey's arguments for a good diet and wine and beer from his experience and particular views of the nature of insanity, he must object to the necessity of bleeding in epilepsy being deduced from what Mr. Dendy had seen in cholera. Nor could he admit the soundness of the practice of combining remedies of opposite qualities, as local bleeding with tonics, which practice in his opinion was the relic of the ancient practice of jumbling all manner of remedies together, in the benevolent hope that one or other of them might chance to do good.

ON THE CATARRHAL PNEUMONIA AND LOBAR PNEUMONIA OF CHILDREN.

By MM. Trousseau and Lasegue.

Catarrhal (or lobular) pneumonia is a disease as distinct from simple (lobar), as variola is from erythema. This is seen in their respective mortality. Of twenty children who have been admitted to the hospital clinique, suffering from *simple pneumonia,* in six months all have recovered; of nearly thirty who were attacked with *catarrhal pneumonia,* not one survived. Most of the first class of cases exhibited an excessive degree of acuteness which burnt out like a fire of straw; while several of the second, notwithstanding their fatal termination, commenced with very mild symptoms.

Simple pneumonia hardly ever affects a child below two years of age, and rarely those of two or three, but becomes of more and more frequent occurrence as the child approaches adolescence. Its cause and symptoms resemble those of the adult, with some modifications. After twenty-four or thirty-six hours, the souffle and bronchophony can alone be heard; the crepitant rale, which is often observed in the adult when the patient coughs, even when much souffle is present, is hardly ever heard in the child. So afterwards, from day to day, without the crepitation of resolution, the souffle disappears, leaving only a feeble respiration. The progress of the disease is also more rapid than in the adult. In the mild form of the disease, recovery takes place rapidly, and in large proportion; but in its grave form many cases are lost by any mode of treatment. M. Trousseau generally bleeds the child, gives it an emetic of sulphate of copper, and then a mixture, containing Kermes mineral and extract of digitalis.

Catarrhal pneumonia commences with a catarrh, which rapidly extends to the small bronchi, and then we hear numerous and small subcrepitant *rales* disseminated over both lungs, and especially posteriorly. These *rales* may persist for four, six, eight, or fifteen days, without any *souffle* becoming manifest; but sooner or later we hear a *souffle,* the resonance of the cries or the voice, or at least a prolonged respiratory murmur. While these latter sounds, common to simple and catarrhal pneumonia are thus manifesting themselves, we find, by the subcrepitant *rales,* that the capillary catarrh is still persisting in the rest of the lung. The disease has extended from the mucous membrane to the parenchyma of the organ. Febrile action is less than in ordinary pneumonia, being predominant at some portions of the day, and entirely ceasing at others; and these alternations of better and worse may continue for fifteen, twenty, or thirty days; the disease being originally a pulmonary catarrh, and partaking of the obstinacy and uncertainty of catarrhal complaints. As more and more of the parenchyma becomes implicated, the fever becomes more continuous and intense, and the respiration more difficult, until the children die exhausted. In other cases, in which the bronchial phlegmasia was very intense from the first, and the lung became rapidly invaded over a great extent, death takes place with rapidity. The progress of the disease has usually been more rapidly fatal; when it has succeeded to measles, chronic disease of the skin, or laryngitis. All means of treatment that have been tried have proved impotent.

These two affections may be compared, *exceptis excipiendis,* with erysipelas and phlegmon. Erysipelas traverses the surface, like the catarrh; and when it persists too long, it induces ulcerations of the skin, furuncles, and circumscribed subcutaneous abscess, just as the capillary catarrh induces suppuration of the lobules, little abscesses of the lungs, and circumscribed pneumonia. Simple pneumonia, on the other hand, progresses like simple phlegmon, violent in its febrile reaction, but terminating abruptly and rapidly.

It must not be supposed, from what has been said, that catarrhal pneumonia is almost invariably fatal. Although this is the case amidst the miasmata of

an hospital, which exert effects at once so terrible and so difficult to avert, it is not so in private practice. In this, one half the patients may be cured, by repeated vomiting, flying blisters, antimonials, and digitalis; but how terrible are the ravages of a disease, which, under the most favourable circumstances, kills one-half its subjects!—*L'Union Medicale.*

MIDWIFERY.

STATISTICS OF THE LYING-IN INSTITUTION AT MAYENCE.

By Dr. F. Kilian.

These statistics embrace 42 years (1806—48), during which period 7,739 women have been delivered at the Institution. Some of the particulars of their cases are but imperfectly recorded; but those which have been noted are interesting, as exhibiting the results of a very uniform system of practice followed by the successive directors of the establishment. This practice is founded upon the maxims inculcated by Boer, of leaving the cases as much as possible to nature, and avoiding all meddlesome interference.

The 7,739 mothers produced 7,833 children (91 twin births, and 1 triplet), 7,369 being born alive, 464 still-born, and 147 dying in a few days—that is, 611 still-born or dying (nearly 1 in 19). The presentations were as follows:

Head	7559
Face	38
Arm	31
Breech	115
Feet	81
Knee	5
	7829

Of the 7559 children offering *cranial* presentation, 333 were born dead—several, however, having died prior to the commencement of labour, as attested by their putridity — leaving 236 (1 in 32), who died during labour, including premature births. In 92 of the 7559 cases, aid was required. Of the 115 *breech* presentations, there were 16 twin cases; and in 20 the child was premature. Assistance was required in 7 cases.—Of 81 *foot* presentations, 19 were twin cases, and 23 were premature children. In 2, aid was required for the delivery of the head.—Of the 38 *face* presentations, in 3 only were the forceps employed. All were born alive except 2, in whom traces of putrefaction were present.—Among the 31 cases of *arm* presentation, 5 were twins, and 2 premature births. All were turned: 12 being born alive, 12 still-born, and 7 having died prior to labour.

Premature Births.—These amounted to 250 (1 in 31), the proportion varying much in different years. A far larger proportion occurs in the latter half of pregnancy than in private practice, exceeding those of the first half by 11 to 1: the greatest absolute numbers occurring in the seventh and eighth, the least in the fourth month. There were 13 twin births; 114 children being born alive, and 149 dead. There were 215 cranial, 20 breech, and 23 foot presentations; 5 being unrecorded.

Prolapsus of the Funis.—Of this, 32 instances are recorded, being, Dr. Kilian believes, far fewer than really occurred. Of the 32 children, 11 were born alive, 17 still-born, and four putrid or non-viable. Interference was resorted to in seven cases: in four of which (2 living, 2 still-born) turning, and in 3 (all living) the forceps were employed. Of the 25 cases left to nature, 7 were born living, 4 were putrid and non-viable, and 14 were still-born. In 4 of cases the cord had remained too long pulseless to justify interference, while

in 2 born *living*, it did not pulsate during labour, nor until some moments after its completion. Dr. Kilian considers that no general rule can be laid down for the treatment of these cases; but, without declaring so absolutely in favour of this view as Hoffman of Wurzburgh does, he believes that the majority of cases should be left to nature; the possible ill-consequences of our interference, to the mother as well as to the child, inculcating the necessity of restraining it within the narrowest possible limits.

Operations.—The *forceps* were employed 79 times (1 in 99), 61 of the children being born living and 18 dead. Dr. Kilian attributes the rare use made of this instrument (compared to what is usual in continental practice) to the *little intermeddling which takes place in the early stage of labour*, whether for the purpose of regularizing abnormal activity of pain by depletion and other means, or of exciting it it, when defective, by ergot or stimulating drinks.—*Turning* is recorded as having been performed in 34 cases—a number the author believes to be below the real one. In three of these the mother died; in one instance, suddenly, while the lips were passing; *post-mortem* expulsive action completing the delivery. Of the 34 children, 15 were born living, and 19 dead—*Perforation* was resorted to in three cases.

Maternal Mortality.—Of the 7739 women delivered, 41 (1 in 188) died: 11 of these deaths occurring in women who had undergone operations for delivery, and 30 among those whose cases were left to nature. In 22 cases there was something remarkable in the progress of the labour, or in the placenta. In 14 cases death occurred from peritonitis, and in 17 the cause is not stated. In explanation of so small a mortality, the practice of non-interference, so strictly observed, is to be borne in mind; as is the remarkable fact, that during the 42 years of the existence of the establishment, it has never been visited by an epidemic or *puerperal fever*. Among the circumstances which may be supposed to favour the exemption from this scourge, is the fact that no clinique for medical students exists, and there is, consequently, far less mental and physical disturbance of the women during labour than in establishments where there is a clinique. Deaths, too, are known by the patients to be rare; and when they do occur they are carefully concealed from the cognizance of the other inmates; and indeed all sources of mental disquietude are sought to be avoided. Attention to cleanliness and ventilation also prevails. It is not, however, meant to be asserted that by these and other precautions puerperal fever can always be prevented; and indeed the town of Mayence seems to share the immunity of the hospital, as the oldest practitioners cannot call to mind the prevalence of an epidemic of this disease, even when large portions of the continent had been ravaged by it. The same immunity prevailed with respect to the cholera of 1832 and 1849.—*Neue Zeitschrift fur Geburtskunde.*

ON DELAY IN DIVIDING THE FUNIS.

By Dr. Storer.

Dr. Storer observes that it has been for many years a rule with him to wait until all pulsations have ceased to be felt before applying a ligature to the funis. Early in his career he met with two cases in which profuse bleeding occured from the funis after it was divided, which could not have happened had not the blood continued flowing on the cord. As in the vast majority of cases the pulsation ceases in a few minutes (in one only has he known it continue for twenty-five), it seems best to listen to the dictates of nature, as considerable danger of injuring some of the important viscera may arise from too suddenly diverting circulation upon them. It is not unusual to find the funis pulsating with great force at birth; and is it not more rational to allow this to subside gradually, than to check it all at once? May not some of the cases of hæmorrhage from the mouth and nose, which have been published, be due to the

pulmonary congestion thus caused? Dr. Channing recently met with a case in which the child, born apparently quite healthy, died while being dressed from violent hæmorrhage from the mouth, none proceeding from the funis, which had been tied directly after birth.—*Amer. Jour. Med. Sc.*, vol. xxii. p. 82.

FORENSIC MEDICINE.

CASE OF DEATH BY CHLOROFORM—POST-MORTEM EXAMINATION.

The autopsy was conducted by Mr. Paget; and our readers will easily perceive, by the following details, that the examination does not yield any clue to the suddenly fatal effects of the inhalations of chloroform. It would certainly be a great pity if this accident were to render surgeons loath of availing themselves of the advantages of anæsthetic agents, for it cannot be denied that operations are now-a-days far more effectual and safe than formerly; and that, independently of the absence of pain, there are a certain number of surgical measures which, with chloroform, may be had recourse to with great ease; but which, without the assistance of this agent, could not be thought of.

From the numerous operations which we have seen, and from the valuable works which have been published on the subject of anæsthesia with ether or chloroform, we cannot but think that the chance of escaping accidents would be far better, if those who administer chloroform would take more time in obtaining insensibility, and allow the chloroform to be mixed with a large quantity of atmospheric air. Patients might inhale the anæsthetic agent in the ward, (as is always done at the University College Hospital, among Mr. Erichsen's patients,) and the proper time might thus be afforded. M. Sedillot, of Strasbourg, has lately written on the subject, and is firmly of opinion, that, with more time, and a greater waste of chloroform, more security would be obtained.

Dr, Snow, who so frequently administers chloroform in this metropolis, has lately read a paper before the Medical Society of London in which he states:—

"When dogs, cats, or rabbits were made to breathe air containing from three to five per cent of vapour of chloroform till they died—a process which occupied generally from ten to fifteen minutes—the heart continued to act for a minute or so after breathing had ceased, as he had ascertained by means of the stethoscope; and then in some instances, the animal gave a few gasping inspirations, about the time when the heart was ceasing to act, which had the effect of restoring it to life. On the other hand, when such animals were made to breathe air containing eight per cent., or more of the vapour, *death took place very suddenly*, the respiration and the heart's action ceasing together. . . . . He believed that no accident had occurred from the continued exhibition of chloroform vapour, *well diluted with air*, (the italics are our own.) In the fatal cases which had happened, death had taken place suddenly by way of syncope, showing that the heart had been paralyzed by the action of vapour constituting not less than eight or ten per cent. of the air inspired just before death. . . . . There were two methods of insuring the dilution of vapour of chloroform with atmospheric air, to such an extent that death could not occur without giving sufficient warning to allow of accidents being prevented by ordinary attention and skill. The first and best of these methods was, to exhibit pure chloroform by means of a suitable inhaler; the other method was to dilute the chloroform with rectified spirit of wine, before pouring it on a handkerchief or sponge. Equal parts, by measure, of each, is the proportion which Dr. Snow is in the habit of using; and he thinks that the best means to be employed, in case of impending death from chloroform, is *artificial respiration*."

It would also be wise, if, in each large hospital, some gentleman,

remunerated for his trouble, were *exclusively* entrusted with the administration of chloroform, (as is the case at St. George's Hospital;) such a person would then naturally gain much practical experience in the manipulation of the narcotizing agent; and the surgeon could, without having his mind worried by apprehensions, give his whole attention to the operation in hand.

Post-mortem Examination.—Body well formed and muscular; rigor mortis complete in the trunk and limbs. Venæ innominatæ, and vena cava superior, full of blood, and probably would have been distended, but that some blood had flowed into the coffin from the opening of the external jugular vein. The right auricle and ventricle were full of blood, and would probably have been more so, but for the escape of blood just alluded to. The left auricle and ventricle contained very little blood, and the left ventricle was in a complete state of contraction. The heart was of full size; it appeared in every part natural in its texture, and as if it had possessed full power. All the valves were also healthy. Neither could any disease be traced in any of the large blood-vessels within the chest. The blood, however, was very fluid, and did not coagulate after its escape from the heart and vessels. It had a brownish-purple hue, like that which is generally observed in the spleen; none of it, when thinly spread out, presented the ordinary dark, black or crimson colour of venous blood.

Both lungs presented old adhesions about their apices and posterior surfaces; but these were of small extent. The pulmonary texture was healthy, but the lungs appeared more than usually collapsed and dry. The vessels were not overfilled; the mucous membrane of the large bronchi and trachea was turgid, apparently from congestion of its smaller blood-vessels. A similar condition existed in the larynx, above the chordæ vocales, but not to such an extent as in any appreciable degree to cause a narrowing of the glottis.

The mucous membrane of the stomach was, over a great extent, especially at the fundus, blotched and suffused, and presented a dark, crimson colour, from the exceeding fulness of its veins and small blood-vessels; but the coats of this viscus appeared healthy. It contained a small quantity of thin, brownish fluid, being probably the remains of the last meal. The whole intestinal canal, as far as can be judged from an external examination, appeared healthy. The liver, pancreas, and spleen were natural; and the hepatic venous, plexuses, and intra-lobular veins, seemed over-filled.

In the kidneys, which were of natural size and texture, the tubular portions were very dark, apparently with intense venous congestion; but the cortical portion was comparatively pale. The vena cava inferior, and its chief branches were more than usually filled with blood.

The skull was natural, except in small portions of the diploe, in which a congested state of the blood-vessels, corresponding with the disease in the vicinity of the ear, was noticed. The dura mater and longitudinal sinus presented nothing abnormal; the cerebral arachnoid membrane was in many parts, and over a wide extent, opaque, and somewhat thickened; and a few ochre yellow small spots also appeared in it. The tissue of the pia mater was infiltrated with more than the ordinary quantity of transparent fluid. Between the anterior lobes of the cerebral hemispheres, small portions of the opposed surfaces of the arachnoid membrane were adherent; but both this and all the other morbid conditions of the membranes of the brain appeared to be the results of disease which had probably existed long previous to death.

The convolutions of the cerebrum were small, and the furrows between them of wider extent than usual. The surfaces of the optic thalami were uneven and wrinkled, as if these portions of the cerebrum had somewhat contracted; but no unnatural appearance presented itself in any other part of the brain or medulla oblongta.

Every possible exertion has used to resuscitate the subject of whose post-mortem examination we have just given an account. Dr. Herepath, of Bristol, considers that the electric current, steadily kept up between the mouth and diaphragm, is our sheet-anchor; and we have no doubt that this advice will

be acted upon in the event of another accident with chloroform. We cordially recommend the perusal of Dr. Herapath's paper, both as to his views regarding the cause of death by chloroform, and the resuscitating means to be employed. The following passage should be particularly attended to.

"In resuscitating from an over-dose of chloroform, galvanism is the only chance. Keep up a current of electricity through the fifth nerve, medulla oblongata, phrenic nerves, and diaphragm, as long as respiratory movements can be produced, and let the patient have plenty of fresh air or oxygen gas, and the case must do well, for the blood will remain fluid for a long time, and circulation will go on as long as respiration continues to be carried on artificially. The blood and the air cells throw off their load, and in proportion as the pneumogastric, medulla oblongata, and motor nerves, slowly resume their functions, so respiration begins to assume a less artificial character; at length the cerebrum aids us, and respiratory movements, both voluntary and involuntary, keep up the functions of life unaided."

But we would also call the attention of surgeons for a few moments to the following extract from a foreign journal. The method therein mentioned, and which has several times been the means of saving life, may perhaps prove serviceable in this country.

Prof. Rigaud relates the following case in the *Abeille Medicale* of Nov. 3d, 1851:—He was on the point of removing a tumour from the chest of a female patient. After a few inspirations of chloroform, the pulse stopped suddenly, and the woman did not give any signs of life. The chloroform was at once removed, cold water dashed on the face, and frictions made all over the body. These means, in about a couple of minutes, produced a few weak pulsations of the heart, which, however ceased almost immediately, and were not accompanied by any respiratory act. Dr. Rigaud now thought of using the method which has been advocated by Dr. Escalier, and passed his index finger along the dorsum of the patient's tongue, raised the epiglottis, and drew the former out of the mouth. This had the effect of producing an inspiration, which circumstance was taken an advantage of to make the patient inhale ammonia. As soon, however, as the tongue was lost hold of, it glided back into the mouth and respiration ceased again. The same manœuvre was now repeated, but this time Dr. Rigaud kept the patient's tongue out of the mouth; the respiration then set in again, and the woman quickly recovered. The operation was then performed without chloroform, and Dr. Rigaud considers that Escalier's method saved the patient.

THERAPEUTICS.

ON THE TOPICAL USE OF CHLOROFORM.

By Dr. Rauch.

To obviate the volatile character of chloroform when employed topically, Dr. Rauch combines it with olive oil and some liquor ammoniæ, forming an emulsive liniment. This is less expensive, relieves sooner, and is not so volatile as chloroform. The ingredients were at first employed in equal parts; but were afterwards used in other proportions, according as to whether a counter-irritant effect (when more ammonia and chloroform must be added) were desired or not. It is applied on a woollen cloth, so folded that the inner layer is saturated by the liniment, and the outer kept dry, so as to prevent evaporation. When first applied, it feels cool, then smarts and burns so for ten minutes as hardly to be borne; and then an agreeable coolness, with relief of pain, succeeds. When it causes too much irritation or vesication, it should be removed, or applied to another locality. The skin is made red by it, and often vesicated;

and if a mere rubefacient is required, it should be applied by friction, or the cloth should remain on only for a short time. When a speedy vesicant effect is required, it is more useful than a sinapism or blister, and is easier of application, especially in children, who often fall asleep during its application. Dr. Rauch found it of great use, combined with other means, in cholera; and in relieving the painful affections of the abdomen in children, it is preferable to any anodyne. In the case of superficial burns, a compound of equal parts of chloroform, olive oil, and lime water, has been found highly useful.—*Amer. Jour. Med. Sci.*

ON IODINED OIL.

By M. Guibourt.

M. Guibourt recently read a report of a committee, consisting of himself and MM. Souberain, Gibert, and Ricord, appointed by the Academy, to consider how far a definite combination of iodine with oil might be employed with advantage therapeutically. M. Marchal first proposed the employment of this substance in 1848, under the idea that iodine was the active element in cod-liver oil, and that a preparation containing a large proportion of this substance would prove of advantage. Since then, MM. Personne and Deschamps have each proposed formulæ for its preparation. M. Personne believes, that small as is the proportion of iodine in cod-liver oil, it is a very important ingredient; and without expecting in all cases to replace cod-liver oil he believes that a larger proportion of iodine, combined with an assimilable substance, may prove a highly valuable medicinal agent—the oily vehicle enabling it to penetrate into the economy, and abandoning it there gradually as it itself is burnt off during respiration. According to his plan of precedure, five parts of iodine are disolved in 1000 of sweet almond oil, and a jet of aqueous vapour is passed through the mixture until quite decolorised. Five other parts of iodine are now added, and the decoloration similarly performed. No traces of vapour of iodine are perceived; but the vapour of the water which is condensed exhibits strong traces of the presence of hydriodic acid. The oil is washed with a weak alkaline solution, as long as any acid reaction appears, and is then filtered. By adding successive quantities of iodine, double the quantity may be combined, but it is then difficult to avoid obtaining a high-coloured liquid.

In this way an oil may be produced, differing little in taste or appearance from almond oil, so that it can be easily administered alone or in emulsion. When forty grammes of it are swallowed, an hour and a half elapses before it shows its presence in the saliva. It becomes more and more manifest during twelve hours, and then diminishes, being still very sensible after eighteen or even twenty-four hours, but quite disappearing after thirty. M. Deschamps produces an iodined oil by the agency of alcohol; but the reporter regards it as nowise superior to that prepared more easily by M. Personne's method.

M. Gibert has experimented with both these oils. In several cases of chronic impetigo, in which it has been employed internally and externally, a rapid resolution of the eruption has resulted, more rapid than under the agency of cod-liver oil, which is so much more difficult of administsation. It has failed in several chronic scrofulous *engorgements*, which had also resisted cod-liver oil. M. Gibert's experiments with the oil have not yet been sufficiently numerous to enable him to estimate its exact value; but he is even now enabled to state that it possesses considerable resolvent power in certain of the chronic eruptions and glandular enlargements. M. Ricord has employed it during a year in a great variety of scrofulous affections, most of which have been mistaken for syphilitic disease. He has derived excellent effects from it in strumous bubo, tubercular epididymitis, and in some cases of scrofulous engorgements of the

joints. In these cases, satisfactory effects have much more promptly followed the use of the iodined than of the cod-liver oil. M. Puche, M. Ricords's colleague, has come to a similar conclusion. The dose given has been usually 60 grammes per diem; but this has frequently been increased to 100. It is usually well borne, only exciting purgation exceptionally, when large doses are given. —*Bull. de l' Acad.*

CHEMISTRY.

ON THE FIBRIN OF MUSCLE.

By Professor Liebig.

The substance of muscular fibre has been commonly designated as *fibrin*, and has been considered identical with the fibrin of the blood. This, however, is an error, as Professor Liebig has proved by the following experiments.

When the fibrin of blood is treated with water acidulated with a tenth part of hydrochloric acid, it soon swells and becomes changed into a gelatiniform mass, which is soluble in boiling water; if more concentrated acid be added, however, it returns to its primitive volume, again swelling when water is again added. This experiment may be repeated several times, without any notable proportion of the fibrin being dissolved. On the other hand, the substance of muscle very speedily dissolves in water containing hydrochloric acid in the above-named proportion, and this at the ordinary temperature; the solution is somewhat viscid, and is rendered slightly opaque by the presence of fat. When neutralized, it coagulates; and the coagulum dissolves in an excess of alkali, or in lime-water —but not in the latter if it have been first boiled.

The proportion of this fibrin readily soluble in dilute acid, is very different in the several kinds of animal flesh; thus the muscular sustance of the common fowl and of the ox dissolves almost entirely; that of the sheep leaves a considerable undissolved residue; whilst that of the calf does not yield more than half its weight to the dilute acid. This insoluble residue is white and elastic, but more gelatiniform and less white than the blood-fibrin swollen up by dilute acid.

The so-called fibrin of muscle is stated by Strecker to have rather the composition of albumen than that of blood-fibrin, containing less azote than the latter; but we must own that we distrust all minute differences in these organic analyses. The differences between two of Strecker's analyses of this very substance, for example, are as great as between one of them and the composition ordinarily given for albumen, which is itself but an average derived from a number of analyses differing considerably from each other.

When blood-fibrin is kept in water in a closed vessel, and at a moderate temperature, decomposition soon commences in it; at the end of three weeks it is completely dissolved, and the liquor, which includes some flocculi of sulphuret of iron, possesses all the characters of an albuminous solution, and forms a coagulum by heat, which has the precise composition of albumen.— *Annalen der Chemie und Pharmacie.*

[These researches are extremely suggestive, and open up the question whether the substance of muscle has that close relationship to the fibrin of the blood which has been commonly assigned to it. For ourselves, we are now disposed to believe that the fibrin of the blood is a stage of transition towards gelatin, and that its purpose is the nutrition of the simple fibrous tissues alone.]

SELECTED MATTER.

ANATOMY AND PHYSIOLOGY.

ON THE PHYSIOLOGICAL EFFECTS OF THE COLCHICUM AUTUMNALE.

By Dr. J. M. Maclagan, Edinburgh.

One of the most remarkable effects of colchicum was discovered by Chelius of Heidelberg. He found that the uric acid contained in the urine of those taking colchicum was nearly doubled in the space of twelve days. In one case the urine before taking colchicum contained 0·069 per mille of uric; four days after commencing to take the colchicum the proportion was 0·076; on the eighth day, it was 0·091; and on the twelfth, it was 0·102. Chelius obtained the same results in other instances.

Dr. Christison examined the urine of a patient taking colchicum, and he found that in two days the quantity of urea was nearly doubled. In the urine before taking colchium there was no deposit of *lithate of ammonia*. Its density 1020. It contained about forty-seven parts of solid matters in the thousand, and of this quantity twenty parts were urea. The specimens of urine passed on the first and second days after commencing to take colchicum were exactly alike. They were very turbid, and their turbidity disappeared with a gentle heat; the deposit was evidently *lithate of ammonia*. The density of the first was 1033·5, and that of the second was 1034, which are both very unusually high for urine not *diabetic*. As they were obviously identical in their nature, Dr. Christison only analysed the second. It contained only seventy-nine parts of solid matters in a thousand; and of this quantity thirty-five were urea. Dr. Christison suspected that the quantity of urea was even greater, for not having added an excess of *nitric acid*, some of the *nitrate of urea* might have remained in solution.

Through the kindness of Dr. Halliday Douglas, I had an opportunity of examining the effect of colchicum on the urine of a sailor, who was a patient in the Royal Infirmary. He was under treatment for secondary syphilis, but was otherwise healthy. I was permitted to give him a few doses of colchicum, in order that I might ascertain the physiological action of that agent on the kidneys, but before doing so I examined his urine. The density was 1025. It contained no deposit, nor was it affected by *heat* or *nitric acid*. It contained:

Total solids,	27·500
Water,	972·500
Urea,	12·360
Uric Acid	0·281
Inorganic salts,	7·436
Organic matter,	7·423
Total,	1000·000

Here it will be perceived that both the urea and uric acid were slightly

deficient, if we compare it with the standard of healthy urine, as given by Becquerel. Density 1018·9. Contains:—

Total solids,	31·185
Water,	968·815
Urea,	13·838
Uric Acid,	0·391
Inorganic salts,	7·965
Organic matter,	0·261
Total,	1000·000

On the third, after commencing to take colchicum, the urine was examined. It possessed a slight turbidity, which, however, was dissipated by heat. Density 1030. It contained:—

Total solids,	29·650
Water,	970·350
Urea,	15·500
Uric Acid,	0·491
Inorganic salts,	6·350
Organic matter,	7·209
Total,	1000·000

Here, it will be observed, the urea was increased by *one-fourth*, the uric acid nearly *doubled*, and the inorganic salts and inseparable organic matters were considerably decreased.

The urine was again examined on the sixth day after commencing to take the colchicum, with the following results. Turbidity rather increased. Density 1034. It contained:—

Total solids,	33·460
Water,	956·540
Urea,	18·341
Uric Acid,	0·750
Inorganic salts,	7·436
Organic matter,	6·833
Total,	1000·000

Here, then, the physiological action of colchicum in increasing the urea and uric acid was well marked.

Having obtained these results from this case (which are only corroborations of many others,) it was not considered justifiable to proceed further with the administration of colchicum with this patient.

It has been supposed that under the use of colchicum a remarkable change takes place in the system,—namely, that the uric acid becomes converted into urea; but this has not at all been substantiated, and from the above cases of Chelius, and the analysis which I have just noticed, we must be led to suppose that no such change occurs, but that an increase in both these principles is the result.

Dr. Graves states, that the beneficial action of colchicum is not owing to its producing a more rapid excretion of *lithates* through the kidneys, but to the remarkable property the plant possesses of altogether putting a stop to the morbid formation of *lithates*.

Dr. Gairdner says, that he has always found that the increase of urea was accompanied by a corresponding diminution of the *urates* in the urine. But, from the above experiments I am inclined to believe that both of these suppositions are erroneous.—*Monthly Journal of Medical Science*, Dec. 1851.

SOURCES AND MODE OF PRODUCTION OF HEAT IN THE BODY.

To explain the production of heat in the body, several theories have been advanced; but it now appears almost certain that the correct one is that which refers the generation of heat, primarily and in general, to certain chemical processes going on in the system; but admits, at the same time, that as these chemical changes are carried on in parts whose functions are, to a certain extent, under the influence of the nervous system, therefore the production of heat is liable to be modified, either locally or in every part, by the operation of that system.

In explaining the chemical changes effected in the process of respiration, (p. 139), it was stated that the oxygen of the atmosphere taken into the blood is, most probably, combined in the systemic capillary vessels the carbon and the hydrogen of disintegrated and absorbed tissues, and with certain elements of food which have not been converted into tissues. That such a combination, between the oxygen of the atmosphere and the carbon and hydrogen in the blood, is continually taking place, is made nearly certain by the fact, that a larger amount of carbon and hydrogen is constantly being added to the blood from the food than is required for the ordinary purposes of nutrition, and that a quantity of oxygen is also constantly being absorbed from the air in the lungs, of the disposal of which no account can be given except by regarding it as combining, for the most part, with the excess of carbon and hydrogen, and being evaporated in the form of carbonic acid and water. In other words, the blood of warm-blooded animals appears to be always receiving from the digestive canal and the lungs more carbon, hydrogen, and oxygen, than are consumed in the repair of the tissues; and to be always emitting carbonic acid and water, for which no other source can be ascribed that the combination of these elements. In the processes of such combination, heat must be continually produced in the animal body. The same amount of heat will be evolved in the union of any given quantities of carbon and oxygen, and of hydrogen and oxygen, whether the combination be rapid and evident, as in ordinary combustion, or slow and imperceptible as in the changes which are believed to occur in the living body. And since the heat thus arising will be generated wherever the blood is carried, every part of the body will be heated equally; or so nearly equally that the rapid circulation of the blood will quickly remove any adversities of temperature in different parts.

To establish this theory it needs to be shown that the quantity of carbon and hydrogen which, in a given time, unites in the body with oxygen, is sufficient to account for the amount of heat generated in the animal within the same time: an amount capable of maintaining the temperature of the body at from 98° to 100°, notwithstanding a large loss by radiation and evaporation.*

An attempt to determine this point was made by Dulong and Despretz. Dulong introduced different mammiferous animals, carnivorous as well as herbivorous, into a receiver, in which the changes produced in the air by respiration, and the volume of the different products, could be determined at the same time that the amount of heat lost by the animal could be ascertained. His experiments led him to conclude, among other points, that supposing all the oxygen, absorbed into the blood from the air in the lungs, were combined with carbon and hydrogen in the system, and that as much heat were thus generated as would be developed during the quick combustion of equal quantities of oxygen and carbon, and of oxygen and hydrogen, still, the whole quantity of heat produced would amount to only from $\frac{3}{4}$ to $\frac{4}{5}$ of that which is developed during the same space of time by carnivorous as well as herbivorous animals. Despretz placed animals in a vessel surrounded with water; an uninterrupted current of air to and from the vessel was maintained, and the volume and

* Some heat will also be generated in the combination of sulphur and phosphoros with oxygen, to which reference has been made (p. 136): but the amount thus produced has not been estimated, and need not be considered in the exposition of a theory which can, at present, be stated in only the most general terms.

composition of the air employed were ascertained both before and after the experiment (which was continued 1½ or 2 hours,) as well as the increase in the temperature of the surrounding water during it: by this means it was found that the heat which should have been generated, according to the chemical theory of respiration, would account for from 0·76 to 0·91 only of that which the animals really gave out during the same time. The failure of these experiments to account for all the heat produced threw doubts on the chemical theory of animal heat (as the proposed explanation has been called), till Leibig lately showed that Dulong and Despretz were in error in their conclusions, from having formed too low an estimate of the heat produced in the combustion of carbon and hydrogen. On repeating their experiments, and using the more accurate numbers to represent these *combustion-heats*, Leibig finds reason to believe that the quantity of heat which would be generated, by the union of oxygen absorbed into the blood from the atmosphere with the carbon and hyrogen taken into the system as food, is sufficient to account for the whole of the caloric formed in the animal body.

Many things observed in the economy and habits of animals are explicable by this theory, and are, therefore, evidence for its truth. Thus, as a general rule, in the various classes of animals, as well as individual examples of each class, the quantity of heat generated in the body is in direct proportion to the activity of the respiratory process. The highest animal temperature, for example, is found in birds, in whom the function of respiration is most actively performed. In Mammalia, the process of respiration is less active, and the average temperature of the body less, than in birds. In reptiles, both the respiration and the heat are at a much lower standard; whilst in animals below them, in which the function of respiration is at the lowest point, a power of producing heat is, in ordinary circumstances, hardly discernable. Among these lower animals, however, the observation of Mr. Newport (xliii. 1837) supply confirmatory evidence. He shows that the larva, in which the respiratory organs are smaller in comparison with the size of the body, has a lower temperature than the perfect insect. Volant insects have the highest temperature, and they have always the largest respiratory organs and breathe the greatest quantity of air: while among terrestrial insects, those also produce the most heat which have the largest respiratory organs and breathe the most air. During sleep, hybernation, and other states of inaction, respiration is slower or suspended, and the temperature is proportionally diminished; while on the other hand, while the insect is most active and respiring most voluminously, its amount of temperature is at its maximum, and corresponds with the quantity of respiration. Neither the rapidity of the circulation nor the size of the nervous system, according to Mr. Newport, presents such a constant relation to the evolution of heat.

Similar evidence in favour of this theory of animal heat is furnished by the fact that heat is sometimes evolved by plants, in a quantity which appears to be in direct proportion to the amount of oxygen they at the same time absorb and convert into carbonic acid. For example, their evolution of heat is most evident during flowering and the germination of seeds, the times at which the largest amount of carbonic acid is exhaled.

The quantity and quality of food consumed by man and animals in the different climates and seasons, also appears to be adapted to the production of various amounts of heat by the combination of carbon and hydrogen with oxygen. In northern regions, for example, and in the colder seasons of more southern climes, the quantity of food consumed is (speaking very generally) greater than is consumed by the same men or animals in opposite conditions of climate and season. And the food which appears naturally adapted to the inhabitants of the coldest climates, such as the several fatty and oily substances, abounds in carbon and hydrogen, and is fitted to combine with the large quantities of oxygen which, breathing cold dense air, they absorb from their lungs.

The *influence of the nervous system* in modifying the production of heat has been already referred to. The experiments and observations which best illustrate it are those showing first, that when the supply of nervous influence to a part is cut off the temperature of that part falls below its ordinary degree; and, secondly, that when death is caused by severe injury to or removal of the nervous centres, the temperature of the body rapidly falls, even though artificial respiration be performed, the circulation maintained, and to all appearance the ordinary chemical changes of the body be completely effected. It has been repeatedly noticed that, after division of the nerves of a limb, its temperature falls: and thus diminution of heat has been remarked still more plainly in limbs deprived of nervous influence by paralysis. For example Mr. Earle (xli. vol. vii. p. 173) found the temperature of the hand of a paralysed arm to be 70° while that of the sound side had a temperature of 92° F. On electrifying the paralysed limb, the temperature rose to 77°. In another case, the temperature of the paralysed finger was 56° F., while that of the unaffected hand was 62°. Sir B. C. Brodie (x in 1811 and 1812) found, that if artificial respiration was kept up in animals, killed by decapitation, division of the medulla oblongata, destruction of the brain, or poisoning with Woorara poison, the action of the heart continued, and the blood underwent the usual changes in the lungs, as shown by the analysis of the air respired, but that the heat of the body was not maintained: on the contrary, being cooled by the air forced into the lungs, it became cold more rapidly than the body of an animal in which artificial respiration was not kept up. With equal certainty, though less definitely, the influence of the nervous system on the production of heat is shown in the rapid and momentary increase of temperature, sometimes general, at other times quite local, which is observed in states of nervous excitement; in the general increase of warmth of the body, sometimes amounting to perspiration, which is excited by passions of the mind; in the sudden rush of heat to the face, which is not a mere sensation; and in the equally rapid diminution of temperature in the depressing passions. But none of these instances suffices to prove that heat is generated by mere nervous action, independent of any chemical change; all are as well explicable on the supposition that the influence of the nervous system alters, in some way, the chemical processes from which the heat is commonly generated. There are ample proofs that the nervous system, especially in the most highly organized animals, does so modify all the functions of organic life; and it appears more reasonable to suppose that it thus influences the production of heat, than to ascribe to it any more direct agency.

In the foregoing pages, the illustrations of the power of maintaining an uniform temperature have had reference to the ordinary case of man living in a medium colder than his body, and therefore losing heat both by radiation and evaporation. The losses in these two ways will bear, in general, an inverse proportion to one another; the small loss of heat in evaporation in cold climates may go far to compensate for the greater loss by radiation; as, on the other hand, the great amount of fluid evaporated in hot air may remove nearly as much heat as is commonly lost by both radiation and evaporation in ordinary temperatures. Thus, it is possible that the quantities of heat required for the maintenance of an uniform proper temperature in various climates and seasons are not so different as they may, at first thought, seem: but on these points no accurate information has been yet obtained.*

Neither, as to the maintenance of the temperature of the body in hot air is more known than that great heat can for a time be borne with little change

* Vierordt has made estimates of the heat given out, per minute, from the lungs in warming the inspired air, and in combination with the evaporated water; it would be enough to heat (at the most) 96-34 grains of water from 32 to 212° (ex. p. 236). At this rate the loss by evaporation from the skin and lungs together may be roughly estimated at enough to heat nearly 4,000 grains of water from 32 to 212°.

in proper temperature of the body, provided the air be dry. Sir Charles Blagden and others supported a temperature varying between 198° and 211° F. in dry air for several minutes; and in a subsequent experiment he remained eight minutes in a temperature of 260.° Delaroche and Berger (cxxxii.) observed that the temperature of rabits was raised only a few degrees when they were exposed to heat varying from 122° to 194.° But such heats are not tolerable when the air is moist as well as hot, so as to prevent evaporation from the body. M. C. James (xix. April, 1844) states that in the vapour baths of Nero he was almost suffocated in a temperature of 112,° while in the caves of Testaccio, in which the air is dry, he was but little discomforted by a temperature of 176.° In the former, evaporation from the skin was impossible; in the latter it was, probably, abundant, and the layer of vapour which would rise from all surface of the body would, by its very slowly conducting power defend it for a time from the full action of the external heat.

It remains to notice certain conditions by which the production of heat is modified.

The *effects of age* are noticeable. M. Edwards found the power of generating heat to be less in old people; and the same was observed by Dr. Davy (xliii. 1844), who, in eight people, between eighty-seven and ninety-five years old, found that, although the average temperature of the body was not lower than that of younger persons, yet the power of resisting cold was less in them—exposure to a low temperature causing a greater reduction of heat than in young persons.

The same rapid diminution of temperature was observed by M. Edwards in the new-born young of most carnivorous and rodent animals when they were removed from the parent, the temperature of the atmosphere being between 50° and 53½° F.; whereas, while lying close to the body of the mother, their temperature was only 2 to 3 degrees lower than hers. The same law applies to the young birds. Young sparrows, a week after they were hatched, had a temperature of 95° 97,° while in the nest; but when taken from it, their temperature fell in one hour to 66½,° the temperature of the atmosphere being at the time 62½.° It appears from his investigations that, in respect of the power of generating heat, some Mammalia are born in a less developed condition than others: and that the young of dogs, cats, and rabits, for example, are inferior to the young of those animals which are not born blind. The need of external warmth to keep up the temperature of new-born children is well known; the researches of M. Edwards show that the want of it is, as Hunter suggested, a much more frequent cause of death in new-born children than is generally supposed, and furnish a strong argument against the idea that children, by early exposure to cold, can soon be hardened into resisting its injurious influence.

Active exercise, as already stated, raises the temperature of the body. This may be partly ascribed to the fact that every muscular contraction is attended by the development of one or two degrees of heat in the acting muscle; and that the heat is increased according to the number and rapidity of these contractions, and may be quickly diffused by the blood circulating from heated muscles. Possibly, also, some heat may be generated in the various movements, stretchings, and recoilings of the other tissues, as the arteries, whose elastic walls, alternately dilated and contracted, may give out some heat, just as caoutchouc alternately stretched and recoiling becomes hot. But the heat thus developed cannot be so much as some have supposed.

The *influence of external coverings* for the body must not be unnoticed. In warm-blooded animals they are always adapted, among many purposes, to the maintenance of uniform temperature; and man adapts for himself such as are, for the same purpose, fitted to the various climates to which he is exposed by their means, and by his command over food and fire, perhaps as much as by his capacity of developing appropriate amount of heat, he maintains his temperature on all accessible parts of the surface of the earth.

MEDICINE.

*ON APOPLEXY AND EPILEPSY; AND ON AN HOSPITAL FOR EPILEPTICS.

By Marshall Hall, M.D., F.R.S.

I propose tracheotomy, not as a remedy for apolexy, or for epilepsy; but for stertor or paralytic Laryngismus and its effects, in the former malady; and as a preventive and security against spasmodic Laryngismus and its effects, viz., Convulsion, and the injury apt to be inflicted on the cerebrum and the medulla oblongata, on the mind and limbs, in the latter dire calamity.

In the apoplexia gravior, with laryngeal stertor, tracheotomy affords the chance for life; in the epilepsia gravior, tracheotomy supersedes the laryngismus, and the Convulsion and its dire effects.

LECTURE I.

1—In the Croonian Lectures for 1850, I gave an outline of *The Diastaltic Nervous System;* in those for 1851, I gave a view of the application of that system to the Pathology of a *Class of Diseases of the Nervous Centres of inorganic Origin;* I propose, on the present occasion, to treat of a *branch* of this Pathology, in its turn, and especially to detail the progress made in its investigation during the present year.

2—The result of a sustained and almost exclusive attention to this subject, during a very considerable period, on my mind, is the conviction that those forms of disease of the nervous centres which are of *inorganic origin*, that is, by far the greater number of these affections, arise from causes acting on the structures of *the Neck*, or of *the Larynx*.

3—Apoplexy and Epilepsy are the two most formidable diseases of the Nervous Centres. Each may be divided into the *milder* and the *severer* forms, which may be designated the Apoplexia mitior and the Apoplexia gravior, and the Epilepsia mitior and the Epilepsia gravior.

4—Both apoplexy and epilepsy may have their origin in organic disease of the nervous centres, or of their vascular structure, arteries, veins, or *intermediate* vessels. Both may be of inorganic origin, and the effect of one or other of the *Emotions* or *Irritations*. Of these, the former act *directly*, the latter *diastaltically*, on *the Neck*, or on *the Larynx;* or the affection of the larynx may be secondary.

5—I have ventured to designate these affections *Trachelismus*, or *Laryngismus*, respectively.

6—I am as persuaded as I can be of any medical view, that the apoplexia mitior and the epilepsia mitior depend on trachelismus; and that they might be designated apoplexia or epilepsia *trachelea*. I am persuaded that the apoplexia gravior and the epilepsia gravior depend upon laryngismus, and that they, in their turn, might be designated apoplexia or epilepsia *laryngea*.

7—Seeing the importance which I attach to these distinctions, you will not, gentlemen, be surprised that I make them the foundation of the division of the present Lectures. The *first* will, indeed, treat of *Trachelismus;* the *second*, of *Laryngismus;* with their relations, anatomical, physiological, pathological, and therapeutic.

8—But if laryngismus be the essential link between the exciting causes and the apoplexia and epilepsia gravior, it is plain that *Tracheotomy*, in

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* As there are cases of epilepsy frequently presenting themselves to the practitioner in this place we desire to furnish them with Dr. Hall's elaborate views on the disease.

superseding the *effects* of this condition, must supersede the graver forms of those diseases, converting them into the milder respectively. But an essential part of the epilepsia gravior is *general convulsion;* laryngismus then is essential to this convulsion; and as tracheotomy supersedes the effects of laryngismus, it must supersede convulsion, with the further train of dire affections, in the epilepsia gravior.

9—This is the case in effect. If we institute tracheotomy, there can be no general convulsion. The epilepsy is cut short; it presents the phenomena of the epilepsia mitior, those of the epilepsia gravior being superseded and prevented.

10—It is partly, chiefly, I may say, but not entirely, on these views that I shall venture, in my *third* lecture, to propose the institution of an *Hospital for the Epileptic.*

11—I now return to the subject of the present Lecture, or *Trachelismus*—a subject which has not hitherto become an object of medical observation, or taken its place in medical literature.

I. On Trachelismus.

12—It is impossible to contemplate the muscular and vascular structures of *the Neck,* and their relative positions, without being impressed with the great influence which the action of the former *must* have on the condition of the latter, and especially of the *veins* of this important region.

13—And it is impossible not to perceive the momentous effect which these effects of muscular action on the cervical veins must have on the *encephalon.*

14—I have long wished for the opportunity of making a most careful dissection of *the Neck,* and of those *anatomical* relations between its muscular and venous structures. This opportunity has not yet occurred with all the leisure I desire; but I trust it is only a labour postponed.

15—Meantime it is obvious that, on the contraction of the muscles of the neck, the veins of this region must be compressed. The effect of this compression is, however, very various, according as that contraction is *clonic* or *tonic,* or otherwise inordinate.

16—A single contraction of muscles on a vein must tend to empty it. If this contraction be followed by relaxation, the vein is soon refilled. And if this alternate contraction and relaxation be rapidly repeated, the circulation along such veins must be accelerated.

17—But if, instead of these clonic actions of the muscles, their contraction be tonic,—if it be spasmodic, inordinate, and sustained,—the veins are emptied, are *not* refilled, and the circulation at their *origins* become impeded or utterly interrupted, or even retrograde.

18—Let us imagine the effect of such an event on the veins and on the blood-channels intermediate between these and the arteries, in the delicate substance of the cerebrum and medulla oblongata!—the congestion, the ecchymosis, the rupture,—the danger,—to which these must be subjected!

19—We readily comprehend how these effects may manifest themselves. If this interrupted flow of the venous blood obtain in the exterior tissues of the head, we shall observe redness, purpurescence, intumescence, according to its degree: we see flushing, lividity, and fulness of the face and of the neck. If the interrupted flow occur within the encephalon, we observe *symptoms* of affection of the nervous centres, these symptoms assuming the varied apoplectic or epileptic character, according to the nervous centre specially affected.

20—To me it is wonderful that a heavy burden can be poised on the head without interrupted flow of venous blood, so great as to induce cerebral symptoms. The fact can only be explained by the circumstances that mere poise does not imply either great, or constant, or inordinate action of the muscles. With every movement that is made, every step that is taken, a new order of muscular actions takes place, and, with this, an accelerated flow of venous blood.

21—Very different is the event in certain cases of inordinate action of the

muscles of the neck. The late Professor Gregory used to mention the case of a man, who, being in a boat, suddenly turned his head so as to look backwards, and fell down apoplectic. I have two patients, subject to vertigo, who cannot move the head rapidly to the right and left, without experiencing this symptom: although the same persons can turn the head and trunk together with equal rapidity, without experiencing that effect: yet the *difference* is only—the action of the muscles of the neck—or *trachelismus*. I have an epileptic patient who cannot turn the head extremely to the right, without experiencing a strange feeling of vertigo and confusion, and the threatening of a seizure. A similar action, turning the head to the left, produces no such effect. The phenomenon is obviously owing to the forcible contraction of *certain* muscles, compressing *certain* veins: the action is *special*. The patient being epileptic, these muscles and these veins may be those especially implicated in the paroxisms.

22—That the tonic contraction of the muscles of the neck really induces symptoms of affection of the nervous centres, is further proved by the following fact, for which I am indebted to Mr. Reynolds, formerly a pupil at University College:—

23—"A girl, nineteen years of age, was admitted into University College Hospital for aphonia: and, amongst other things in the treatment, she was ordered to have galvanism applied to the larynx daily, by the electro-magnetic machine.

24—"While using this machine, I observed the effect upon the muscles of the neck, and remarked that, when the wheel was turned slowly, and the superficial muscles were alternately contracted and relaxed, *the colour of the face was heightened*, and of a florid hue, and no unpleasant feelings (further than those arising from the shocks) were experienced; but when the wheel was turned rapidly, with a less powerful current, and the muscles were maintained, during the rapidly intermitting action, in a state of almost permanent contraction, *the face became of a deep colour the lips and angles of the mouth livid, the eyes suffused*, and some feelings of confusion of thought, headache, and dimness of sight, alternating with flashing of light, were induced. The latter effects remained after the cessation of the current, for a few minutes, and then disappeared."

25—In these facts we have the *Proof* that a slight degree of contraction of the muscles of the neck, induced by the electric current, induces, in its turn, heightened colour of the face, of a florid hue; and that a greater degree of that contraction induces a deeper colour of the face, the lips and angles of the mouth being livid, and the eyes suffused, with confusion of thought, headache, dimness of sight, alternating with flashes of light; these latter remaining for a few minutes after the cessation of the current, and then disappearing. They present the *Demonstration* of the nature of trachelismus, and of its *effects*.

26—The usual causes of these contractions of the muscles of the neck, and the consequent impeded flow of blood along its veins, are—

1—*The Emotions*, and
2—*The Irritations*;

and especially fright, indignation, anger,—excitement, pleasurable or painful,—amongst the former; and the gastric, the enteric, the hysteric, amongst the latter. These causes induce tonic and mordinate action of the muscles of the neck, of which the patient is frequently quite conscious, and which he describes as "strings," a sense of "constriction," &c.; or he experiences "choking fits," with fullness of the face," as was the case with a physician whose career had been one of the most remarkable of the present day.

27—The most constant of these feelings of trachelismus is that of the cravat being too tight, though it be really not so, the patient constantly or repeatedly endeavouring to loosen it, by drawing it forwards with the fingers.

28—There is no medical fact more familiar to us than that of the occurrence of seizures from the various emotions and irritations. The new and important

question occurs—*How do these causes act in producing these effects?*—the answer to which gives—*the true pathology of the forms of apoplexy and of epilepsy of inorganic origin.*

29—It is plain that they might act by augmenting the action of the heart and accelerating the circulation. But there is no known physiological principle by which such an effort can be supposed to implicate the nervous centres especially. If the circulation be thus accelerated, it is accelerated in every part of the system, at an equal distance from the heart, *equally*. The nervous centres may share in the general result. But they cannot be affected particularly.

30—The same observations apply to other forms and modes of accelerated and augmented circulation. Violent running induces no symptoms portending a seizure.

31—It is, in brief, not accelerated and augmented flow of blood *to* the nervous centres, but impeded flow of blood *from* those centres, which endangers their function and structure, especially in the predisposed. An epileptic, whose case was detailed to me by Mr. George Webster, seeing some men endeavouring in vain to move a barrel, said—"Let me try,"—made a violent effort, and fell into an epileptic paroxysm!

33—But emotion or irritation may act on the muscles of the neck only, without involving the larynx, and thus it is that we see the isolated and distinct effect of trachelismus.

34—It is in this manner that shame and anger induce blushing and flushing, respectively. It is in this manner that I have traced the former into epilepsy, and that the latter, as is well-known, is apt to pass into apoplexy or epilepsy.

35—It is not less known that an indigestible meal or a loaded colon equally induce apoplectic or epileptic threatenings or seizures.

36—The emotions act through the nerves on the muscles of the neck *directly*. The irritations act on the same muscles *disastically*.

37—The muscular actions thus induced are not the *well-balanced* actions of voluntary motion, but *inordinate* action, both in the combination of the muscles involved and in the tonic form of that action. Hence the sustained compression of the veins, the *appearances* of flushing and fulness of the face and neck, the *symptoms* of appoplectic or epileptic seizure, &c.

38—I have already, in the Croonian Lectures of last year, presented the argument of the relation of trachelismus to the *Class* of diseases of the nervous centres of inorganic origin and of paroxysmal form. It is my present object to trace the relation of *trachelismus* more especially to *one form and degree* of these maladies exclusively.

39—It is not to apoplexy and epilepsy, but to the apoplexia *mitior* and the epilepsia *mitior*, that trachelismus, when uncomplicated with laryngismus, has its special relation.

40—As long as the affection is limited to the *muscles* and *veins* of the neck, so long the affection assumes the form of the apoplexia *mitior* and the epilepsia *mitior*, but especially the latter; when the larynx becomes implicated, the *severe* forms of this disease are induced, or superinduced.

41—In apoplexia this condition of the muscles and veins of the neck is more continued; in epilepsia it is more forcible, but less continued; indeed, it is frequently quite transitory. In both, however, the patient *may* become affected with vertigo and confusion, and even *fall*, and recover immediately.

42—In this state of the question it is interesting to inquire whether other facts and phenomena are observed which may throw a ray of light on this important subject; and I am happy to be able to illustrate my views by a reference to various facts of this kind.

43—One of these is, the effect of a tight cravat applied round the neck; and in this place I must borrow several deeply interesting facts from my former Lectures.

41—It was observed by Dr. Donald Monroe that soldiers were liable to be "carried off by apoplexy, in consequence of stricture of the veins of the neck, from being obliged to wear their cravats too tight."

45—Abercrombie quotes a case from Zitzilius, of "a boy who had drawn his neckcloth remarkably tight, and was whipping his top, stooping and rising alternately, when, after a short time, he fell down apoplectic. The neckcloth being unloosed, and blood drawn from the jugular veins, he speedily recovered."

46—The following case occurred in the person of a most intelligent member of our profession. I give it in his own words:—

47—"A few weeks ago, my shirt-collar was made too tight, and felt rather uncomfortable, yet not so much as to induce me to change or slacken it. On looking into the mouth of a patient, in such a position as to twist my neck a little, I dropped down in my surgery as if I had been shot, in a moment, as helpless as a dead man. I soon got up, my head was giddy for some time. I changed my shirt, and lost all fear of return of the accident. There can be no doubt that it arose from compression of the veins."

48—The influence of a tight collar or cravat is not duly appreciated. It may be slight in a state of repose; but on moving the head variously, the muscles of the neck expand. This expansion cannot take place *outwardly*, it therefore takes place *inwardly*, and so compresses the subjacent veins. It is on this principle, not, I think, generally acknowledged that a moderately tight cravat may prove an unsuspected source of danger. Under the influence of such a cravat or collar, the not unusual actions of the muscles of the neck become a sort of trachelismus, perhaps more frequently than is imagined. The cravat, too, which is not tight generally, may become so under the influence of sleep, of emotion, of gastric repletion, or of certain positions.

49—A further illustration of the same subject is afforded by the fearful events recorded in the history of Thuggee;—a crime which is perpetrated by the application of a ligature round the neck of the victim, inducing instant apoplectic insensibility.

Illustrations of the same fearful kind are afforded by every kind of *strangulation;* the first effect of which is instant apoplectic insensibility; the second, the epilepsia gravior: the third asphyxia; in a series of fearful interest.

51—Similar confirmations of these views have been presented by experiments on animals:

52—One of the most interesting of these was performed, at my request, by Mr. Martin Coates, of Salisbury; and is described in my former lectures.

53—The subject has been recently taken up by Dr. Wegg, who is engaged in a most interesting series of experiments in illustration in the pathology of affections of the nervous centres. One of them I will briefly detail:

54—A leather strap was applied rather tightly round the neck of a little dog: from being lively and playful, the animal immediately became dull, turned round apparently to select a position, and went to *sleep!*

55—If the collar were drawn still more tightly, the larynx became implicated with the most extraordinary phenomenon of the formation of abundance of epileptic *flan.* But I must not further anticipate Dr. Wegg's account of his interesting investigation.

56—In order to produce the appearances and the symptoms of trachelismus, the action of the muscles must be, like that of the collar, *continuous.*

57—I find that, on applying an accurate measure round the neck—the sterhometer of Dr. R. Quain, for example,—and turning the head extremely to one side, the circumference of the neck is readily augmented by half an inch. This effect is produced by the bulging of the contracted muscles. This bulging must take place equally towards the centre of the neck, and must encroach on the contents of the vessels, especially the veins, of that regions. Hence, when the contraction is tonic and considerable, impeded flow of the blood from the head, and *threatening* of apoplexy or epilepsy.

58—The same augmentation of the circumference of the neck induced by closing the larynx and making an *effort*, the flow of blood being impeded and the veins becoming distended. Hence, when these events occur in an extreme degree, extreme apoplexy or epilepsy; and hence, the relief from tracheotomy.

59—It is not in voluntary action, but in action the effect of emotion or irritation, that is, abnormal, inordinate, and spasmodic action, that these effects are observed.

60—It is also in the action of *certain* muscles, especially, I believe, the omo-hyoid, that impeded venous circulation, with its effects, is induced. I have repeatedly, both in hysteria and in epilepsy, *felt* the clonic action of this muscle under the finger; in apoplectic affections, its action being tonic, would be less detectible.

61—In one case of spasmotic affection, clonic contractions were observed, both of the omo-hyoid and of the cleido-mastoid; of the former *as distinct* as of the latter.

62—The patient complained exceedingly of a sense of choking (the effect of the contraction of the omo-hyoid?) and of pain in the posterior part of the neck (the effect of contraction of the muscles of that region?)

63—We have thus the tangible contraction of the omo-hyoid. But is not contraction of this muscle frequently the cause of the sense of a "*cord*," of a "*spike*," and of "*choking*," so variously felt in paroxysmal threatenings!

On Trachelismus in Relation to the Apoplexia Mitior.

64—I am daily consulted by persons who have experienced attacks of slight apoplectic affection; and I have daily the opportunity of tracing the apoplexia gravior to *antecedent*, but neglected seizures of the same kind.

65—Vertigo, transient confusion of mind, or oblivium, nutation, falling,—are the symptoms which denote this affection. They are frequently attended by flushing of the face, a sense of tightness or constriction about the neck, the patient frequently endeavouring to loosen a cravat, already perfectly loose. In one case there were "choking fits," with "fulness" of the face; once, falling to the ground; once, transient hemiplegia. In another case, similar symptoms were followed by severe apoplexy and lasting hemiplegia.

66—In all these cases, so long as their is no stertor, the seizure may be regarded as the apoplexia mitior; and I think the prognosis, in reference to *life*, favourable.

On Trachelismus in Relation to the Epilepsia Mitior.

67—But the distinction between the epilepsia mitior and the epilepsia gravior is still more marked.

68—Every kind of *spasmodic* affection may occur, as strabismus, distortion of the features, clonic actions of the limbs, &c., with vertigo, oblivium, nutatio, falling even; but if there be no laryngismus, there is no *general convulsion*, no *convulsive* dashing to the ground, and, in a word, no epilepsia *gravior*.

69—Some patients experience the epilepsia mitior, the "petit mal" of the French writers, only. But no fact is so common as that of the same patient being subject to both the milder and the severer forms of epileptic seizure. In every case, laryngismus constitutes the fearful boundary which separates the epilepsia mitior from the epilepsia gravior.

70—In the former, the patient may *fall;* and this fall may be such as to lead to injury. But the state of unconsciousness, unlike that in the epilepsia gravior, is usually but for a moment, and the patient recovers and rises. There may be said, indeed, to be two forms of "*falling sickness*"—one being the result of unconsciousness, the other of convulsion. The former may occur in the apoplexia or epilepsia mitior; the latter occurs in the epilepsia gravior only, and is the truly formidable malady.

71—The worst event, with regard to the mere falling, is that which occurs

upon the stairs, or in a crowded street. These events I have known to occur. I have not known such an attack to lead to falling with violence, or into the fire, or into the water.

72—My object is to state the *truth*, however; and I do not attempt, for the sake of an opinion, or an object, however desirable, to give a colouring to facts which does not belong to them.

To be continued.

A FEW HINTS IN TREATING URINARY AFFECTIONS.

By Dr. Golding Bird, F.R.S., &c.

1—Dr. Bird remarks that, Whenever it is desirable to impregnate the urine with a salt, or to excite diuresis by a saline combination, it must be exhibited in solution, so diluted as to contain less than 5 per cent of the remedy, or not more than twenty-five grains in an ordinary draught. The absorption of the drug into the capillaries will be ensured by a copious draught of water, or any diluent, immediately after each dose.

2—When the urine contains purpurine, or presents other evidence of portal obstruction, the diuretics or other remedies employed should be preceded or accompanied by the administration of mild mercurials—taraxacum, hydrochlorate of ammonia, or other cholitic remedies. By these means, or by local depletion, especially by leeches to the anus, the portal vessels will be unloaded, and a free passage obtained to the general circulation.

3—In cases of valvular disease, or other obstructions existing in the heart and large vessels, it is next to useless to endeavour to excite diuretic action, or appeal to the kidneys by remedies intended to be excreted by them. The best diuretic will, in such cases, be found in whatever tends to diminish the congested state of the vascular system, and to moderate the action of the heart; as digitalis, colchicum, and other sedatives, with mild mercurials.—*British and Foreign Medico-Chirurg. Review, July*, 1851.

ON THE MEANS OF TESTING FOR UREA IN ALBUMINOUS FLUIDS; AND ON THE ORIGIN OF UREA.

By Professor Lehmann.

Urea may generally be very easily recognized by its properties, especially by its behaviour towards nitric and oxalic acids; but when we have to discover very minute quantities of this substance in albuminous fluids, it is often very difficult to determine its presence with scientific precision. It is in alcoholic extracts that we must always seek for urea: but before we proceed to search for it, there are several precautionary measures to be adopted, the neglect of which would render our attempt to discover it futile. In the first place, in reference to the presence of albuminous substances, if we wish to discover small quantities of urea in albuminous fluids, we must not be satisfied with the removal of the albumen by simple boiling: since, by the coagulation of the albumen, the fluid becomes more alkaline, and might, during evaporation, induce a decomposition of the urea; moreover, all albuminous matter is not precipitated by boiling, but a portion remains dissolved by the alkali, and is taken up in the alcoholic extract. On evaporation, this albumen undergoes a change, which probably co-operates with the alkali in inducing the decomposition of the urea. This may explain how it was that Marchand could only recover 0·2 of a grammie of urea from a mixture of 300 grammes of serum and 1 grammie

of urea. Hence, before boiling the albuminous fluid, we must add a few drops of acetic acid, so as to give it a slightly acid reaction, whereby not only is the alkalescence of the fluid prevented, but a much more perfect separation of the coagulable matters is effected. If the residue of the fluid from which the coagulated matters have been filtered be extracted with cold alcohol, and the solution rapidly evaporated, so as to cause the chloride of sodium (taken up by the cold alcohol) to separate as much as possible in crystals, on then bringing a drop of the mother-liquid in contact with the nitric of the rhombic octohedra, and the hexagonal tablets, in which, if the investigation is to be unquestionable, the acute angles (=82°) must be always measured. After the determination of the nitrate, we may also obtain the oxalate, and submit it to microscopic examination. A good crystallometric determination yields, however, the same certainty as an elementary analysis, which, in these cases, would never, or extremely seldom, be possible.

The investigations of Marchand have thrown much light upon this subject [the seat of the actual formation of urea]. This accurate observer could only recover 0·2 of a gramme of urea from 308 grammes of serum, to which 1 gramme of urea had been added. He shows that, even if the urea were only separated from the blood at the end of each successive hour, it could not have accumulated in such quantity as to have been discoverable by the present mode of investigation. The following consideration will give us an idea of the small quantity of urea which, according to Marchand's hypothesis, at the most can accumulate in the blood in one hour:—From the experiments of Edward Weber, which I have in part confirmed, we may assume that there are, in an adult man, at most 6 or 7 kilogrammes [16 to 19 pounds] of circulating blood. Now, if, in twenty-four hours, 30 grammes of urea are discharged, at most only 1·25 grammes could accumulate in one hour in the whole mass of blood; so that only 0·021 could be contained in it. This minute quantity can, however, as we have already shown, only be detected in operating on very large masses of blood, and by the aid of the microscope. Hence it is easy to understand why, during my experiments with an animal diet, while the urine was loaded with urea, none of this substance could be discovered in the blood.

If it be now established that the urine is not primarily formed in the kidneys, the question still remains to be answered, whether it is produced in the circulating blood or in the individual living organs (as, for instance, the muscles), and from what materials it is principally formed. In the present state of our knowledge, we may answer, that the urea is formed in the blood, and that it is produced from materials that have become effete, the detritus of tissues, as well as from unserviceable and superfluous nitrogenous substances in the blood. No animal tissue presents such vital activity, is so much used, and is so rapidly worn out, as muscular tissue; it is in this tissue that the metamorphosis of matter proceeds most rapidly and abundantly; and yet in the large quantities of muscular fluid on which Liebig worked, he could detect no trace of urea, although he found substances from which he could produce urea artificially. We must, therefore, assume, that these substances, as creatine and probably inosmic acid, are decomposed in the blood, by the action of the alkalies and of free oxygen, into urea and other matters to be excreted. Moreover, my experiments, showing that the superfluous nitrogenous food which enters the blood, and the fact that caffeine, glysine (Horsford), uric acid, and alloxantin (Wohler and Frerichs), soon after they have been taken, perceptibly increase the amount of urea in the urine, support the view that urea is formed in the blood. It is impossible to suppose that this nitrogenous food is first converted into tissue, and subsequently into urea, &c., for we cannot think that a process occurs here, analogous to that exhibited by the percussion apparatus of the physicists, where a certain number of parts effecting a percussion give rise to the repulsion of an equal number of parts. Hence the conversion of this matter can occur in no other place than in the circulating blood, and, therefore, it is here that the urea must be formed.

That the urea is formed from nitrogenous matter could not be doubted, even if it did not contain nitrogen (and that in so large a quantity); for it is especially after the use of highly nitrogenous food, that we find an augmentation of its quantity in the urine. If, however, we should further inquire,—from what substances it is produced, and what tissues principally contribute to its formation?—we could not, in the present state of our knowledge, give any satisfactory answer to this question. All that we know is, that urea is a very general product of the decomposition of nitrogenous matter, both naturally within the animal body, and artificially in the laboratory of the chemist. We have already said enough to show that urea is so common a product of the decomposition of nitrogenous bodies, that we could hardly any longer enumerate it among true organic substances, if we tried to establish a distinction between organic and inorganic matter. Moreover, when we treat of uric acid, we shall show, that, in all probability, a great part of the urea separated by the kidneys from the blood is the product of the decomposition of that acid.—*British and Foreign Medico-Chirurg. Review, July,* 1851.

SURGERY.

AN ACCOUNT OF TWO CASES OF INTESTINAL OBSTRUCTION, IN WHICH THE OPERATION FOR THE FORMATION OF AN ARTIFICIAL ANUS WAS PERFORMED: ONE IN THE ASCENDING, THE OTHER IN THE DESCENDING COLON.

By W. J. Clement, Esq., F.R.C.S., Shrewsbury.

Case 1.—The author visited, on the 8th of October, 1841, a married woman, aged forty-seven years, who was suffering from obstruction of the bowels of fourteen days' duration, accompanied by great distention of the abdomen, hiccough, incessant vomiting, which during the last two days had become fæcal; the countenance was anxious; the pulse small, rapid, and fluttering. It appeared that for the previous seven or eight years the patient had suffered from habitual constipation, and had required the constant use of drastic purgatives. The abdomen was tympanitic on percussion everywhere except on the right inguinal and iliac regions, where it was dull. It was evident from the fact that several pints of fluid could be injected into the colon, that the obstruction was not in its descending portion. On the 10th, the symptoms having undergone no abatement, and the patient's state being evidently hopeless unless relief could be obtained by operation, it was proposed by the author, and performed on the same day. The patient was placed on her belly; the incision was made midway between the last rib and the crest of the ilium, extending from close to the spinal column to a line cutting the anterior superior spinous process of the ilium perpendicularly. The colon was found to be distended. It was secured by a couple of ligatures passed through its coats; and a vertical incision being made into it, a large quantity of liquid fæces escaped, together with much flatus. Immediate relief was obtained; the unfavourable symptoms ceased; the fæces were passed more or less freely through the wound, and at the end of six weeks the patient was able to walk a mile. About this time the discharge through the artificial anus became gradually less; at the end of seven weeks vomiting and colickly pains returned, but ceased after the expulsion of a mass of plum-stones, when a free exit for the fæces was again established. The patient lived for more than three years after the operation, enjoying tolerable health, and able to walk a considerable distance, and to attend to her domestic affairs. Aperient medicines were taken regularly, and the passage of fæces was pretty free. Plum-stones were passed at intervals;

the total number collected was 116. The patient's health declined for some months before her death, the appetite decreasing, the strength failing, and emaciation progressing. On examination a very complete stricture was found to exist in the transverse colon, which would not admit even the passage of a bristle. It was about a quarter of an inch in length. The coats of the bowel at this point were of a dense, white, cartilaginous structure. The muscular coat of the cæcum and ascending colon was much thickened, and there was great distention of the gut behind the stricture. No traces of inflamatory action were to be found in the peritoneal cavity, with the exception of three membranous bands, which extended in a lateral direction, connecting the lower part of the ilium with the cæcum and ascending colon.

Case 2.—The driver of a mail coach, a stout muscular man, aged forty-three, consulted the author in January, 1847, suffering from constipation and external piles. In the month of March the constipation had become more obstinate, and the patient was obliged to give up his occupation. The symptoms were relieved by cupping on the loins, calomel, and other purgatives. On the 2d of April, rigors, which had occurred once during the month of March, returned, and were followed by vomiting, which continued for two or three days. Examination of the rectum showed the existence of a stricture about six inches from the anus. The attempt to pass rectum bougies of the smallest size failed. An elastic-gum urethra bougie passed the obstruction, and upon withdrawing it, liquid fæces and flatus were voided. This operation caused great constitutional disturbance, rendering blood-letting, leeches, and calomel and opium, necessary. The discharge from the bowels was very slight; the vomiting recurred frequently. On the 12th of May, three small fleshy bodies, with a little fæculent matter, were voided. No fæces passed from the rectum subsequently. During the remainder of the month of May the patient suffered greatly from hiccough, vomiting, and most troublesome tenesmus. On the 30th, the formation of an artificial anus was proposed, but declined by the patient. On the 18th June, fæculent vomiting began, and returned on the 20th, and the patient then consented to have the operation performed. Examination of the rectum with the finger had given evidence of a morbid growth within the rectum, which was increasing in bulk. The operation was performed on the 20th June. No fæculent matter having passed the rectum since the 12th of May, the abdomen was enormously distended. The incision was made on the left side, in the same direction as in the former case, but of greater extent. The bowel was secured by ligatures, and a free incision made into it, but nothing but flatus escaped. As moderate pressure over the abdomen had no effect in causing a discharge of fæces, the patient was placed in bed on his left side. The vomiting and hiccough continued; about eight hours after the operation an immense discharge of liquid fæcal matter took place, with some abatement of the symptoms. The author gives a detailed account of the patient's state during seven days following the operation, during which there was considerable constitutional irritation, with much tenderness of abdomen, and retention of urine. The catheter was passed repeatedly, but the secretion of urine was very scanty, the fæcal discharge continued more or less constant and copious. At the end of the week the improvement was very decided, and continued for ten days—viz., until July 8th, when acute pain in the left side of the abdomen and rigors occurred, followed by enlargement of the glands in both groins; and sloughing of the skin over the sacrum and right hip, which had begun four days after the operation, but subsequently had appeared likely to cease, began again to extend itself, in spite of the partial removal of pressure by means of the water-bed, &c. It was found, on examination, that the morbid growth occupying the rectum had encreased very much, and it was evident that the difficulty attending the emptying of the bladder was caused by its pressure. A tumour projected through the sphincter ani a few days before the patient's death, which bled on being touched. The enlarged glands in both groins continued to increase in size, and the skin in the left groin began to ulcerate. Death occurred on the 26th of July. No examination of the body took place.

CASE OF INTESTINAL OBSTRUCTION, FROM DISEASE OF THE RECTUM, TREATED SUCCESSFULLY BY OPENING THE DESCENDING COLON IN THE LEFT GROIN.

By Alfred Baker, Surgeon to the General Hospital, Birmingham.

On the 15th of August, 1849, the author was called to visit Mrs. T——, aged sixty-two, who was suffering from severe pain in the umbilical and hypogastric regions, with retching and vomiting, fullness in the abdomen, flatulence and constipation. The symptoms were at first attributed to her having eaten indigestable food, and were treated with that view. The symptoms for the most part disappeared, but the pain continued from time to time. On the 1st of October she had a recurrence of the symptoms, not referable to any obvious cause; and again on the 11th. On the 9th of November, the author was summoned to her, and found many of the signs of intestinal obstruction present, and within reach of the tip of the finger the rectum was found obstructed by a firm growth, occupying its whole circumference. Attemps were made for a few days, and with partial success, to unload the bowels, by passing a small œsophagus-tube into the structure, by injections, and the use of purgative medeines. In a few days, however, constipation returned, and with symptoms of peritonitis. These symptoms were subdued, and diarrhœa came on; but this ceased spontaneously, and constipation returned, and increased gradually; and on the 17th of January, 1850, perfect obstruction took place. On the 23rd, the symptoms were so urgent that an operation was proposed and assented to. The descending colon was opened in the left lumbar region, an incision being made transversely across the left loin for five inches. After the division of the muscles and fascia, the quantity of fat which presented itself was so great, that it was necessary to cut away part of it. The intestine was attached by four sutures to the skin before opening it. The opening was followed by the escape of a large quantity of semi-liquid fæces. The daily reports of the state of the patient after the operation are given by the author. She went on favourably, and on the 18th of April, it is reported that she got up, three weeks after the operation; that her general health is good; and that she has gained flesh. The lumbar opening is large enough to admit the index-finger, and the motions pass easily through it. She wore an ivory plug, attached to a padded steel plate, fastened by a belt; but after a time the plate was found inconvenient, and the plug was attached simply to a plate of vulcanized india-rubber. Up to this time, she has remained free from symptoms of intestinal obstruction; but within the last few months, has been attacked with symptoms which indicate that the morbid growth in the pelvis has extended to the abdomen. The author then gives his reasons for preferring the operation in the loin, in this case, to that proposed by Littre, which were—1st. That there was less risk of rekindling inflammation of the peritonæum. 2nd. That the presence of femoral hernia, which existed in this case, might have given rise to displacement and adhesion of the intestines, so as to interfere with the finding of the large intestine, in an operation of the groin. 3rd. That as the point of obstruction was ascertained, there was no need of any exploratory incision; and he then points out the general advantages of the operation selected. In commenting on the operation at the loin, the author adverts to the fact, that the appearance of the anterior layer of the lumbar fascia may induce the supposition that the intestine is arrived at, as it has at times a bluish-green colour, and looks like intestine. But the longitudinal fibres which characterize the large intestine will not be seen; and on making a careful puncture of the fascia, a protrusion of loose renal fat will take place; and until this fat is reached, the operator may be sure that he has not arrived at the bowel. In speaking of the tendency which always exists, after these and all similar operations, to contraction of the cicatrix, the author expresses his belief that this tendency, in the present case, was materially lessened by the habitual use of the plug, which, he says, was a great comfort to the patient, as it enabled her

to go about, and mix with the world, without the fear of the accidental escape of the contents of the bowel; and he adds that the patient was able herself to adjust the apparatus, and attend to the evacuations and to the dressing of the wound, without requiring the aid of any second person.

CASE OF SCIRRHUS OF THE RECTUM; FORMATION OF AN ARTIFICIAL ANUS.

By John Adam, Esq., Surgeon to the London Hospital.

The patient was a lady, thirty-five years of age, the mother of children. She had for a considerable time complained of great difficulty in passing her motions. This was accomplished with pain and much straining, and she was the subject of hæmorrhoids. She was hereditarily predisposed to cancer. About a year ago, a surgeon pronounced her case one of cancer of the rectum, with ulceration. The bowels were constipated nine days, and the usual purgatives were administered, and scruple doses of calomel, without effect. Her sickness was allayed by opium and sucking ice. The rectum-tube could not be passed above four inches. Scirrhous rectum very high up, was presumed to be the cause of the obstruction. Metallic mercury, to the extent of two pounds, was given, a small quantity of which passed soon after. The operation was performed according to Mr. Luke's method (see his case in the last number of the Society's *Transactions*). The descending colon and sigmoid flexure were undistended. In the course of a few hours, a large quantity of fluid fæces passed, and the relief was complete. She continued to progress favourably, and since the operation has been better than she had been for some years. Occasionally a small quantity of fæces pass per anum, but it is nearly all discharged by the wound; there is also occasionally a small quantity of bloody mucus passing per anum. A light truss is used to restrain the constant passage of the fæces, and there is a distinct tendency to pass them twice daily. A large quantity of the mercury passed by the wound soon after the operation, but a very considerable quantity was retained until a short time ago, and it then passed per anum. The patient was slightly salivated, apparently from the calomel, the mercury being unaltered.

MIDWIFERY.

ON THE USE OF GALVANISM IN OBSTETRIC PRACTICE.

By John Hyde Houghton, Esq., Surgeon to the Dispensary, Dudley.

[With the exception of Dr. Simpson, those who have published cases of uterine hemorrhage speak in unvarying terms of the power of galvanism, and would lead us to look to it with confidence in some of the most trying difficulties in obstetric practice. Dr. Radford, of Manchester, speaks in high terms of praise as to its value.]

Case I.—Mrs. M., aged 28, a small, delicate, anemic-looking person, is now, July 13, 1847, at the full term of her eighth pregnancy. I saw her at 7 A.M.; she has had premonitory pains for a day or two, but they are now regular and tolerably strong every three or four minutes; the os uteri is three-fourths dilated; membranes entire, soft parts lax and well lubricated; head just descending through the brim. Pain continued regular, with little progress, until 8 o'clock, when I ruptured the membranes. The pains then gradually diminished in force and frequency, and at a quarter-past 8 had quite left her. Three doses of ergot of rye, stimulants, bandage, and frictions, failed to produce the slightest pains; the head had descended into the hollow of the

pelvis. I waited until half-past 11 o'clock, when she became very anxious about herself, and begged I would, if possible, hasten the delivery. I therefore sent for the galvanic apparatus, and at 12 o'clock commenced its administration by applying a very feeble current, one pole being placed in the vagina, the other on the abdominal wall; she immediately cried out, "Oh! you are running a pin into me." After three or four minutes she had a very slight pain, and after three or four minutes more one stronger; the pains continued increasing in force and frequency for about twenty minutes, when she was delivered of a small, but healthy child.

The pains produced by the galvanism exactly resembled those of natural labour; and, but for the presence of the apparatus, one would have said she was completing her labour in a natural and favourable manner. The poles were *kept constantly applied.* The uterus soon contracted firmly, and expelled placenta. The mother and child did quite well.

Previously to her present pregnancy, Mrs. M. had several abortions from ulceration of the cervix uteri, which had yielded to the usual treatment. From this time she enjoyed better health than she had done for years. She got stout, lost the anæmic look, and remained quite free from uterine symptoms, which had long troubled her.

Case II.—On the 2nd of July, 1848, at 4 A.M., she was again in labour at her full time. I was prevented from attending her myself; and she was attended by Mr. W. W Tinsley, now of the Sheffield General Infirmary, whom I acquainted with the facts of her previous labour. The following is that gentleman's account of the labour, slightly condensed:—"She had been in pain all night, and the pains now came on every five minutes, not strong, and sometimes not quite so often. The membranes were ruptured before I saw her; soft parts relaxed, cool, and moist; os uteri high up, soft, and dilatable, but little dilated; head presenting in the first cranial position. In an hour, little progress being made in the dilatation or descent of the head, I gave her a dose of ergot of rye, which only produced vomiting: in another hour she seemed much the same; and with my hand on the abdomen I could feel that the uterus did not contract with the pains, which continued gradually decreasing, although they never left her entirely. The bandage, friction, &c., were now tried without effect. This state continuing, as I knew that galvanism had been used with effect before, it was again applied. An attendant placed one pole over the fundus on the abdominal wall, and I applied the other at the outlet of the vagina, at the same time watching the progress of the head. At first we could not regulate the power, but at length we got a *continuous stream*, which was gradually increased as far as she could bear, for that only seemed sufficient to produce contraction. In less than ten minutes she complained of the pains getting stronger, and indeed the effect was obvious, for the pains soon became expulsive; the head began to advance, and in about a quarter of an hour made its appearance at the outlet. The poles were now removed, and a natural pain was sufficient to expel the head, and soon afterwards the body. The placenta came away in a few minutes, and the uterus contracted firmly. Her convalescence was quite favourable. During the progress I sometimes moved the pole from the vagina to the sacrum, but the effect seemed to be the same. The chief pain was caused by the pole over the fundus. The galvanism did not seem to produce natural alternating pains, but rather one gradually increasing contraction, which hardly left her until the child was born. The time which elapsed from the first application of the poles until the birth of the child did not exceed one quarter of an hour."

Case III.—Mrs. I. was taken in labour September 5, 1847. I saw her at half-past nine, P. M., when I found the os uteri dilated to the size of half-a-crown, soft, and dilatable; head presenting naturally; pains regular, but feeble; the pains continued frequent and feeble until half-past one, at which time the child was born. As the head was passing the outlet a copious gush of blood took place, and blood continued to flow *violently* after the birth of the child. I at once removed the placenta without difficulty; the flooding, howe-

ver, continued; pressure with the hand caused the uterus to contract, by which the bleeding was *restrained;* a slight drain still continued; at times the uterus relaxed under the hand, when the blood immediately returned. Shortly an alarming gush took place, followed by pallor, restlessness, vomiting, and fainting; ordinary means had been already tried, and now cold water was *dashed* freely on the abdomen, without producing any permanent effect. The uterus *would not contract effectually,* and the bleeding continued. I now sent for the galvanic apparatus, and during three quarters of an hour which elapsed until its arrival, I kept firm pressure on the uterus with my own hands, by which means the flooding was restrained. The organ, however, showed *a constant disposition to relax,* and consequently the *bleeding to return.* The state of the patient had become very alarming; the poles were applied without delay; and they soon produced *firm and permanent contraction, and complete suppression of the bleeding.* For a short time I kept my own hand firmly on the uterus, that no unobserved relaxation and internal flooding might occur. Shortly afterwards I desired an assistant (who had much midwifery experience) to feel if the contraction continued; the moment he laid his hand on the abdomen he looked quite astonished, and said he had never felt so firm a uterus. Her recovery was gradual, and without accident or drawback, more than that which resulted from loss of blood. I have since attended her in an easy and natural labour, in which there was no accident or complication.

To be continued.

HOW TO MAKE A SPONGE TENT.

By Dr. Edward Rigby.

[Speaking of the best means of dilating the os uteri, Dr. Rigby says]

A well-made sponge tent is a most effectual means for dilating the os uteri to a considerable extent, with but a moderate degree of force, and but trifling pain; indeed, some patients appear to suffer nothing more than a sensation of pressure from it. But a well-made sponge tent is a great desideratum, and I believe that the medical man must make them himself if he wishes to insure them of the requisite quality; for it is very difficult or impossible to get them made so firmly, that they can be introduced without breaking, and yet be capable of expanding to the necessary amount.

A piece of tolerably fine sponge, previously well dried, should be soaked in mistura acaciæ, and rolled up into a cylindrical form, somewhat in the shape of a small cigar, tapering to a point at one end. The other, or thick end, must be rolled round a middling-sized awl, partly for the purpose of leaving a central perforation into which the end of the instrument which carries it is to be inserted, and partly to fix it, while a piece of stout cord is wound tightly and closely round it from the thick end up to the point. By this means, the sponge is powerfully compressed into the cylindrical form above mentioned, and, if well dried, becomes as hard as a piece of wood, and retains its compressed state perfectly when the cord is removed. Any little projections or roughness may be trimmed off with a sharp knife; and, lastly, the tent is to be dipped several times in melted tallow rendered harder by the admixture of a little white wax, until it has become thickly coated. A piece of string or tape is fastened to the lower or thicker end to assist in removing it from the os uteri when expanded. The heat of the part soon melts the unctuous covering, and thus enables the tent to slide up in its own grease as it gradually melts, when otherwise it might have been difficult to introduce it. The secretions of the part slowly pervade the sponge, and dissolve the hardened gum with which it has been soaked, and the sponge gradually expands as it returns to its full size.

Twelve hours is usually a sufficient period to effect this in; and the degree of dilatation produced will guide us as to the introduction of a larger tent on the removal of the first.—*Med. Times, Dec.* 6, 1851.

SELECTED MATTER.

ANATOMY AND PHYSIOLOGY.

CASE OF GANGRENE OF THE LUNG.

Dr. Hailey read an extract from a letter giving an account of an interesting case of gangrene of the left lung, which occurred at the Merchant Seaman's Hospital at Hong Kong, under the care of Dr. W. A. Harland, of that hospital:—"An American sailor was brought up to the hospital in such a condition as almost to preclude all hope of his surviving beyond a few days. On the second day after his admission he fell out of bed, and on being raised up again he complained of severe pain under the left mamma. Dr. Harland immediately examined the part, and found a swelling of considerable size, and with distinct fluctuation. With some hesitation he made a small incision, and finding an escape of pus only, he enlarged the opening to about an inch and a half in length, when upwards of half a pint of extremely fœted pus, with small shreds of gangrened lung, were evacuated, the discharge being suddenly stopped by a mass of the dead lung itself protruding through the orifice. By gentle traction with the forceps he was enabled to withdraw this to the extent of nearly two inches: but finding it not yield any further, and afraid of hæmorrhage if he cut it, he applied a poultice, which was renewed every three or four hours, and at each dressing pulled out the lung a little more as the opening increased in size by ulcerative absorption. On the third day after the incision was made, the protruding mass became so troublesome and offensive, besides exhibiting some appearance of spontaneous separation, that Dr. Harland removed it by the knife; no hæmorrhage occurred, and the opening appeared to be filled up by the pericardium, (as by a valve,) which gradually became thickened and adherent all round to the parietes of the chest. The detached piece of lung would scarcely go into a pint measure, and weighed nearly a pound. The man rapidly recovered, and about three months afterwards shipped again as a seaman, in a vessel bound to New York." Dr. Harland subsequently writes—"Last month (Dec. 1851,) I was agreeably surprised by a visit from my old patient, who came to show me that he was alive, and quite strong and hearty again. It appears that in consequence of the ulceration having exposed a part of one rib, some discharge continued to ooze from the wound for some months after he left me, and on which account he went into one of the hospitals on his arrival home in New York. Under medical treatment for a short time the wound soon healed, and his case excited considerable interest, almost all the principal medical men there having visited and examined him. From New York he went to sea again, and then to the mines in California, where he worked as a gold-digger for above a year, and at last to Hong Kong, where he arrived last month. He told me he could work as well as ever, and had never been troubled with cough since he left me; the only difference he could perceive was, that he thought that he was rather shorter-breathed than before, if he had to exert himself strongly for any length of time. A marked difference, too, is at once perceptible in the relative dimensions of the two sides of the chest. He went back to California a few days ago, (Jan. 29, 1852.)" Dr. Halley regretted that more particulars were not added in regard to the subsequent physical and auscultatory signs, but hoped at a future time to lay them before the Society; at the same time he thought the case so remarkable and unique as to be well worthy of their attention, even in its present form.

NEW STETHOSCOPE.

Mr. B. W. Richardson exhibited to the Society a new form of stethoscope which he had recently invented. With reference to conveyance of sound, this stethoscope did not differ from other kinds, but it was much more portable and convenient. The improvement lay in the ear-piece, which was joined to the tube by means of a very simple hinge-joint, on the principle of the joint used in the enema syringe, so that the ear-piece could be folded down upon the tube when not in use, (like the top of a round table,) and could then be carried in the breast pocket without causing the slightest inconvenience. The joint was also so managed that the instrument could be used with the ear-piece standing at different angles, an advantage of some importance. Two other stethoscopes were likewise shown, in which the ear-piece moved down upon the tube; but these, though simple in construction, were not so complete as the one above described. The stethoscope was made by Weiss and Son, and was of moderate cost.

MEDICINE.

ON APPOPLEXY AND EPILEPSY; AND ON AN HOSPITAL FOR EPILEPTICS.

By Marshall Hall, M.D., F.R.S.

I propose tracheotomy, not as a remedy for appoplexy or for epilepsy; but for stertor or paralytic Laryngismus and its effects, in the former malady; and as a preventive and security against spasmodic Laryngismus and its effects, viz., convulsion, and the injury apt to be inflicted on the cerebrum, and the medulla oblongata, on the mind and limbs, in the latter dire calamity.

In the apoplexia gravior, with laryngeal stertor, tracheotomy affords the chance for life; in the epilepsia gravior, tracheotomy supersedes the laryngismus, and the convulsion and its dire effects.

LECTURE II.

On Laryngismus.

73—Laryngismus has scarcely taken a more prominent place in medical observations and writings than trachelismus. The term has hitherto been applied and limited to a form of infantile convulsive affection. It ought to be extended and applied to *all* affections of *the Larynx* of a secondary and functional character, and not dependent on alteration in the structure of the larynx itself.

74—As in the case of trachelismus, I propose to trace this affection through its different forms, back to its various origin, and onward to its most dire and dangerous consequences.

75—Trachelismus is, I believe, always spasmodic. Laryngismus is sometimes of a *spasmodic*, sometimes of a *paraly'ic* character.

76—The beautiful experiments of Legallois give us the type of the *paraly'ic* form of this affection. The recurrent laryngeal nerves being divided, the rima glottidis collapses partially and induces paralytic laryngismus.

77—A similar effect is produced whenever the influence of the cerebrum or of volition is withdrawn from this organ.

78—Even in the partial subtraction of this influence in *deep sleep* we observe a first degree of laryngismus in the snoring or stertor of this state of the cerebrum.

79—As in the case of the muscles of the neck in general, volition being withdrawn, during sleep, from the muscles which preserve the larynx open, the constrictors contract, from tonic or spinal action; and as the veins are compressed in the former case, so, in this, the larynx is partially closed and a slight stertor is induced.

80—The most familiar instance of *spasmodic* laryngismus is observed on allowing a drop of water or a crumb of bread incautiously to drop into the rima glottidis; or in the still more formidable case of choking.

81—One gentleman experiences a spasmodic laryngismus on attempting to take a *draught* of cold water.

82—From these slighter forms of laryngismus I pass on to the severer morbid forms of this affection observed in apoplexy and epilepsy.

83—In the milder cases of these affections, no affection of the larynx, or none of any degree of severity is perceptible. It is when the severer forms of these maladies take place, that the paralytic (or apoplectic) and the spasmodic forms of laryngismus present themselves.

84—The occurrence of paralytic or of spasmodic laryngismus denotes the *severity* of the malady. But the *re-action* of this condition of the larynx, in augmenting that severity of the disease, is of a still more formidable character.

85—The influence of laryngismus and of the obstructed respiration on the veins and integuments of the neck and face, and of the encephalon, is of the most dangerous character. On it a distended condition of the veins, purpurescence, and intumescence of the face and neck, and coma, alike depend; the proof of which is afforded by the observed effects of tracheotomy.

86—The apoplectic affections may be divided, as I have already stated, into those of the milder and into those of the severer forms: the former of these depends on trachelismus; the latter on laryngismus. By tracheotomy the apoplexia gravior is changed into the apoplexia mitior!—and even this may speedily subside. This conclusion is one of great practical, as well as pathological, importance.

87—Analogous remarks may be made upon the spasmodic laryngismus. Epilepsy also assumes two forms: the first, that of the epilepsia mitior; the second, that of the epilepsia gravior. As long as there is only trachelismus, the case assumes the form of epilepsia mitior only. If laryngismus supervenes, the case becomes epilepsia gravior;—convulsion and its dire effects supervene. Tracheotomy restores the patient to the condition of the epilepsia mitior!

88—Thus spasmodic laryngismus is the effect either of direct or of diastaltic action by or through the *medulla oblongata*. It is direct when it is the effect of emotion, and when it is excited by the condition of the circulation in the medulla oblongata induced by trachelismus. It is diastaltic when it is the effect of dental, gastric, enteric, or uterine irritation.

89—That spasmodic laryngismus should be the essential condition of convulsion and all its dire consequences, and that tracheotomy should render such events impossible,—are surely conclusions of the deepest interest, both to the pathologist and the physician. I shall shortly adduce practical proofs of these inferences and conclusions from theory,—inferences and conclusions not adduced by any of the great *observers*.

90—To prevent convulsion, is, frequently, to save life, to preserve the intellect, to avert paralysis; in a word, to achieve a victory over the worst form of that malady which the ancients designated κατ' ἐξοχην, the Herculean disease, the morbus sacer, &c., from its fearful immunity: the victory has been achieved by physiology, and especially by the discovery of *The Diastaltic Nervous System!*

91—Laryngismus, in its spasmodic form, is, like trachelismus, doubtless, frequently the immediate effect of the emotions or the irritations. But, in both its spasmodic and its paralytic forms, it is frequently also the effect of trachelismus itself:—

92—When trachelismus has induced cerebral congestion, and when this congestion has attained a certain degree of intensity, a degree of stertor or of paralytic laryngismus supervenes, with an augmented degree of the same congestion in its turn;

93—When trachelismus has induced congestion of the medulla oblongata, laryngismus, convulsion, torticollis, bitten tongue, convulsive dashing to the floor, are the terrific consequences!

94—All these events I have observed and traced in patients with great care and attention. The chain of facts becomes, indeed, matter of *pure observation!*

95—The patient may be affected with the slightest apoplectic symptoms only. Or these may pass variously into deep apoplexy, with the most formidable stertor, with apoplectic laryngismus; or into epilepsy, with frightful convulsion, the result of spasmodic laryngismus.

On the Laryngismus in relation to Apoplexia Gravior.

96—What is *stertor?*

97—The closure of the eyelids during sleep is accomplished by a *positive* power; for in cases of extreme debility or exhaustion, in which all the nervous centres participate, this closure is incomplete.

98—The orbicularis, during sleep and especially in comatose affections, is, in reality, contracted by the influence of tonic or *spinal* action, unmodified by volition. This phenomenon presents the *type* of other phenomena belonging to the same class,—of the state of the muscles of the neck in the trachelismus of sleep, and of the muscles of the larynx and of the isthmus faucium in the laryngismus of sleep. It is in this manner that sleep, and especially heavy sleep, so frequently passes into apoplectic or epileptic affection!

99—Volition being withdrawn, in heavy sleep and in coma, from those muscles whose office it is to preserve the larynx freely patent, partial closure, and paralytic larangismus, or *stertor*, is the consequence.

100—But this laryngismus brings with it impeded respiration, and with this a still more impeded venous circulation in the neck, and an augmented apoplectic condition of the cerebral nervous centre. This condition of the larynx, of the respiration, and of the cervical venous circulation, is, in reality, one of the causes, if not the principal cause, of the fatal result in apoplectic and other comatose affections. M. Andrel observes—"Le stertor de la respiration est en general un signe d'un tres facheux augure; et il est rare que le individus qui le presentent d'une maniere prononcee echappent a une mort prochaine." "C'est veritablement par la gene de la inspiration que succombent le sujets frappes d'hemorrhagie cérébrale, dans le cas ou l'attaque est forte et ou ils meurent promptement." *

101—But this fact, and indeed all that has been said relative to the influence of apoplectic laryngismus, are confirmed by the following cases of Mr. Sampson, formerly of Salisbury, now of Chester-street, Belgrave Square, and of Mr. Cane of Uxbridge,—cases, the results of which are amongst the *greatest achievements of the Medical Art.*

102—Mr. Sampson's case is extracted from the *Transactions* of the Royal Medical and Chirurgical Society, vol. xx., p. 45. Why Mr. Cane's case, which I recently presented to that learned body, was refused a place in those *Transactions*, it would puzzle any one, without imputing the most unworthy motives, to explain. Mr. Sampson's skill saved the patient's life. Mr. Cane's prompt and energetic procedure not only saved the patient's life, but preserved him afterwards from dire attacks of *Epilepsy!*—an event certainly of no ordinary character in itself, and the fulfilment of a *prediction* of mine made many years ago.

103—I give Mr. Sampson's case without abridgment:—

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* Clinique Medicale ed. 2 tom. v., p. 383.

104—"Abraham Harris, aged 31, was brought to my house on the 31st of March last, in a state of complete insensibility from intoxication, the pupils largely dilated, the breathing stertorous, and all voluntary motion having been lost for at least four hours before I saw him. The account given by those who came with him was, that he had attended a convivial meeting in the course of the day, at which he had drunk freely both of beer and brandy; his companions admitted that he had taken more than a pint of the latter; but it has since been ascertained that his glass was repeatedly filled up, without his knowledge, with white brandy instead of water, so that it is impossible to calculate what quantity of spirit he had actually taken.

105—I immediately used the stomach-pump, and drew off between three and four pints of fluid, a great part of which appeared to consist of brandy; after which, tepid water with ipecacuanha diffused in it, was several times injected into the stomach, and after a while withdrawn again, with a view to excite vomiting, and thus rouse the energies of the brain. Finding, however, that means failed, a strong solution of salt and water, and afterwards the sulphate of zinc, were repeatedly tried, without any better result; he became, if possible, more comatose, the countenance turgid, the breathing more and more difficult; the pulse grew fainter, and was at last scarcely perceptible; at the same time, the whole surface of the body was cold and clammy, and he was insensible to every kind of stimulus. As he was some miles from his home, I had him removed to the Infirmary, and called a consultation of the other medical attendants, who arrived in the course of half an hour; but as, in addition to the above symptoms, he had lost the power of swallowing, and every appearance indicated the rapid approach of death, nothing was ordered for him but a turpentine injection, there being no ground to justify a reasonable hope of recovery.

106—"At this period, it occurred to me, whilst standing by his bed side, that the comatose state in which he lay might not arise from apoplexy, but from torpor of the brain, in consequence of that organ being supplied with blood not duly oxygenated; for the shrill tone and extreme difficulty of respiration showed the existence of a collapse of the glottis, with imperfect transmission of air into the lungs, which might be accounted for by a paralyzed state of the eighth pair of nerves and recurrent branches. With this view of the case, I again appealed to my colleagues, and strongly urged that a trial should be given to the operation of tracheotomy; for I could not but hope that, if mechanical respiration were carried on for a time, the blood might regain its proper stimulant properties, and restore the energies of the brain and nervous system. Upon their consenting to give him this chance, the operation was performed, without loss of time, by Mr. Andrews, under whose care, as surgeon for the week, the patient was now placed.

107—"The trachea was no sooner opened than the distension of the veins about the head and neck subsided, the violent efforts of the extra-respiratory muscles ceased, and in about half an hour regular and easy respiration through the wound was completely established; at the same time the pupils became lightly sensible to the stimulus of light, and the pulse returned to the wrist.—The immediate result of the operation being thus far satisfactory, nothing remained to be done but to give directions for the frequent removal of the mucus which appeared at the wound, and to keep the surfaces of the incision asunder until the integuments and muscular layers had become agglutinated to each other: this latter object was effected by means of a piece of strong spring wire, with a bow at each end of it, which, being introduced in a bent state, was allowed to expand, and the opening in the trachea was thus prevented from being covered by the muscles, even during the efforts of deglutition.

108—"He continued perfectly quiet during the night, but had no return of consciousness until the following morning, when he gave us to understand, by signs, that he suffered from headache and soreness in the pit of the stomach; there was tendency to sickness, and the tongue was coated with a peculiar

whiteness, as if rubbed over with chalk. Moderate purgatives, followed by mild alkaline medicines, soon removed these symptoms, and a few leeches were applied to the throat for the purpose of checking too high a degree of inflammation; after which no further treatment was required; but the wound being healed in about three weeks, he was discharged cured, and has continued up to this time in the enjoyment of perfect health."

109—To Mr. Cane's case I shall have to refer hereafter, in its relation to the treatment of epilepsy; for, as I have already stated, it possesses a double and an extraordinary claim to our attention and admiration. I give it in Mr. Cane's own words, as addressed to myself:—

"Uxbridge, May 17, 1852.

110. "My dear Sir,—I was called suddenly to attend A. B. aged twenty-four, on Feb. 1, 1851. I was told that he had been taken out of a canal-boat the day before, apparently in a dying-state. On entering the house, I found the patient in convulsions, with a most turgid face, with extravasated blood on the conjunctivæ, which were of a perfectly livid colour, with cold extremities, and a pulse imperceptible at the wrist, his heart acting most feebly. Respiration was impeded to such an extent, that I concluded that the whole mass of his blood was becoming rapidly of a venous character, and quickly losing the properties necessary to support life. The platysma myoides and the sterno-mastoidei were in powerful action, but most so on the right side, throwing the chin, which was kept in constant motion by spasm, nearly to the shoulder on the left. Inspiration was only accomplished by seldom and short catches. The veins of the head and neck were everywhere visible, and greatly distended. I was told by the by-standers that he had been in the same state for nineteen hours, with some intermissions, but without any return of sensibility. I looked upon this as a case commencing with spasm of the larynx and muscles of the neck in general—the insensibility being produced in the first instance by obstruction to the return of blood from the brain by this state of spasm, and kept up and brought to a state of coma by spasm of the arytænoid muscles preventing free access of air to the lungs, or even enough to arterialize the blood sufficiently to support life. With respect to the exciting cause of the spasm, I could learn nothing at the time; but if acidity in the stomach, or dyspepsia, will, (as it frequently does) produce cramps in the legs, how readily can we understand how the same thing may happen with the muscles of the larynx, particularly when we remember the origin and distribution of the gastric and recurrent laryngeal branches of the pneumogastric nerves.

111. "Feeling convinced that the patient must shortly expire, and that the root of the evil was in the closure of the larynx, I at once proceeded to open the trachea—a matter of no small difficulty on account of the twisted state of the neck, the engorged state of the vessels, and the constant action of the muscles. However, I felt it must be done, and I directly made an incision from the upper border of the sternum, extending in the medium line upwards for about two inches. After separating the edge of the sterno-hyoidei muscles, a large and much distended vein protruded forwards (the middle thyroid,) which in its engorged state, was so troublesome, that I at once put two fine ligatures round it at each extremity of the wound, and removed the part between them. After this, the rings of the trachea were soon reached and divided, and the cut ends of the rings were then seized with a tenaculum, and small pieces removed from each.

112. "The immediate effect of the entrance of the air into the chest was to relieve all spasm. I was enabled to place the man's head straight, and in a remarkably short space of time the turgescence of the head was relieved. The face assumed a mottled appearance at first, then became red, and, in the course of ten minutes, pale; but, during these changes, the pulse had again become perceptible at the wrist, and means were used to induce circulation in the legs and feet.

113. "I proceeded to convert the stilet of a female catcheter into hooks, which were placed in the wound, and fastened behind the neck so as to keep the trachea open. This I was obliged to do, from being thrown in the way of the case with only my pocket instruments at hand.

114. "By the time I had done this, the patient was sufficiently sensible to try to speak, and I was able to tell him what had been done, and to induce him to be quiet. In an hour I left him, and shortly after returned with a canula, which I placed in the trachea, drawing the wound with the integuments together with strips of plaster, until it exactly fitted the silver tube. The two ligatures were brought out at the upper and lower extremities of the incision.

115. "On my next visit, on the following day, the pulse was about 90, and he was, in every respect, doing well: and on my placing my finger on the mouth of the canula, he was able to tell me that he had been the subject of epilepsy for seven or eight years, and that lately the attacks had been more frequent and more violent, and that he did not think he had passed two days together, during the last two years, without an attack.

116. "He has done well from the first day, the ligatures coming away on the fourth and fifth days and the wound uniting by the first intention, and closing round the canula.

117. "He remained under my care until the 15th of the month, without any return of his fits. He then went with his boat into Staffordshire, and has not yet returned; but I hear that he has remained quite well up to this time."

118. No one can read the accounts given by Mr. Sampson and by Mr. Cane, of the immediate effect of the operation of tracheotomy on the circulation of the neck, without feelings of the deepest interest.

119. Mr Sampson observs—"The trachea was no sooner opened, than the distention of the veins about the head and neck subsided."

120. Mr. Cane states that—"the veins of the head and neck were everywhere visible and greatly distended;" that—"the immediate effect of the entrance of air into the chest was to relieve all spasm;" and that—"the face assumed a mottled appearance at first, then became red, and, in the course of ten minutes, pale."

121. I leave these facts to my readers' attentive consideration. I write for the candid lover of our profession and of truth.

"To Dr. Marshall Hall."

(To be continued in our next.)

ON FATTY DEGENERATION IN ITS RELATION BOTH TO SOFTENING ON THE BRAIN AND APOPLEXY.

By William Frederick Barlow, Esq., Resident Medical Officer of the Westminster Hospital.

[Fatty degeneration of the arteries is one of the most important points to be noticed in relation to the ramollissement of the brain.]

Among the facts which might be adduced to show that obstruction of the larger vessels can give rise to ramollissement, two cases, observed and recorded by Mr. Vincent, wherein that affection succeeded to the tying of the carotid artery, may be well mentioned. The frequent concurrence of degeneration of arteries and apoplectic effusion has long been known. It is remarked by Mr. Gulliver that, "in a man who died of this disease, the coats of the arteries of the brain, even of the smallest branches, were studded with and made fragile by fatty patches." Professor Rokitansky, whose observations rest upon immense experience, alludes especially to fatty degeneration of the middle coats of arteries in cases of this affection, and says of the well-known changes of those vessels:—"Hence it may be inferred that the more minute arteries, and

even the capillaries of the brain, are in a similar condition; especially as the former are sometimes found ossified, and the brain filled as if with silver wires." But microscopical research was wanting to place the state of the minute vessels beyond dispute; and I must refer to the observations of Mr. Paget, who not long since published an important paper 'On Fatty Degeneration of the Small Blood-vessels of the brain, and its relation to Apoplexy.' "It cannot (he observes) but be that this affection should constitute a predisposition to apoplexy, whether occurring in its simplest form or in connection with cerebral softening." Rokitansky, speaking of disease of the vessels (and assuming, as I suppose, the implication of the minute ones), observes:—"Such a state of the coats appears to some extent necessary as a cause of apoplexy; for it is often observed that the deepest congestions, whatever their nature, but especially those intense mechanical ones which give rise to cyanosis, do not produce apoplexy." It is, doubtless, in those cases wherein fatty degeneration of the smaller vessels of the brain exists that the impeded return of blood from the organ, so much insisted on by Dr. Marshall Hall is peculiarly dangerous. And it would be of much moment to know whether some of those causes of epilepsy which pass into, and end mortally by, apoplexy, are not thus fatal because of the small cerebral channels being so diseased as not to be able to resist the distension which the convulsion brings. Let it be always remembered that whatever may be the bad effects of degeneration of the easily discernable vessels, it is as nothing compared with that of the minute ones: from hence escape those disastrous effusions which either paralyse or strike with apoplexy. That fatty degeneration which is here referred to means, not a simple *additio* of fat merely, but implies the damage or destruction of the tissues proper to the affected vessels. No longer are they safe channels for the blood to course through; their condition is such that hæmorrhage may surprise at any moment or softening insidiously begin. But let us turn to Mr. Paget's clear description of it:—"When the fatty degeneration has made much progress, changes in the structure, and, not rarely, changes in the shape also of the affected vessels may be observed. The chief change of structure appears to consist in a gradual wasting of the more developed proper structures of the vessels. Growing fainter in apparently the same proportion as the disease makes progress, the various nuclei or fibres are at length altogether lost, and blood-vessels of even 1-150th of an inch in diameter appear like tubes of homogeneous pellucid membrane, thick-set with the fatty particles. The structures of the vessels are not merely obscured by the abnoimal deposits: they waste and totally disappear."

What is the state of the minute vessels in cases of capillary apoplexy where in patches of hæmorrhage are numerously interspersed throughout the softened tissue? Fatty degeneration of the minute vessels would, in all probability, be found in many of them, and especially in the immediate neighbourhood of the effusions. Dr. Hughes Bennett, in his "Pathological and Historical Researches on Inflammation of the Nervous Centres," speaks repeatedly of exudation-granules coating the vessels of the soft portions of the brain; but Mr. Paget has remarked that Dr. Bennett's "attention being directed primarily to the changes of the structure of the brain itself, and to the products of inflammation in it, he appears not to have minutely examined the state of the blood-vessels in the diseased parts. Some of the appearances produced by fatty degeneration are represented by him (in the Edinburgh Medical and Surgical Journal, vol. lviii. pl. v. *fig.* 56, and very accurately in vol. lix. pl. i. fig. 5); but he refers them to the vessels becoming coated externally with exudation-granules, the products of inflammation." I need not say what a point of magnitude is started here in reference to a theory of causes of the ramollissement. A very elaborate writer on this affection, M. Durand-Fardel (as cited by Dr. Hughes Bennett) has gone the extent of saying that "softening connected with sanguineous infiltration, is a proof of inflammation." But the observations of Mr. Paget, to go no farther, would imply this statement to be erroneous.

Rokitansky says that "there is no single cause that will account for the frequent repitition of attacks of apoplexy in many individuals, and its simultaneous appearance at several different spots in the brain, but the presence of disease of the vessels This also partially explains its happening symmetrically* in corresponding portions of the brain at the same or nearly the same period."

But now I come to a point which touches the diagnosis of cerebral affections. A knowledge of the *constitutional tendency* is, it is superfluous to state, of the highest consequence in the investigation of disease. We judge often of what a particular part of the body may be doing, by what the whole body seem disposed to do; much, as in the moral world, we interpret acts by what we know of the *character*. Now, in the study of diseases of the brain, the pathological tendency often demands the closest inquiry, as the tuberculous affection of that organ well show. Obscure head-symptoms occasionally come before our notice, which may portend nothing of moment, or threaten apoplexy. If apoplexy shall be proved, as I doubt not it will be, an extremely common consequence of fatty degeneration, any clue to the probable progression of the latter in the cerebral vessels will be plainly of value. And may we not find it in the *arcus senilis*, which Mr. Canton has proved to be one form of such degeneration, and often associated with other kinds of it? In several cases of apoplectic effusion, which I have lately met with, the arcus senilis has been very palpable; and, so far as my observation at present leads me, I should, in certain cases, wherein it seems doubtful whether apoplexy be foreshadowed or not, lay considerable stress on the presence or absence of this arc, and the rather if a heart were suspected with reason to be diseased, or signs of the degeneration of the kidneys were evident. I believe too, that the arcus will be found, to *some* extent, diagnostic of ramollissement, especially of that form of it which slowly progresses, and without any well-marked symptoms of inflammation. Whether it be so or not demands enquiry at any rate.

The arcus has already been found of utility in the investigation of heart-disease; and this makes it the more reasonable to believe that it will be of service in exploring affections of the brain.

Mr. Canton found, very early in his inquiry, that the arcus senilis, and fatty degeneration of the heart, existed together, and so frequently as to show that the one might be a clue to the presence of the other.† Dr. Williams and Dr.

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* Probably the most unvarying form of symmetrical degeneration is that instanced by the arcus senilis.

† "I have in no instance found this senile arc, when well developed, unaccompanied by fatty degeneration of the heart."—*Lancet*, May 11th 1850. Dr. Latham, in his lectures on the Diseases of the Heart, vol ii. p. 166. points out the difficulty of diagnosing a fatty change of the organ, and insists on the importance of attending to the "constitutional peculiarity;" it is an indication of this that the arcus will be proved of value, in *helping* us to be "able during life to conjecture a fat heart with such strength of probability that we almost know it." See Dr. Ormerod's Observations on Fatty Degenerations of the Heart, for some remarks in reference to its diagnosis and history.—*Medical Gazette*, 1849.

By Dr. Wilde. Mr. Lawrence has noticed the arc (instead whereof a most complete *circle* often happens) at thirty three years of age; Mr. White Cooper, at twenty-eight. Mr. Canton has seen it beginning at sixteen (See Part ii. of Mr. Canton's observations). Dr. Quain tells me that he has observed it well marked at seventeen. Mr. Gulliver says of fatty degeneration of the arteries—"though most common in old age, it was twice seen in subjects not past twenty-one, and once in a boy of sixteen." It would be of great interest to examine the small vessels of the brain in cases of *early* apoplexy. Its most usual time of occurrence has been treated of by Dr Quain, who has analysed a large number of cases in his "Observations on Cerebral Apoplexy, at different periods of life."

Quain have both tried the arc of the cornea as a diagnostic sign, and bear strong testimony to its use in the investigation of heart affections. That use will, *cæteris paribus*, be found the greatest where it appears considerable before its time; but its amount has yet to be determined by inquiry. Sometimes the arc is seen so soon that it may properly be termed an arcus *juvenilis*—an expression which has been already used.

A great part of the interest connected with the whole subject of fatty degeneration lies in its universality, just as its danger consists in its liability to damage, perhaps prematurely, parts necessary to life. See, too, how many phenomena, and what varying consequences, it may produce! Now it encircles the cornea, now stops the heart, now leads to apoplexy, and now (may we not surely say?) to softening of the brain. Mark, moreover, its extreme commonness. Is it not better well to study it than some morbid affection of rare occurrence and strange anatomy? Its relation to many obscure diseases of the nervous system will be found great, I doubt not; and we cannot but believe this, seeing that there is no organ, no part which it affects, that does not influence *very* commonly. Nor will it be the key only to infirmities of body, but to weaknesses and aberrations of the mind. By involving opposite parts at once, it makes imminent more than one kind of dissolution. It may *simultaneously* impair the irritability of the heart, and damage severely the minute blood-channels of the brain; and could a pathologist bring before his eyes the exact condition of the two organs, he would perhaps be enabled to say which would be the more likely to destroy—the sudden arrestation of the heart's action or the apoplectic seizure. All its effects upon the muscular system alone have not been traced as yet. May it not damage the *intestinal* contractility, and so lead to one form of obstinate constipation, and one more especially prevalent in old age?

In speaking of fatty degeneration, I have, of course, borne in mind the wide difference between the deposition of fat on a part or about it, and that serious change which necessarily implies both damaged structure and unpaired action. In Dr. Quain's representations, hardly less full of instruction than their originals, this difference is admirably drawn. The pathological consequences of that fatty deposit which takes place at the "expense and detriment" of the heart's substance are seen at a glance, for fat is not irritable like muscular fibre, nor will it, like it, respond to stimuli and propel the blood. "The heart is a muscle," as Dr. Latham phrases it, "and its functions flow from its attributes as a muscle;" and this may advantageously remembered by any one who is at a loss to discover why so much stir, as he may term it, should be made respecting the conversion of its fibres into fat.—*Med. Gazette, July* 11, 1851.

MIDWIFERY.

A MEMOIR ON THE PATHOLOGY AND TREATMENT OF LEUCORRHŒA, BASED UPON THE MICROSCOPAL ANATOMY OF THE OS AND CERVIX UTERI.

By W. Tyler Smith, M.D., Physician-Accoucheur of St. Mary's Hospital.

The author first directed attention to the minute anatomy of the os and cervix uteri; and here, at the outset, he was desirous of expressing his warmest thanks and obligations to Dr. Arthur Hassall for his valuable assistance in the microscopical part of the investigation, and without which he could not successfully have prosecuted his researches. The mucous membrane of the os and cervix uteri, like the mucous membrane of other parts, consisted of epithelium, primary or basement membrane, and fibrous tissue bloodvessels and nerves. But as there were some special characteristic pertaining to this tissue, he

proposed, for the convenience of description, to examine, first, the mucous membrane of the os uteri and the external portion of the cervix; and, secondly, the mucous lining of the cervical cavity or canal. The epithelial layer of the former of these situations was tesselated or squamous, and so arraigned as to form a membrane of some thickness: by maceration, it could be easily detached, and it was then found closely to resemble the epithelial covering of the vagina, with which it was continuous. Beneath this epithelial layer was the basement membrane, covering numerous villi or papillæ, which studded the whole surface. Each villus contained a looped bloodvessel, which passing to the end of the villus, returned to its base, and inoculated with other bloodvessels of the contiguous villi. These villi had been mistaken for mucous follicles, usually described as covering the surface of the os uteri; but the microscope failed to discover any distinct follicular structure in this situation. When a thin section of the surface of the os uteria was examined by a low power, the points of the villi could be seen as dark spots through the epithelial layer. A careful examination exhibited these spots as slightly depressed in the centre, yet nevertheless slightly elevated in their margin, nipple-shaped, and containing red points, which were the terminations of the looped bloodvessels. The appearances were produced by the villi being obscured by their epithelial covering. The thick layer of scaly epithelium, and the villi with their looped vessels, were the principal anatomical features of the mucous membrane of the os and external part of the cervix uteri; and these structures played an important part in the pathological changes which occurred in the lower segment of the uterus in leucorrhœa. Between the margin of the lips of the os uteri and the commencement of the penniform rugæ, within the precincts of the cervical canal, a small tract of smooth surface was usually found, which to the naked eye seemed of more delicate structure than the neighbouring parts, and when examined by the microscope was found to be composed of cylinder epithelium arranged after the manner of epithelium covering the villi of the intestinal canal. The cylinder epithelium covered in this situation villi two or three times larger than the villi upon the surface of the os uteri—so large, indeed, as to be visible to the naked eye when viewed by transmitted light. Within the cavity of cervix uteri, the mucous membrane contained four columns of rugæ, or folds, arranged in an oblique, curved, or transverse direction; and between these columns were four longitudinal grooves. The two sulci in the medium line, anteriorly and posteriorly, were the most distinct; and of these, the sulcus of the posterior columns was the most strongly marked. In the normal state, the transverse rugæ, with the fossæ between them, were filled with viscid, semi-transparent mucus; and when this was brushed away, a reticulated appearance, caused by numerous secondary rugæ, was visible. The author gave a very minute description of these four rugous columns, and the furrows between them, which was illustrated by some very beautiful drawings of the cervical canal, displaying the rugous columns and fossæ of the natural size, and magnificent nine and eighteen diameters. The latter power showed a large number of mucous fossæ and follicles, crowding the depressions between the rugous elevations also. The author mentioned that a healthy virgin cervix, of normal size, contained at least ten thousand mucous follicles. This anatomical arrangement secured a vast extent of superficial surface, which was still further increased by the presence of villi similar to those found in the lower part of the cervix: they were found in considerable numbers on the large rugæ and other parts of the mucous membrane in this situation. By this disposal of the mucous membrane of the canal of the cervix, a very large extent of glandular surface was obtained for the purposes of secretion. In effect, the cervix was an open gland; and in the opinion of the author, it was in this part of the utero-vaginal tract that the principal seat of leucorrhœa would be found to exist. There was an analogy here which should not be lost sight of, bearing as it did, on the pathology and treatment of leucorrhœa, which was, the great similarity which existed between the skin and the mucous membrane of the vagina and

the external part of the os and cervix uteri. The resemblance, in these situations, was certainly much nearer to the cutaneous structure than to the mucous membrane of more internal parts. These analogies were strongly confirmed by what was observed of the pathological conditions to which these parts were liable, and by the effect of the rapeutical applications. The author dwelt on the fact that the epithelium of the os uteri and external portion of the cervix was constantly squamous, and that the epithelium immediately within the os uteri was cylindrical but not ciliated, but that in the rugous portion of the cervical canal the cylindrical epithelium became ciliated. The mucous secreted by the glandular portion of the cervix was alkaline, viscid, and transparent; it adhered to the crypts and rugæ, so as to fill the canal of the cervix. It consisted chiefly of mucous-corpuscles, oil-globules, and occasionally dentated epithelium, all entangled in a thick, tenacious plasma; it was remarkable for its tenacity; while the mocous found in the lower part of the canal was thinner in appearance. There was an essential chemical difference between the vaginal mucous and the secretion of the interior of the canal of the cervix; the first was always acid, and the latter invariably alkaline. Mr. Whitehead, of Manchester, had noticed this fact, and the observations of the author confirmed his views. The acid of the vaginal secretion was more than sufficient to neutralize the alkaline secretion of the cervix, and when any secretion from the cervical canal entered the vagina it became curdled from the coagulation of its albumen. It was worthy of note, that that part of the mucous membrane of the uterus and vagina which resembled the skin was the only part which, like the skin, furnished an acid secretion. The vaginal mucus was a simple lubricatory fluid. But the uterine cervical mucus had other uses besides that of lubrication; in the healthy condition, in the intervals of the catamenia, it blocked up the passage from the vagina to the fundus; it thus defended the cavity of the uterus from external agencies, and from its alkaline character afforded a suitable medium for the passage of spermatozoa into the uterus.

Having stated his views on the structure of the utero-vaginal mucous membrane, the author expressed his opinion that the glandular portion of the cervix uteri was the chief source of the discharge in leucorrhœa. Leucorrhœa, in its most simple and uncomplicated form, was the result of an increased activity of the glandular portion of the cervix. A follicular organ, which should only take an active condition at certain intervals, became constantly engaged in secretion. Instead of the discharge of the plug of mucous at the catamenial period, an incessant discharge was set up. At first the discharge was but an unusual quantity of the elements of the healthy portion of the cervix. The quantity increases, and becomes a serious drain of the constitution, and the glandular cervix in some cases becomes so excitable, that any unusual stimulus, even mental emotions, provokes an augmentation. The author next referred to the conditions under which the epithelium of the os and external part of the cervix uteri and upper portion of the vagina might be partially or entirely removed. The mucous membrane then presented an intensely red colour, from the presence of the naked villia, and an appearance of roughness or excoriation presented itself. He thought that among the causes which produced this aspect of ulceration were eruptive disorders, similar to herpes or eczema, which strongly marked the analogy between this tract of mucous surface and the skin. He had observed cases in which an occasional herpetic eruption upon the os uteri always produced herpes præputialis in the husband. But the most frequent cause of denudation arose from the alkaline mucous discharge of the cervix irritating the acid surface of the os uteri, and causing the rapid shedding of the epithelium round the margin of the os. A microscopical examination was given of the various discharges met with in these affections, in making which the author was assisted by Dr. Handfield Jones and Dr. Hassall. In cervical leucorrhœa the discharge consisted in quantities of mucous-corpuscles, and in severe cases pus-carpuscles and blood-disks, with fatty matter, involved in a transparent plasma. The epithelial debris is constantly present, but in

limited quantity. In vaginal leucorrhœa, including the secretions of the external portion of the os and cervix uteri, the plasma is opaque, and contains myriads of epithelial particles in all stages of development, with pus and blood globules when the vilh are affected. When a circumscribed ulcer is visible upon the os uteri to the naked eye, after death, such as occurs in eruptive affections of the os and cervix, and is examined by the microscope, with a low power, it is found to consist of a base from which the villi are entirely removed, or in which only the scattered debris of villi remain; and surrounding this base there is a fringe of enlarged villi, partially or entirely denuded of epithelium. The character of the so-called ulceration of the os uteri was detailed, and the nature of the discharges described. The author then observed that if any division of leucorrhœa were made, two principal forms must be recognised—

I. The *mucous* variety, secreted by the follicular canal of the cervix.

II. The *epithelial* variety, in which the discharge was vaginal.

With respect to the so-called ulcerations of the os and cervix, two kinds of morbid change would be observed—

I. *Epithelial abrasion*, by far the most common, in which the epithelium alone was deficient.

ON THE USE OF GALVANISM IN OBSTETRIC PRACTICE.

Concluded from our last.

[In a fourth case, pains commenced on the morning of the 8th of May, 1851, at 2 o'clock; at 9 o'clock the os uteri was fully dilated; at noon it remained in the same state, the membranes were ruptured, and two doses of ergot given, which slightly increased the pains, but the effect soon passed off. At half-past six in the evening she remained in the same state,—the head low down, the pains slight and infrequent. Galvanism was now resorted to, but the result was by no means so decided as in the former, though there can be no doubt that it much hastened the completion of the labour.]

I shall add a resume of all the cases I can find in which galvanism has been used in midwifery practice.

Mr. Dorrington.—1. Internal hemorrhage during the labour; the pains had subsided; *os very rigid*, size half a crown; uterus quite lax; there were all the symptoms of exhaustion from flooding present. Laudanum was given; the bandage applied; and then half a drachm of secale cornutum administered. This produced pains; the head bore down on the os, but between the pains the uterus was very lax. Galvanism was applied. In a short time "*tonic contractions of the uterus had been called into play.*" It was removed, and "*strong contractions took place at once.*" After using it for twenty minutes, it was finally left off. "So firm a state of tonic contraction having been induced, that we considered it safe to leave the woman," ordinary nourishment, &c., &c. The flooding ceased; pains soon commenced, and labour was completed in four hours afterwards. The following day she was going on well, but died of an "obscure peritoneal affection" some days after.

2. A. B.; ninth pregnancy; full time. A copious gush of blood took place at 12, p. m. Mr. Dorrington saw her at 2, a. m.; she was faint; pulse feeble; no hemorrhage or labour pains; the uterus was lax. "*The firmest tonic contractions were induced the moment galvanism was applied.*" "*No more flooding occurred.*"

3. Placenta prævia; flooding without obvious cause between seventh and eighth month. She was seen at the eighth month; slight flooding going on; os the size of a penny piece; membranes entire; slight pains. The membranes were ruptured, and the galvanism applied; "*good strong uterine action set in at once.*" The hemorrhage was very slight, and did not return; the child was born in three hours after. The use of galvanism had to be resumed, as the pains subsided a little.

4. A case of twins, at seventh month; first child born; no pains for an hour after: they then commenced very slightly; in four hours they were very slight, and recurred only every half hour. Galvanism was used, "*the effect was immediate, strong labour pains coming on, and continuing while the current was complete.*" The child was born in a quarter of an hour.

5. Induction of premature labour at eighth month. "The uterus hardened under the application, and she felt labour pain, but this lasted only while the shock was continued.

Mr. CLARKE.—Two cases of uterine inaction, in which galvanism was used with succes and safety to both mother and child. (Mentioned in the Dublin Hospital Gazette, March 1, 1845.)

Mr. CLEVELAND.—Atony in previous labours; pains commenced on Sunday, and continued until Wednesday evening, when they abated, but did not cease. Ergot, &c., failed to produce pains; symptoms of exhaustion then set in. Electro-galvanism was applied. "*A very decided effect was soon produced.*" "*Regular, strong, and frequent pains came on; and in a quarter of an hour a living child and the placenta were expelled with the least degree of hemorrhage I ever witnessed. Immediate and firm contraction of uterus followed.*

The following cases, by Mr. Demsey, I have extracted from Dr. Golding Bird's lectures:—

1. Hemorrhage after the birth of child in a natural labour. Ergot, cold, friction, introducing the hand, and extracting placenta, failed to produce action. After five minutes' application of galvanism, "*energetic contractions ensued, emptying the uterus. In a minute or two uterus was felt firmly contracted,*" and all "*danger at end.*" "The patient quick'y recovered."

2. Profuse flooding with each pain for six hours; patient much exhausted; cessation of pain for three-quarters of an hour. No fœtal pulsation could be heard; os size of crown-piece, soft, dilatable; placenta presenting, and beyond this the head; application of poles seven minutes, *when pains commenced; application suspended ten minutes; no indication of pain. Repeated applications and suspensions—first for ten minutes and then for five minutes—for forty-five minutes, when the child was born.* Renewal of application was necessary for the expulsion of the placenta.

3. Almost identical with last.

4, 5, 6, 7. Galvanism, for post-placental hemorrhage; "*the loss ceased almost immediately on passing a current through the uterus.*"

8. Lingering labour, from atony; labour protracted nearly thirty hours; pelvis capacious, well formed; pains extremely feeble, and at long intervals; fainting at short intervals; no pains for nearly three hours; os dilated; fœtal heart heard. On first application, *slight pains;* repeated after an interval of five minutes, *pains decided and energetic;* galvanism applied every five minutes; child born in forty-five minutes. Ergot, &c., had been previously used in vain.

9. Hemorrhage in miscarriage, without uterine action. Pregnancy at the third month; flooding (from fright) of three days duration: os rigid and unyielding; no pains at all; a portion of placenta felt protruding; constant drain going on; acetate of lead, ice, and cold lotions, ineffectual after seven hours trial; four doses of gallic acid then produced no effect; ergot every twenty minutes without benefit. Galvanism was now used; no effect for twenty-eight minutes, the contractions *then became quick and forcible, and the ovum was expelled in sixty-eight minutes.* Convalescence was speedy.

10. Induction of premature labour at the seventh month; puncture of membranes; no pains in forty-eight hours. Galvanism was then used *thus:* five minutes' application and ten minutes' interval; after the third application, slight, transient, grinding pains came on. Suspension for half-an-hour; cessation of the pains; application resumed every ten minutes for forty minutes; pains became strong and regular; it was now discontinued, but the pains gradually increased, and expelled the head.

Dr. Radford.—1. Hemorrhage at eighth month. Uterus flaccid and inactive. Rupturing the membranes, and other ordinary means, failed to produce action. "*From the moment the circle was completed, uterine pain was excited, and a bearing down effort was produced.*" Tonic contraction took place; the *flooding was arrested, and did not recur*, and the labour was completely favourable.

2. A fourth labour: after full dilatation and rupture of the membranes, the pains ceased; constant discharge of blood for six hours; the uterus could be felt flaccid through the abdominal parietes. Galvanism was now tried; a slight power was at first used, and gradually increased; the poles were applied on various parts of the abdomen. The beneficial influence of the agent was soon apparent; the *atonic state of the uterus was soon changed; the parietes became firmer; pains, at first grinding* and slight, *became powerfully expulsive*, and the child was born one hour after the commencement of the operation. *The uterus contracted firmly; the discharge of blood ceased as soon as the uterus began to contract, and there was no further flooding.*

Dr. Radford observes: "The powerful and sanitary influence of galvanism was most decidedly obtained in this case, and the great advantage of this case is, that its effects may be carried to any degree, from at first only exciting the uterus so to contract that its diameter may be diminished, and that its tissues come to be applied to the surface of the child. This, however, may be so increased as to effect the expulsion of child and placenta.

Dr. Simpson's eight cases are recorded with unusual accuracy, and, apparently, every care was taken to avoid any erroneous or fallacious result being produced, and, in order to insure this, he noted first the duration of pain, and then the duration of the interval,—

1st. Before the application of the wire.
2nd. After the application of the wire, but before the circle was complete.
3rd. Whilst the wires were applied, and the circle complete.
4th. After the removal of the wires.

He thus, it seems, took the best means to ascertain accurately the extent to which uterine action was excited, and to avoid a fallacy which might be caused by the influence of emotion, &c.

I shall not enumerate Dr. Simpson's cases at length, as I have done those previously recorded, because I have a *resume* of them in his own words, in which, of course, the result are truly stated; and as my business is with the results simply, there is not any necessity for my giving them in detail. The others were so given that I might use as much as possible the words of the authors, and thus insure accuracy.

"In one instance the pains were more frequent in their recurrence, but shorter in their duration, during the application of galvanism. In five other cases, the employment of galvanism neither increased the average frequency of the pains nor their average duration. In one case the pains ceased while the galvanism was applied; and returned on its removal; in the instance which I have last detailed the uterine action ceased while the galvanism was applied, and did not return upon the withdrawal of the galvanic action, nor for twenty-four hours subsequently. There was no reason whatever at the time to expect this as a probable occurrence, independently of galvanism. But even admitting, for the sake of argument, that the cessation of the uterine action was not the result of the galvanic influence used, still the fact is amply sufficient to show that the galvanic current had not, at least, the power either of increasing the pains, or of continuing and maintaining them when they offered to fail. It may be proper to add, that during the galvanic action I did not find (in any of the experiments) between the *clonic* uterine contractions or pains, any evidence whatever of unusual *tonic* contractions of the uterus, as shown either by any degree of hardness of the general uterine tumour, or by any degree of tension in the pressure of the bag of membranes, or the child's head, against the cervix uteri."

Thus the results of the foregoing thirty-two cases, in which galvanism has been used in obstetric practice, have been cited, and it appears that its effects were—

Decided in 24 cases.
Equivocal in 1 "
Negative in 7 "

So that in 75 per cent. the effects of the agent were clearly manifest:—In eight cases, for hemorrhage before expulsion of the ovum; in six cases for hemorrhage after expulsion of the ovum; in eight cases, for atony of the uterus; and in two, for induction of permature labour.

The cases recorded have occurred in the practice of eight observers. Of these, seven bear unvarying testimony to its powers, whilst one altogether doubts it. Indeed, all the cases in which the results were equivocal or nugatory were observed by him; and he has not observed a single case in which the result was satisfactory.

Hence, the facts seem to be in such strange contrast that one is almost forced to the conclusion that in Dr. Simpson's cases some undetected source of fallacy must have existed; and Dr. Golding Bird seems to entertain a somewhat similar opinion, for in his "Lectures on Galvanism" he says: "I cannot for one moment admit the validity of his (Dr. Simpson's) opinion when opposed to the facts of Dr. Radford, Dr. Lever, and others; but would endeavour to show the mode in which these opposite statements appear to admit of reconciliation. This is founded on the opposite effect of currents, according as they follow the course of the centripetal or centrifugal nerves. Now in the magneto-electric coil, in which currents are excited by repeatedly breaking contact by a vibrating bar, we have two currents moving in opposite directions, to each of which the patient who is subject to the experiment becomes submitted. Now, these currents are of an equal strength, and if the most energetic, that on breaking contact, be passed in the direction of the *vis nervosa*, it will produce painful contractions, which, the moment it passes in the opposite direction, will become relaxed; for a direct current tends to produce contraction, an inverse current, paralysis. Hence, I should urge the accoucheur not to employ the apparatus in which both these currents are produced, but simply the *single current* machine. In using this I would suggest the positive conductor to be placed over the lumbo-sacral region, and the other to be carried over the abdominal surface, with a gentle friction. If this way powerful uterine contractions may be easily excited."

I am not sufficiently master of the subject to offer any opinion as to the theoretical truth of this explanation; but, as referring to the case in question, it must fail as an explanation of the want of success in Dr. Simpson's cases, for Dr. Simpson says that "he used an instrument similar to the one used by Dr. Radford, and made by the same makers." Hence it aught to have produced the same sort of currents, whatever they were, and the same results.—*Dublin Quarterly Journal of Med. Science, Feb.* 1852.

[There can be no doubt that galvanism may be considered as a most important addition to our list of remedies in cases of flooding, either before or after delivery, or in cases of atony of the uterus, though the results are more important in cases of flooding than in simple atony. Speaking of the ergot of rye, Mr. Houghton remarks, that in the last 330 cases of labour he has attended, he has given the ergot in 38: its effects were decided in 26; equivocal in 6; and nil in 7. Although this is a mere statement from a few cases, yet it shows that the ergot failed or was equivocal in just one third of the cases; while galvanism was quite successful in three-fourths of the cases in which it was employed. Should Pulvermacher's hydro-electric chain batteries prove effectual, it will be a most convenient method of applying galvanism in obstetric practice.]

THERAPEUTICS.

NEW GALVANIC APPARATUS.—DR. PULVERMACHER'S HYDRO-ELECTRIC CHAIN.

[Dr. Pulvermacher's modification of Volta's pile has attracted so much attention that the following account of its value, by Dr. Golding Bird, cannot but be interesting.]

Everybody is aware that the apparatus contrived by Volta consisted of plates of metals, differing in their respective affinities for oxygen, alternated with pieces of cloth dipped in a saline solution. Thus, in the most common modification of this pile, a plate of copper is placed on the table, on this a plate of zinc, and then a piece of flannel or cloth, dipped in a solution of common salt; and on this a second pile of copper, and so on. The theory of the apparatus is so well known, that it is unnecessary to say more than that, under the chemical action of the saline fluid on the zinc, the combined electric fluids existing normally in both the metals employed, are separated,—the positive electricity being found in the zinc, and the positive on the copper surface. Wollaston's and Cruikshank's are but modifications of the same contrivance,—cells filled with the saline fluid replacing the moistened cloth or flannel. The cumbrous nature of those contrivances, the time required to excite them, the rapidity with which the intensity of the electric current diminishes, as well as the tact and management required to apply the current they evolve, have always presented most serious obstacles to their adoption into medical practice. On this account they have been almost completely replaced by the different machines for furnishing a current of induced electricity. These, it is true, possess many advantages, and become most important appliances in the treatment of disease, as has been repeatedly pointed out by myself and others. Still we have often felt the want of an apparatus by which a uniform and uninterrupted current of voltaic electricity could be at our command at a short notice, and without involving the necessity of any manipulative test in its application. The hydro-electric chain completely fulfils these desiderata.

The apparatus I have used was placed in my hands during last winter by Dr. Pulvermacher himself. He is a scientific man, and well acquainted with physical science generally, nor is he, I presume, responsible for the manner in which his invention has been extolled, as a sort of universal panacea, by the London agent in the public advertisements. Each element of this battery consists of a small piece of wood, around which are wound two wires, nearly, but not quite in contact, one of these wires consisting of zinc, the other of gilded copper. These represent the plates in Volta's pile: each terminates in a ring, by which it is connected with the wires of the next link or member of the chain —the zinc of one being united with the copper of the other, and so on. When one of these links is immersed in a fluid capable of exciting a chemical action on the zinc, enough is retained by capillary attraction between the fold of wire to disturb the electric equilibrium of the metals, and to throw the negative and positive fluids into a state of current. The exciting fluid recommended by Dr. Pulvermacher is common vinegar, and if one of his chains be immersed in that fluid for a minute, and then lifted out, so that all not retained by capilarity may drain off, it will be at once fit for use.

The electricity excited by this apparatus is necessarily small in quantity, as the amount of electricity evolved must be in a ratio with the intensity of the chemical actions exerted on the oxidizable metal; yet its tension is tolerably high. It is indeed sufficient, both in quantity and tension, for the developement of physiological phenomena. The following experiments will illustrate these properties, a chain of fifty alterations being employed:—

1. A thin piece of platinum wire being attached to the terminal links, they were immersed in water acidulated with sulphuric acid, and very distinct

evolutions of exceedingly minute bubbles of oxygen and hydrogen were evolved from the two wires. The dilute acid being replaced by a solution of iodide of potassium mixed with starch, iodine was almost immediately set free at the wire where the positive current entered the fluid. The quantity of these electrolytes decomposed was exceedingly small, as the electrolytic power of the evolved current would of course bear relation to the amount of effective chemical action going on in the links of the chain.

2. The platinum wires were then connected with an astatic galvanometer; the wires were immediately deviated under the influence of the current, but the latter was not sufficient to retain the needles at right angles to their normal position. The astatic galvanometer was then replaced by an ordinary one, having a coil of thirty folds of wire, and carrying a magnetic needle, five inches long. The current was barely able to produce a permanent deviation from the magnetic meridian of five degrees. This feeble action on the magnetic needle is explained by the small *quantity* of electricity circulating through the chain.

3. The chain being held in a vertical position by one end, the terminal link was allowed to touch for an instant the lower plate of a condenser, six inches in diameter, in connection with the gold-leaf electrometer. On lifting off the upper plate the gold leaves separated to an extent of a couple of inches. When only half of the chain was brought in contact with the electrometer, considerable divergence also occurred. This experiment well illustrates the comparative high tension of the evolved electricity.

4. The first and last link of the chain being placed in cups of water, and a finger of each hand being immersed respectively into the two cups, a smart shock was experienced in each finger. This shock was repeated every time one finger was *raised out* of the fluid and redipped. But no shock was felt all the time the finger *remained* immersed, as the electricity passed in a continuous stream through the body from one end of the chain to the other; the physiological phenomenon of "shock" being produced only at the moment the current first entered the body. This is of course the same with all voltaic apparatus which yield an uninterrupted current.

These experiments are sufficient to demonstrate the electrogenic power of Pulvermacher's apparatus, and to point out that the current evolved is small in quantity, but of moderately high tension.

When a continuance of sensible shocks is required, an ingenious apparatus, contrived by the inventor of the chain, may be used. This consists of a small helix of thin wire fixed in a glass tube; one end of this wire passes through a cork in the tube, and ends in a hook; the other end is free, and is barely in contact with a metallic plate (also furnished with a hook), which closes the other opening of the tube. On connecting a chain of fifty elements to each of the hooks of this apparatus, the first and last links being grasped in the hands, a rapid succession of rather violent shocks will pass through the arms. These occur in consequence of the slight motion communicated to the chain by the hands, being sufficient to make the helix vibrate, and thus rapidly approach and recede from the plate at one end of the tube.

It must not be supposed, however, that sensible shocks are required to develope physiological phenomena or therapeutic effects. We are chiefly indebted to the laborious researches of Dr. Marshall Hall for teaching us the vast amount of therapeutical influence developed by a continuous current of voltaic electricity. I cannot indeed, too strongly impress upon those who have to treat a case of old paralysis (unconnected with spasm) the vast importance of allowing a current of voltaic electricity to traverse the palsied limbs persistently for half an hour or more daily for weeks and months, nor to be disappointed at not witnessing any *immediate* good results. Nutrition of the limb is certainly thus increased, its waste and emaciation prevented, at least to some extent, and the probabilities of cure are much increased. Pulvermacher's chain, when once excited by immersion in vinegar, soon begins to evolve a current of decreasing intensity; but so long as even a small quantity of fluid remains

unevaporated between the folds of the wire, evidence of the circulation of electricity can be made out by the electrometer. A moment's re-emersion in vinegar will at once restore the energy of the current.

The advantage of this apparatus to the medical man consists in its giving him a means of obtaining a current of electricity, of amply sufficient tension and quantity for all physiological purposes, at a moment's notice. He can, moreover, diminish or increase the tension by making use of a greater or smaller number of links. He can make the current continuous or interrupted, painful or painless, at will,—and he has, moreover, an apparatus so easily managed as to require no especial tact for its application. On the other hand, it must be recollected that the current evolved has no *peculiar* properties, and that it will effect nothing more than that evolved by any other means. It is indeed, deeply to be regretted that so convenient a source of electricity runs the risk of losing favour in the sight of educated men generally, and of our profession in particular, by being injudiciously puffed in the public prints, by advertisements claiming for it a medical influence it in no wise possesses.—*Lancet, October* 25, 1851.

[The following ingenious and novel mode of applying galvanism as a remedial agent to the human body has been brought forward by M. Pulvermarcher. The apparatus was placed in the Great Exhibition.]

The apparatus is in the form of a chain battery, which may be worn continually on or around the affected part; and it differs from the other forms we have already noticed in affording a weak and almost painless current, which occasions little or no inconvenience to the wearer. The construction of the chain battery is thus described by the inventor. "In order to produce a large surface within a small space, and with little material, positive and negative wires (*of zinc and brass*) are coiled round a small lengthened piece of wood in such a matter that they run parallel to each other at very small distances, but without immediate contact. At each extremity of the wooden core, the end of one of the wires is bent into a gilt eye (the other end being fixed into the wood) so that at one extremity of the wood, the eye from the positive wire, at the other extremity that from the negative wire, project beyond the core; the whole forming the metallic part of a galvanic element, with space between the wires for the fluids. A number of such elements linked together on the principle of the voltaic pile, therefore, constitutes the metallic part and arrangement of a battery, permanently connected, flexible in all directions, of considerable surface (quantity) in proportion to its size, and of an intensity, only limited by the number of elements employed. These batteries, although so small and light, are capable of producing powerful effects, as we have ourselves experienced, and we have no doubt that all the statements made by the inventor in his prospectus are correct. Thus a powerful shock can be obtained by a battery of 120 elements charged with distilled vinegar; and when the two halves of the battery are connected by an interrupting cylinder, consisting of a spring fixed in a small glass tube, so that every motion of the instrument breaks and renews connexion, producing a vibratory current, the effect is almost insupportable, and approaches in character to the more powerful electro-magnetic machine. The inventor states that a battery of eighteen of the elements decomposes acidulated water, while one of 150 produces visible sparks with the interrupted clock-work. Another curious effect is produced by these batteries, when the clock-work apparatus just mentioned is added to the apparatus, that of exciting rather powerful muscular contractions, unaccompanied by any considerable amount of pain. We have personally tried the apparatus, and can vouch for the accuracy of the statement that, while muscular contractions were excited, little pain or other inconvenience was produced. The exciting fluid may be either water, together with the prespiration, the mildest form in which it can be

applied, which produces a "constant mild irritation, felt as a slight itching, and the production of small pimples; or vinegar, when charged with which, the battery produces a burning sensation at and near the poles. When the interrupted current is required, as in paralysed limbs, the small glass tube or interrupting cylinder may be inserted in any part of the chain, by which slight but sensible shocks are produced by every movement of the body.—*Med. Times, August* 2, 1851.

ON THE INFLUENCE OF VARIATION OF ELECTRIC TENSION AS A CAUSE OF DISEASE.

By William Craig, Esq., Ayr.

[Mr. Craig thus recapitulates the heads of the arguments he has advanced in a very interesting paper upon this subject.]

1st. That heat and electricity are identical, as the one can be converted into the other.

2nd. That a large volume of electricity surrounds every primary constituent of matter, especially that form of matter which constitutes the gaseous bodies.

3rd. That animal heat is supported by the electricity liberated from the primary constituents of matter during the processes of respiration, digestion, and assimilation.

4th. That electricity is evolved during these processes on the same principle as that which is evolved during the action of a galvanic arrangement.

5th. That electricity and nervous power are analogous, if not identical; as the section of the one can be successfully substituted for the other.*

6th. That the majority of diseases are caused by either the sudden abstraction or slow abduction of electricity from the body.

7th. That a low state of electric tension on the surface of the earth, produced either by the operation or evaporation or some occult movement in the great internal currents of the earth, is the remote cause of epidemic and pestilential diseases.

8th. That occasional and ordinary diseases are produced by the sudden abstraction or slow abduction of electricity from the body, or its undue elimination during the vital processes.

9th. That since electricity is so essential to the integrity of the vital operations, it is indispensible that measure be taken to promote its evolution and prevent over-radiation.

10th. That electricity is the source of vitality in vegetable life.

11th. That electricity is attracted by the fibres of the roots of the plants; and by the instrumentality of the electric fluid does the plant extract its constituents from the soil.

12th. That vegetables of rapid growth require a large supply of electricity to secure their perfection and completion; and the potatoe is a plant of this kind.

13th. That the disease in the potatoe was produced by want of nutrition.

14th. That the want of nutrition arose from from defective electric agency.

15th. That the cause of the deficiency of this agency was those abstracting influences which produced low tension of electricity.—*Med. Gazette, October* 10, 1851.

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* This conclusion is, in our judgment, not justified by facts. Nervous power cannot be transmitted by anything but *nerve*. Electricity may be transmitted by a variety of conductors, organic or morganic, and of these, nerve is one of the worst. Animals which evolve electricity are provided with distinct organs for this purpose. By nervous power, milk, urine, and bile are secreted into blood. Electricity, in any form, cannot produce these or similar results. They resemble each other in traversing their respective conductors with equal rapidity; but this is not sufficient to establish their identity.—*Ed. Gaz.*

SELECTED MATTER.

MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE DIAGNOSIS OF FEVERS.

[The investigations which have been carried on within the last few years, have led physicians to consider that the symptoms and post-mortem lesions of continued fever are not attributable to any variations in its character, but to the presence of two or three diseases allied as to community of character, but separated by peculiar and distinctive marks, and which being mixed up in various proportions, give to each epidemic a peculiarity according to the predominance of the existing disease. We must first examine whether this disease (continued fever) can be separated from the affections with which it has so far been confounded. The disease here alluded to has received many names, as "Seven-day Fever," "Bilious Remittent Fever," the "Mild Yellow Fever," &c. Perhaps as good a name as any, as indicating the main feature, is the one generally used, viz., the "Relapsing Fever." Let us select the main symptoms, suggesting the fundamental differencies between this and other fevers.]

Relapsing Fever affects all ages and both sexes, and perhaps in an equal ratio.—In its onset there does not appear to be anything distinctive, unless the suddenness of the accession, and the severity of the early muscular and articular pains, lead to a suspicion of the real affection. But after two or three days, the symptoms, although not absolutely distinctive, become more marked; the feverishness is considerable; the muscular pains and headache severe; and on the second or third day there are, for the most part, more or less severe pain and tenderness about the epigastrium, and vomiting; there is, however, no other abdominal tenderness, and diarrhœa is generally absent. The heat of skin is alternated both with rigors and sweating, so that the resemblance to an irregular intermittent has been noted by several writers. On the third or fourth day, the symptoms are at their height; and a typical case, that is, a case presenting the main diagnostic symptoms, and no others, can be generally known by the slightness of the head-symptoms, the chief being headache, and in a small proportion of cases (about 8 per cent.), delirium, by the absence of chest-symptoms, and by the presence of epigastric and splenic tenderness, and vomiting, engrafted on a severe pyrexial state; that is to say, a state characterised by great restlessness and sleeplessness, a hot skin, the temperature of which may rise to 107°, a white tongue, thirst, and a pulse which is seldom below 100, in more than half the cases more than 120, and in a considerable number is still higher than this, yet whose rapidity and sharpness are not indicative of commensurate danger. In a certain number of cases, as more particularly noticed below, on the third or fourth day, a peculiar tint of skin becomes perceptible; to use Dr. Cormack's expression, there is a slight "bronzing," which is most marked in the face; this appears to be the commencement of an approaching attack of jaundice, which becomes more fully declared on the fifth or sixth day of disease; the vomiting is now often severe, the matters vomited being bilious, or sometimes even coffee-ground like, or being absolutely like the black vomit of yellow fever. This jaundice is not attributable to any obstruction in the ductus communis choledochus, as bile

passes freely, and even copiously, with the stools, and as after death the gall-duct is pervious. In these yellow cases there is generally tenderness over the liver, which may be also enlarged. The spleen is also often enlarged. If the patient be now bled, the blood is often boiled, and the serum is sometimes yellow, sometimes unusually green.

A day or two after this, when every symptom appears hourly becoming graver, when the restlessness and general distress have reached their highest point, there ensues, in the majority of cases, though not in all, a most remarkable series of symptoms, followed by as remarkable an intermission of all symptoms, and an apparent restoration to health. This period has received the name of "Crisis," although it would have been very desirable if some other term than this, to which so many meanings have been given, had been chosen. For the most part, at this period, the patient falls into a profuse sweat, which lasts sometimes for thirty or thirty-six hours, but is usually shorter than this. The chemical qualities of this sweat have not yet been determined; but it has like the partial sweats which have previously occurred, a very sour and peculiar smell. When the sweat has passed off, an extraordinary change is found to have taken place; the hot skin has become cool; the quick and strong pulse is feeble and slow; the feelings of distress and discomfort have disappeared; and in severe cases are succeeded by a state of excessive languor and feebleness, as if the person had been reduced by some immense hemorrhage. After rallying from this, perfect convalescence seems to have commenced, and the yellow tinge begins to disappear, and in four or five days may have altogether vanished. This so called "crisis," is not always accompanied by sweating, a discharge of some other kind may occur in its place, such as diarrhœa, epistaxis, diuresis, or even sometimes menorrhagia.

There seems no doubt that this apparent end of the disease may be the actual one; but in a certain number of cases another phase occurs. After six or seven days of improvement, and (taking the mean) on the thirteenth, fourteenth, or fifteenth day of the disease, the fever suddenly returns. This second attack, the so called "relapse," exactly resembles, except it may be in point of severity, the primary attack; the shivering, the severe muscular and articular pains, the restlessness, discomfort, and sleeplessness, the burning skin, the rapid pulse, present themselves over again. If Jaundice has not been present in the first accession, it may appear on the second or third day of the second accession. After four or five days, the symptoms begin to improve, the feverishness abates, and, about the twentieth day of the disease, the patient is really convalescent. In an uncertain number of cases, this second accession is terminated by a "crisis" similar to that of the first.

The disease may thus terminate, either gradually or suddenly, and in the greater number of cases it does really so end. In some cases, however, he second accession is not only terminated by a crisis, but this crisis is succeeded at an interval of four or five days by another accession, which may again be followed by a crisis, intermission, and a fourth accession. No less than five of these accessions and relapses, have been known to occur.

Various sequences follow this fever, of which the most remarkable are a form of ophthalmitis (well described by Mackenzie), rheumatic-like pains, parotitis (which may also occur during the fever), anasarca, and furunculi.

The disease, whose main features we have thus imperfectly indicated, although comparatively unknown ten years ago, has been so attentively studied by British practitioners, that we are, perhaps, better able to determine the mode of succession and the variation in the symptoms, by means of numerical analysis, than in the case of any other fever.

[With regard to the mortality, it is stated that in uncomplicated cases scarcely any die. Sudden collapse may come on, and in some severe cases the patient dies from unreal poisoning. In other cases, complications, thoracic or abdominal, lead to a fatal result. In 1843, the fatal cases averaged from 3 to 6 per cent. 1846, 6·38 per cent. A very marked feature in relapsing

fever is the frequency of abortion in pregnant women, though this is not invariable.]

The post-mortem appearances in this fever need be described no farther than to say, that although in many cases there is congestion of, and in some instances extravasation of blood into and beneath, the mucous membrane of the stomach, and in a less degree of the intestines, yet Peyer's patches remain without deposition and ulceration. The spleen is generally enlarged, and according to Jenner, this occurs more frequently and to a greater extent than in any other form of fever. Robertson observed also some kind of deposit in the spleen, which he could not identify with the typhoid exudation, but which, like it, underwent a process of softening.

This detail of symptoms must be sufficient to convince any one of the difference betwee this and other forms of fever. To take only the most striking symptom, the relapse, it appears that this is hardly known except in this disease. In "many hundred" cases (1600 to 2000) of typhus, Henderson has never known a relapse; and Jenner, in his extensive field of operation, has also never yet seen such an occurrence in typhus. In the fever, or variety of fever, termed typhoid, or Dothnententis, relapse, as noted by Stewart and Jenner, will occur, but it is exceedingly rare. The majority of the so called relapses in typhus and typhoid fevers, are simply sudden superventions of some complication, or a sudden exacerbation of some previously existing complication, as pneumonia, pleurisy, &a. In 1145 cases of typhus, treated by Perry, in Glasgow, there were nineteen of these so called relapses, which were all traced to some local inflammatory action. But putting the relapse aside, the course of the remaining symptoms is completely dissimilar to that of other fevers; and the absence of the cutaneous eruptions common to other forms is also a strong proof of their non-identity. When to these facts we add the invariable absence of the anatomical sign of Dothinenteritis, viz. the affection of Peyer's patches, the argument against the identity of relapsing fever, and the disease described by Louis, becomes most absolute; and though the anatomical signs of exanthematic typhus are not so definite, still there are perceptible differences in this case also.

It is therefore not surprising, that the Scotch physicians, who have described the epidemics of 1813 and 1817, should have so unahimously decided on the specific nature of this fever; and the evidence in favour of this view, will, we think, appear to every one sufficiently exact.

But, in addition, another most convincing argument in favour of the same opinion has been recently brought forward by Dr. Jenner, which proves that cases of other forms of fever do not give rise to relapsing fever, and that exposure to relapsing fever gives rise only to similar disease, and not to another form of fever.

Before passing from the subject of relapsing fever, we may remark, that it can probably return several times in the same subject, and even at intervals of some few months only. In this, also, it shows a remarkable variation from the other Engli. fevers.

Relapsing fever appears to predispose to typhus fever, and to be also predisposed to, by typhus. Steele remarks, indeed, that patients who had formerly suffered from typhus, "enjoyed the immunity from this disease;" but this is contrary to the direct and positive evidence of many other observers.

Contagion, in some instances at least, is admitted by all, except Dr. Craigie, who thinks that "although it is, perhaps, contagious, this is rather a presumption than a well-founded inference." The observations of Douglas and Jenner, however, seem conclusive on this point.

Like other fevers, this disease is influenced in an extraordinary degree by the sanitary condition of the population attacked by it. This has been traced out in Glasgow, with care, by David Smith. Wardell says, this fever "was importantly connected with destitution."

Having thus separated Relapsing Fever from the disease, which it has

been customary, in this country, of late years, to call "continued fever," and having shown that the opinion of those who have considered it "a fever, *sui generis*," is justified by its strongly-marked and peculiar symptoms, by its post-mortem characters, and by its not arising (as more fully explained elsewhere) from the causes of the other continued fevers, we are prepared to enter on a further inquiry, the object of which we may present in the following question:—

II. *Is the disease, which, with relapsing fever occasionally added to it, formed the affection termed in this country "continued fever," a single disease, or have two or more affections been included here also under a single term?*

[Dr. Jenner entered systematically into this inquiry in the London Fever Hospital. It has been long known here, that cases of fever with deposit in and under Peyer's patches, and in the mesenteric glands, were common; and so were cases of fever without any trace of this deposit. Dr. Jenner patiently accumulated nearly 2600 acurate reports. He first separated the cases of relapsing fever, and then instituted a rigorous comparison of the rest.]

It is necessary, however, to state his method more fully, as it appears to us the only one which can possibly solve the question, and it is a model of close observation and logical induction. From the great number of histories of fever-patients he possessed, he selected the fatal cases which had been examined, and the diagnosis of which, therefore, had been confirmed. He found that he had sixty-six such cases and post-mortem examinations. Of these sixty-six, twenty-three had the intestinal and mesenteric lesion, which Louis says is the anatomical sign of typhoid fever and forty-three were without this appearance. Now, if Louis' doctrine be correct, that no case is typhoid fever unless it presents this appearance, it was to be seen, whether the forty-three cases in which the intestinal lesion did not occur, differed so much in symptoms and other post-mortem appearances, from those in which it did occur, as to render it impossible to suppose that they were the same disease; or whether, contrary to Louis' opinion, the symptoms were so similar as to lead to the belief that the presence or absence of intestinal lesion was a matter of little consequence. Accordingly Dr. Jenner took these two groups and compared them throughout, and found that while the symptoms and post-mortem appearances of the twenty-three cases were exactly the same as those described by Louis in his great work, the symptoms and post-mortem appearances of the other forty-three cases were entirely different, so different, indeed, as to render their separation from the other cases a matter of absolute necessity, if any certainty was to be introduced into the description of these diseases, and into their treatment. The disease which affected the twenty-three patients, he called after Louis, typhoid fever, and to the other affection he gave the name of typhus. Between these two diseases, no transition forms could be observed. The results arrived at by Jenner agree remarkably with those of Gerhard, Stewart, and others.

If we collate the chief works which have been written on the two diseases by those who have had a full persuasion of their distinctness, we find that typhus and typhoid fevers are said to differ:

1. In the *age* of the patients they affect. Typhus affects all ages young and old; typhoid, chiefly persons under forty; it will affect older persons, but with difficulty.

2. In their *modes of attack*. Typhus being sudden, typhoid insidious, as a general rule.

3. In their *duration*. Typhus fever is of much shorter duration than typhoid, as has been noted by Gerhard, Stewart, and Jenner. The average duration of Jenner's fatal cases of typhus was fourteen days; of Reid's (113 cases) thirteen days; of the typhoid cases, twenty-two days. In the cases of recovery the difference is just as well marked. The average of 255 typhoid cases noticed by Jackson in Massachusetts was twenty-two days. Reid's average, in the

same locality, was thirty-nine days. Jenners average is from twenty-one to thirty days. The average of non-fatal cases of typhus appears to be much less than this. Jenner states that after twenty-one days, local lesions sufficient to cause death were always discovered in typhus, that is to say, that after this date, death did not occur *from the fever alone*, as may be the case before the twenty-first day. He states the average duration to be from fourteen to twenty-one days; but not infrequently, in very mild cases, it terminates before the fourteenth day, in the same way as mild cases of scarlatina will cease before the average time arrives at which the fever is usually held to terminate.

4. *In the kind of eruption.* Nothing can be more distinct than the repeated scanty crops of rose-spots in typhoid fever, with their bright colour, their disappearance under pressure, and their duration of three or four days, compared with the permanent, dark red, or mulberry coloured, ineffaceable copious rash of typhus. Mistakes sometimes arise, however, from the typhus eruption being seen on the second or third day of its appearance, at which time it disappears under pressure, as was noticed forty years ago by Wedemeyer, in his account of exanthematic typhus.

5. *In the colour of the skin, the expression of the face, and in manner.* Typhus patients often present in its highest degree those characters which the old writers often termed "oppression" and "prostration;" the face is darkly and generally flushed, the complexion muddy, (particularly after the sixth day,) the manner stupid and confused, and the eyes unintelligent. On the contrary, in typhoid fever the complexion does not get muddy, except to a very slight degree; consequently the flush of the cheeks, when present, is bright and pinkish, and not dark red, and it is often circumscribed to the cheeks, and then is strongly contrasted with the surrounding pale skin. The manner, also, is often natural, or even a little sharp, provided there be no delirium. The differences are marked, even in the slightest cases of typhus the muddiness and flushing may be insignificant compared with the severe cases. In relapsing fever, the complexion is clear, or has a slight yellow or "bronzed look." (Cormack.) It is very conceivable that the peculiar complexion of typhus has not always obtained the attention it merits, on account of the confusion of cases typhoid and relapsing fever, in which this complexion is not seen.

6. *In the severity and course of the head symptoms. Headache* is an almost constant symptom in each. Although there is considerable variation in individual cases, yet on throwing large numbers together, it becomes apparent that both in typhus and typhoid fevers, the *headache* has a determined duration. In typhus it ceases usually on the tenth day, and always on the fourteenth day; in typhoid fever, about four or six days later, and may last till near the end of the third week. *Delirium* commences earlier in typhus than in typhoid, by several days. In Jenner's fatal cases, it was more *active* in typhoid; the patients were more vivacious, and disposed to leave their beds. *Somnolence*, although frequently absent in both, is more common and earlier in typhus than typhoid. The peculiar symptom which has been appropriately called "*coma-vigil*," and in which, as Jenner defines it, "the patient lies with his eyes open, evidently awake, but indifferent or insensible to all going on around him," occurred in one-fifth of his fatal cases of typhus, but in none of the typhoid patients.

7. *In the degree of loss of muscular power.* Typhus patients almost always take earlier to their beds, and are more completely prostrated at an earlier date than typhoid cases. This is well illustrated by Jenner's cases, as here typhus and typhoid cases were lying side by side in the same wards.

8. *In the frequency of epistaxis*, which is very rare indeed in typhus, rather common in typhoid, (one-third of Jenner's available fatal cases; nearly half of Louis'.)

9. *In the condition of the eyes.* In typhus fever the conjunctivæ are generally injected, and the pupils contracted; in typhoid fever, the conjuctivæ are pale, and the pupils larger than natural.

10. *In the state of the tongue,* which is drier, browner, and larger in typhus; is more frequently small, fissured, red, or partially covered with a pale-brown fur in typhoid.

11. *In the chest symptoms.* Sonorous rhonchi are very frequent in typhoid; rare, comparatively, in typhus. Dulness of the depending portions of the lungs, (a little above the basis,) depending on hypostatic congestion, is common in typhus, rare in typhoid.

12. *In the state of the pulse,* which is much more varible in typhoid than in typhus fever.

13. *In the abdominal symptoms.* The abdomen is *painful* on pressure, in about three-fourths of typhoid patients: is almost always quite painless in typhus, or if painful, this is slight and transient. *Gurgling* exists in perhaps a fourth of the typhoid cases; in one fortieth of the typhus. The abdomen is *distended* and *resonant*, more or less in almost all cases of typhoid; it is, with scarcely an exception, natural in shape, or even a little concave, in typhus. *Diarrhœa* exists as a rule in typhoid, as an exception in typhus. *Intestinal hæmorrhage* occurs in one third of fatal cases of typhoid: in no case of typhus without dysentery. The *discharges from the bowels* are different in the two diseases; in typhoid they are loose, watery, fawn or dark brown colour, alkaline from fixed alkalies, and contain a large proportion of soluble salts, and a small quantity of albumen. In typhus they are generally solid, often acid, or if alkaline, are so probably from ammonia, and in most cases do not appear altered from health, unless medicines have been taken. Although diarrhœa, meteorism, and abdominal tenderness are the rule in typhoid fever, it must not be supposed that they are always present. In some of the worst cases, the first abdominal symptoms may be announced by peritonitis, consequent on perforation. But when vast numbers are collected, these exceptional cases are lost in the large proportion of those in which these symptoms exists in greater or less intensity.

14. *In the occurrence of epiphenomena and of sequences.* Sloughing from pressure is equally common in both diseases; but erysipelas, phlebitis, and local inflammations and ulcerations are much more common in typhoid fever. So also tubercular deposition in the lungs is decidedly more frequent as a sequence of this disease than of typhus.

15. *In the continuance of the eruption after death.* The spots of typhus last uneffaceably after death; the rose spots of typhus cannot be found.

16. *In the duration of cadaveric rigidity* which ceases more quickly in typhus than typhoid cases, according to Jenner's interesting observations.

17. *In the more rapid dissolution* (so to speak) *of the tissues* in typhus than in typhoid. As an instance of this, it appears from Jenner's researches that the epithelium detatches itself very rapidly indeed from the basement membrane, a fact which is well seen when a microscope section of the kidneys of typhoid and typhus patients are examined, or when the surface of the œsophagus is observed. To the same class of facts must be referred the abnormal facility with which, in typhus patients, the pia mater and arachnoid separate from the surface of the brain.

18. *In the frequency of the occurrence of hemorrhage into the arachnoid* in typus, which occurred in one-eight of Jenner's fatal cases, while it was not found in one of his typhoid cases, nor in any of Louis' or Chomel's cases. The amount of intracervical serosity is also decidedly greater in typhus.

19. *In the frequency of ulcerated mucous membranes in typhoid fever, and the rarity of ulceration in typhus.* In typhoid fever ulcerations in the pharynx exist in about one-third of the cases; but in typhus such lesion is never found, or is excessively rare. In typhoid, ulceration of the larynx and of the œsophagus occurs once in every 15th case; in typhus, ulceration of the larynx happens once in every 26th case; ulceration of the œsophagus very seldom indeed. In typhoid the gall-bladder and the urinary bladder occasionally participate in this ulcerative tendency; in typhus they are never attacked, or very rarely

indeed. So also the mucous membrane of the large intestine suffers frequently (in seven of 20 cases, Jenner,) in typhoid; but scarcely ever in typhus, unless there be concurrent dysentery, which is a composite disease, and distinguished without difficulty.

20. *In the concurrence of a peculiar exudation into and under the patches of Peyer, and the mesenteric glands in typhoid fever.* This is constant in all cases which present the symptoms of typhoid fever; it is never absent, although its amount varies greatly in intensity. After the publication of Louis' work, Andral, Chomel, and others, observed cases which they thought were typhoid fever, without the anatomical sign; but these cases did not bear examination, and subsequent experience has proved that this lesion is constant. It never occurs in typhus fever.

21. *In the greater softness and flabbiness of the muscular tissue of the heart,* in typhus than in typhoid.

22. *In the frequency of lobular and lobar pneumonia in typhoid fever, and the rarity of those local inflammations in typhus.* In typhus consolidation, or perhaps we should rather say, carnification of the depending portions of the lungs from congestion, is more common, but true inflammation is rare.

23. *In the more frequent occurrence of pleurisy in typhoid fever,* (40 per cent. of fatal cases, Jenner,) than in typhus (55 per cent. of fatal cases.)

24. *In the degree of softness of the spleen,* which is greater in typhoid than in typhus.

25 *In mortality*, that of typhoid being decidedly greater than typhus.

In addition to those differences, there is some evidence of different constitutions of the blood; but as the the data are not very exact, we pass them over.

It is also possible that other differences may hereafter be indicated; thus comparative observations have not been made on the urine with sufficient care i the relative temperature of the two fevers, and other points of the like kind, have yet to be considered.

Such is the statement of differencies which we have been able to glean from the writings on the subject. Although some of the distinctions may appear slight and trivial, yet others are not so. Thus among symptoms, the absolute diversity of the eruptions and state of the skin, the differences in the duration of the disease, in the mode of onset, and in the pronounced abdominal symptoms of one, with the absence of these in the other, are very marked. So also among post-mortem lesions, the entero-mesenteric disease, the tendency to ulcerations of mucous membranes, and to local inflamations, of typhoid fever, are strongly contrasted with the absence of pronounced local changes in typhus, and with the epithelial separation, apparently from prior injury to structure, which is so marked in that disease. So great are these differences, that we do not hesitate to affirm, if any one will take two confessedly distinct, yet somewhat kindred diseases, such as measels and scarlet fever, and will compare them in the way we have compared these continued fevers, he will not find a stronger case made out for their separation, than for that of typhus and typhoid fevers.

Jenner concludes his comparison in the following way:—

"At the commencement of this analysis, I proposed to examine whether typhoid fever and typhus fever differed from each other in the same way as smallpox and scarlet fever differed from each other; and for the purpose of comparison, I laid down certain grounds, as those on which we founded our belief in the non-identity of the two last named diseases. Those grounds were:—

"1st. In the vast majority of cases the general symptoms differ; i. e. of smallpox and scarlet fever.

"This holds equally true with respect to the general symptoms of typhoid and typhus fevers.

"2d. The eruptions, the diagnostic characters, *if present*, are never identical, i. e. in smallpox and scarlet fever.

"The particulars detailed in the foregoing papers prove that this is as true of the eruptions of typhus and typhoid fever, as those of smallpox and scarlet fever.

"3d. The anatomical character of smallpox is never seen in scarlet fever.

"Just in the same way, the anatomical character of typhoid fever, i. e. lesion of Peyer's patches and the mesenteric glands, is never seen in typhus fever.

"4th. Both, i. e. smallpox and scarlet fever, being contagious diseases, the one by no combination of individual peculiarities, atmospheric variation, epidemic constitutions, can give rise to the other."

III. *Are these differences always well founded, or is it not possible that, existing in well marked cases, they may yet not be constant, but may disappear in some transition cases?*

The assertors of the non-identity state that they have never been able to find any transition forms. Jenner, in London, during the last four years, has seen 2000 cases all of which could be referred to one or the other form of fever without difficulty. Gerhard and Bartlett, and the other writers on the same side, assert the same thing.

Similar evidence is to be found in that vast mass of instructive matter, which the zeal of the editor of the "Dublin Medical Journal," has collected. In the history of the terrible Irish fevers of 1846-47 and 48, contained in that excellent periodical, we find the most undoubted evidence, that exanthematic typhus and relapsing fever were the two great scourges, and that typhoid fever, dysentary, and scurvey were here and there intermixed. Frequently, when the writers have been little aware of it, they have given the strongest proofs of the existence of several intercurrent forms of fever.

It should be also remembered that the positive evidence in favour of the non-identity has been gradually accumulating for several years. *Every one who has paid a tension to the subject* has adopted the same conclusion. And that the differences between the two diseases are not dependent on any variety of epidemic constitution present in one year, and not in another, is proved by the length of time over which the observations run; Stewart's, Shattuck's and Jenner's paper make up an aggregate period of observation of nearly fifteen years, during which time every one who has attended to the point has recognised these distinctions. In America, also, Gerhad's observations were made sixteen years ago; and every year since that time has more firmly convinced him and other American observers of their accuracy.

When to this strong argument, that the positive evidence is (with one probably unimportant exception) all on one side,—and when, in addition to the fact that Pringle and Huxham, who were so familiar with fevers, distinguished two forms, which it is almost certain were the typhus and typhoid of our own days, and that Armstrong also, more lately, has observed and described two fevers, and that since Louis fixed the symptoms and the post-mortem lessions of typhoid fever, the evidence in favour of the correctness of his views; and consequently of the specific difference between it and other fevers, has been constantly augmenting,—when to all these arguments we are able to add, that if we admit these several formes of fever, the discrepancies between the observations of different countries disappear, order is introduced into this intricate and perplexed subject, and light thrown suddenly on its dark and obscure outlines,—we are strongly impelled to admit at once the truth of Jenner's conclusions, and to separate typhus, typhoid, and relapsing fever, as completely as we do smallpox and scarlet fever.

To the reasons we have above adduced for adopting the doctrine of a diversity of fever, we are able to add one more, viz., the fact that fever of one form always gives rise, as far as observation at present goes, to a similar disease, and to no other. It was noticed by Henderson, in Endinburgh, that typhus did not arise from intercourse with persons affected with relapsing fever.

His conclusion is, "that in not a single instance has the typhus fever presented itself in circumstances that warrant the opinion that it must have been produced by the contagion of the epidemic fever. (Op. cit. p. 218.) The same question has been elaborately argued by Jenner. Typhoid fever produces a like disease, but not typhus or relapsing fever; and each of these two latter diseases produces its own kind, but no other.

We may, therefore, state our answer to the proposed question in the following terms:—The facts adduced in evidence of the specific differences of typhus and typhoid fever are sufficient, if they be hereafter proved universal; there is no evidence for the identity of the three diseases, which can be at all compared, in point of precision and extent to that which goes to prove their non-identity; and it is, therefore, excessively probable, strange as it may seem, that the progress of inquiry will soon enable us to decide positively that in the *fever* of Great Britain, diseases the most diverse have been bound together by the enthralling yoke of a simple name.—*Brit. and For. Med. Chirurg. Review, July* 1851.

SURGERY.

CALCULUS IN THE BLADDER OF A GIRL.—DILATATION OF THE URETHRA.—LITHOTRITY.—EXTRACTION BY SEVERAL OPERATIONS OF THE FRAGMENTS.—RECOVERY.

A pale, sallow-looking, intelligent, and anxious girl, aged 10, came under my care in December last, with symptoms of stone in the bladder. Frequent micturition during the day, with great and acute pain; tenesmus, with occasional involuntary passage of fæces; and incontinence of urine during the night, and occasionally during the day, were among the most prominent. Her mother stated that the child had always been very delicate, had had two or three attacks of inflammation of the lungs, and that about two years and a half before she brought her to the hospital, had complained of great pain in passing water, and by degrees lost control over the bladder.

A calculus was detected by the female sound, which gave evidence of the stone being very large. The urine was found loaded with mucus, of a light specific gravity, destitute of albumen, but containing a large number of the crystals of the triple phosphate. The skin was hot; the pulse quick and irritable; the tongue loaded, and the bowels rather constipated. She was placed on milk diet and a chop. A mucilaginous alkaline mixture, and an occasional dose of castor oil with five minims of tincture of opium, and a warm bath every morning, were prescribed.

This treatment was pursued for a week after her admission, with much relief to the general and local symptoms.

Dilatation by Weiss's two-bladed dilator was now commenced, and the instrument was used three times; the first time being kept in the urethra one hour and a half; the second, on the day following, the same length of time; and the third, two days after, about half an hour only, as it gave rise to considerable pain. At the next dilatation, after the interval of three days, the three-bladed instrument was used, and was had recourse to every second or third day for the space of a fortnight, chloroform having been exhibited each time before its introduction into the bladder, and its action on the system having been kept up for about ten minutes, so that when the child recovered, the instrument was removed, and she no longer complained of the pain, which had been very severe on the first occasion, when the anæsthesia had not been induced. A day or two after the last dilatation is was observed, that at the lower and front part of the urethra there was a slight slit. After the several applications of the

instrument, and when the child had fully rallied from the effects of the chloroform, she was placed in a warm bath. After the dilator had been thus used, and fifteen days subsequent to the commencement of this treatment, the index finger could be introduced into the bladder, and it was imagined that the urethra was sufficiently dilated to admit of the removal of the calculus. It was seized, consequently, with a pair of polypus forceps; but, after three careful attempts, could not be extracted in consequence of its great bulk. A good deal of constitutional disturbance supervened on the attempt at removal, and nothing further was done till it had subsided. Twelve days afterwards the patient was placed on the operating table, and, when under the influence of chloroform, a common lithotrite was introduced into the bladder, but owing to the urethra having been so much dilated previously, the urine came away after the introduction of the instrument, and it was found from the contracted condition of the bladder, and the difficulty of moving about the lithotrite, impracticable to seize the calculus. The lithotrite was withdrawn, and a pair of strong, small lithotomy forceps introduced; the stone was seized, broken up into several fragments, a few of which were removed. The remainder were eventually taken out at intervals of a week, four operations being required, and chloroform being used at all, and the bladder immediately after being washed out with warm water. The treatment adopted after and preceding the removal of the fragments, consisted of warm bath, diluent drinks, and diuretics. Incontinence of urine continued day and night for three weeks, at the end of which time the diurnal incontinence had subsided, but the nocturnal continued. The child was sent out of the hospital thirteen weeks after its admission, much improved in health the symptoms of calculus having entirely disappeared. She was brought to me as an out-patient about a month afterwards, and I prescribed for her the same medicine she had been taking for some time while under treatment in the hospital, sarsaparilla and nitric acid. The incontinence at night time, her mother informed me, was now only occasional.

The stone, when dried, weighed five drachms, was composed of the fusible calculus, and a little organic matter; and gave no trace of lithic acid; and occupied the third of a wide-mouthed ounce phial.

CALCULUS IN THE BLADDER OF A GIRL.—DILATATION OF THE URETHRA.—LITHOTRITY.—SLIGHT INCISION SUBSEQUENTLY.—EXTRACTION OF THE FRAGMENTS AT DIFFERENT TIMES.—RECOVERY.

Case 2.—A workhouse girl, of ruddy complexion, red hair, and apparently in good health, came under the care of Mr. Adams, on the 20th of last January. She suffered from frequent micturition, followed by great pain, accompanied by occasional and sudden stoppage of the flow of urine. She had never passed blood; but the water was loaded with mucus and a thick reddish sediment. Previously to her admission she had been treated for inflammation of the kidneys, blisters having been applied to the lumbar region with temporary benefit.

She had had scarlet fever four years ago; and her mother stated, that it was only six weeks before the child came into the hospital; that she complained of pain after passing her urine, and a sudden and frequent desire to do so.

Dilatation of the urethra by Weiss's three-bladed instrument was had recourse to about a week after the patient came into the hospital, and was repeated every fourth or fifth day. After five applications of the instrument (the three last having been made while the child was under the influence of chloroform) the point of the index finger could be introduced into the bladder; but, on the calculus being seized with a pair of lithotomy forceps, it was found impossible, from its large size, to extract it. The child was placed on the operating-table, and chloroform given; the stone was seized by a pair of strong

forceps, and several fragments broken off. The bladder was afterwards well washed out with warm water. A good deal of hæmorrhrge followed, and it was deemed expedient to defer chrushing the entire stone to a future period. Great pain in the side and stomach came on during the evening, and in the urethra during the passage of the urine; and there was complete incontinence during the night. A warm bath, a dose of castor-oil, and a mucilaginous mixture, with soda, was ordered. Under this treatment, the pain in the side and stomach gradually declined, and the incontinance of urine during the night became less. A week after the first operation a second attempt was made, and some small fragment extracted. The entire calculus was ultimately removed at three subsequent operations, a good deal of bleeding attending and succeeding each. At the last operation, the largest fragment (which could not be broken with the forceps by which it had been seized) was taken away, the urethra having been slightly incised on its left and front part prior to extraction. This last portion of the stone appeared to form its main bulk, and contained a nucleus of lithate of ammonia. The remainder of the calculus was composed of lithic acid, with a circumference of the triple phosphate.

Incontinence of urine lasted six days only; and the child left the hospital in the middle of April, having been under treatment about three months.

CALCULUS IN THE BLADDER OF A WOMAN.—INCISION OF THE URETHRA.—RECOVERY.

A weaveress, aged 48, the mother of twelve children, came under the care of Mr. Adams in the middle of April. She complained of great bearing-down pain, frequent desire to micturate, the urine passing with great difficulty and in small quantity, and being occasionally tinged with blood. At times she could not urinate except in a recumbent position. The urine was highly offensive and ammoniacal. The symptoms of stone in the bladder had been of sixteen months' duration, and were preceded by cessation of the menstrual discharge for nine weeks. After this, she had fever for three weeks, and then a miscarriage.

One or two attempts were made at dilatation, but owing to the subsequent pain,—which was excruciating,—were not further persisted in. The symptoms, on admission, gradually increased in intensity, notwithstanding the recumbent position, opiates, warm baths, etc. About ten days after her admission, the patient was placed under the influence of chloroform; the calculus was seized with a pair of strong forceps, and drawn to the neck of the bladder; an incision of the latter and the urethra was made downwards and outwards on the left side, parallel to the descending ramus of the pubis. The calculus was then drawn forwards, and extracted after a little careful traction had been exerted.

Immediate relief followed the operation. Perfect incontinence of urine lasted four or five days, at the end of which time she could hold a little, and at the eight day could retain it for three quarters of an hour. On the twelth day she could hold it for an hour an a half. The incontinence gradually declined, and when she left the hospital, on the thirty-fifth day after her admission, she had perfect control over the bladder; the urine, which had been thick and loaded with mucus, was nearly clear.

The calculus was made up entirely of the triple phosphate, and was equal in size to a large chesnut.

Remarks.—Experience appears to show, that the removal of a calculus from the female bladder, after the neck of the latter and the canal of the urethra have been gradually dilated by forceps constructed for the purpose, sponge tents or otherwise, is a preferable proceeding to its removal after the urethra has been incised. It also demonstrates, that dilatation can be carried to an extent which, in the absence of well-authenticated facts, would appear

almost incredible. The most remarkable case among those recorded, with which I am acquainted, is one related in the first volume of the "Medico-Chirurgical Transaction." It occurred in the practice of Mr. Oaks, of Cambridge. The patient was eleven years old. Sponge tents, gradually increased in size with string attached, were introduced into the urethra, opium, preceded by purging, being administered. The sponge was used morning and evening, and for three successive days. The urethra was sufficiently dilated, on the third day, to allow of the calculus being withdrawn. It is stated, that the stone measured in circumference, at its major axis, $3\frac{3}{4}$ inches; in its minor, $3\frac{1}{8}$ inches. The calculus was seized in its long axis, and therefore the urethra must have been distended by the calculus and the thickness of the forceps to a circle of at least $3\frac{1}{4}$. Incontinence of urine lasted only three days.

This plan of treatment by dilatation is in imitation of that made use of in the natural efforts to get rid of a calculus from the female bladder, the stone itself acting as the dilating power; and numerous remarkable instances are on record of calculi thus voided, without any subsequent incontinence of urine. In far the greater number of cases, however, in which the calculi have been large, incontinence of urine, which was a marked antecedent symptom, has persisted; this, probably, arising from ulceration at the neck of the bladder, in consequence of the pressure of the foreign body. Thus, there is in the London Hospital Museum a calculus which was removed from a woman by the late Mr. Headington. Its anterior extremity was found sticking in the urethra, and the entire stone was removed easily by traction of the two index fingers. It measured $3\frac{1}{8}$ inches long, 2 inches broad, $1\frac{7}{8}$ inch thick, $7\frac{1}{4}$ inches round its larger, and $5\frac{1}{4}$ inches in its smaller circumference. Incontinence of urine lasted till death. The fact of incapability of retaining the urine usually following the natural method of expulsion, when the calculus has been large, suggests the propriety of operating without any unnecessary delay, in order to avoid this inconvenient and distressing sequence.

In the treatment of the first case, it was contemplated to remove the calculus entire, but, as the urethra had given way slightly at lower and anterior part after the last dilatation, it was deemed prudent to desist from further extension, of the canal; and to crush the stone, and remove it piecemeal, which was easily effected at different operations. It might have been surmised, that the presence of the fragments in the bladder would have given rise to such an amount of irritation as to call for their removal at one operation. This was not found to be the case; and it was considered that the aggregate amount of irritation was not so great as would in all probability have taken place had all the fragments been taken away at one operation—a proceeding which would have involved much time, and in which, from the frequent application of the instrument, for extraction, an amount of injury might have been induced, which would not, in all probability, have been well tolerated.

In no case did the chloroform appear of more eminent service; for, without this agent, the pain induced, and the restlessness of the patient in consequence, would have rendered the process of dilitation, and the subsequent operations for extracting the fragments, much more difficult than they were found to be.

The second case is interesting, inasmuch as the operation for removal of the calculus consisted partly of the dilatation of the urethra, and partly of incision. In consequence of dilatation having been had recourse to in the first instance, the incision subsequently practised was but slight and limited in extent, and incontinence of urine continued only a few days.

In the third case, the symptoms were of that aggravated character, both locally and constitutionally, that it was considered imprudent to wait during the time requisite for the gradual dilatation of the urethra; and an incision of that canal was made. This incision was as limited in extent as possible, the stone having been previously brought to the neck of the bladder, and the latter having been rendered tense prior to the incision of it and of the urethra.

CALCULUS IN THE BLADDER OF A BOY.—LITHOTOMY WITH KEY'S STRAIGHT STAFF AND SCALPEL.—RECOVERY.

A child, aged four, not apparently out of health, came under my care at the end of March.

The symtoms of calculus, according to the mother's account, had only lasted a fortnight. There was great pain in passing the urine, preceeded by tenesmus and the occasional passage of fæces, etc.

The child had been delicate in health since its birth, and had pneumonia when five months old, lasting five weeks. A calculus, apparently small, was detected in the bladder. He was kept quiet in bed for about a fortnight, on a generous diet without stimulus, and ordered a warm bath every morning; and the bowels were regulated by castor oil. The severity of the symptoms having declined under this treatment, the operation for extraction was performed. The child was placed on the operating-table, and a curved sound was introduced into the bladder. The calculus was at once detected. (On several former occasions it could not be made out, owing to its small size.) The curved sound was then withdrawn, the bladder injected, and Key's staff introduced. The patient was then placed in the ordinary position, and the operation conducted in the manner recommended by Mr. Key. The staff was held by an assistant, with the handle slightly inclined towards the left side. The external incision of the usual extent was then made with Key's scalpel, until the groove in the staff was opened; the point of the scalpel being kept steadily against the groove, the handle of the staff was taken hold of by the left hand, and the instrument depressed, the right hand being kept fixed. By a simultaneous movement of both hands, the groove of the director and the edge of the knife were turned obliquely towards the left side. The left hand keeping the staff fixed, the scalpel was passed along the groove into the bladder; and owing to the blade of the knife having been kept at a very acute angle with the staff, while being carried into the bladder, a very small incision of the prostate was made. The knife was then withdrawn along the director, the staff was taken in the right hand, and the fore-finger of the left passed along the director through the opening of the prostate; the director was withdrawn and exchanged for the forceps, which was passed along the finger into the bladder. The calculus was seized and withdrawn; it was about the size of a horse-bean.

The boy went on well till the 8th day, when the wound looked rather pouting and sloughy on the surface; the tongue was loaded, skin hot, and pulse quick. These symptoms subsided, after a warm bath and the free action of the bowels. The urine began to flow by the urethra on the ninth day, and perinæal wound had cicatrised seven weeks after the operation.

MIDWIFERY.

REPORTS OF PRACTICE ILLUSTRATIVE OF THE DIAGNOSIS, TREATMENT, AND PATHOLOGY OF OVARIAN TUMORS.

By Frederick Bird, M.D.

In addition to the aid afforded to the diagnosis of ovarian tumours by percussion, that furnished by the respiratory movements is often of much value. If visual examination of the abdomen be carefully made while the patient is in the recumbent posture, it may be observed, that on the completion of each expiration, the outline of the tumour, especially at its fundus, can be traced with more or less facility, being more evident in thin persons, and aways more remarkable when the abdomen is viewed in profile. In those who have become thin, the attenuated parietes of the abdomen fall upon and appear to compress

the diseased growth beneath, leaving a curved depression or sulcus at the epigastric and lateral regions, describing often very accurately the ovoid figure of the tumour. During inspiration, the hollow viscera of the abdomen are pressed downwards by the descending diaphragm, and for a time replace the epigastric depression observed on expiration, by marked and prominent fulness. When the tumor do not much exceed the size of the uterus at the full term of gestation, and the patient is emaciated, as under such advanced state of disease she generally is, this method of examination is of much advantage. It is only in instances of extreme abdominal distention, or concomitant obesity, that it fails to be of practical utility.

Shape, thus determined by percussion and respiration, may be confirmed by further inspection. The abdomen presents an unequal distension, principally affecting its central regions, and leaving the lateral boundaries much less affected than in other diseases with which ovarian tumour is sometimes confounded. The lumbar regions are never much distended, and often present a remarkable flatness when compared with the evident distension of the central portions, especially of the umbilical space, and its protruded centre, which, full and prominent, gives to the abdominal enlargement a defined and circumscribed form, and assists materially in the diagnosis between ovarian tumor and other diseases of the abdomen characterised by the formation of fluid. For in ascites, with which ovarian disease is perhaps most commonly confounded, the umbilical region is not more prominent, but often less distended than other parts, and generally presents a somewhat flattened appearance: while the dissimilarity to ovarian distention is still further shown by the fulness and bulging of the lateral regions. And thus, if a more exact examination be made by measurement, it will be found, that a line drawn from the superior spinous process of the ilium to the false ribs would scarcely be curved in ovarian tumour; while in equal distension from ascites the curve would be considerable. As an extension of this means of investigation, it may be observed, that the central antero-posterior measurements, taken in the recumbent posture, will bear a different ratio to other measurements in each of the two diseases. Thus, if the space between the umbilicus and corresponding portion of the spinal column be taken, and next, that between the fourth lumbar vertebra and opposite point in the mesian line anteriorly, the ratio in such measurements will show great disparity, while, in ascites, they will scarcely differ.

As ovarian tumours, whether of the more simple and fluid forms, or of the condensed and compound type, are formed primarily of a containing sac, rendered tense by accumulated cystic secretion, the results of manipulatory examination or palpation, seldom vary materially, but commonly evidence uniform hardness, combined with elasticity; there is not the unyielding hardness of a fibrous growth, but a resilient hardness. When the tumor consists chiefly of one large cyst, the elasticity is equal in all parts; but, in the case of more compound growths, where condensed masses of secondary cysts are attached to the walls of the primary sac, the sensation of elasticity is lost over the parts corresponding to the seat of such formations, and becomes replaced by inelastic hardness. In the frequent presence of fluctuation, as the physical result of contained fluid, an important diagnostic mark and source of error are encountered. Fluctuation may or may not be present in ovarian tumour, and may not be observable even when large accumulations of fluid exist. When present, it generally differs materially from that furnished by ascites,—the cause of this difference being

probably the same as that which explains the absence of evident fluctuation in ovarian tumour, even when fluid in large quantities is present,—the fact of such being contained within an investing cyst, and not, as in ascites, in the general cavity of the abdomen; for, excluding the class of fibrous tumour of the ovary in which fluctuation is necessarily absent, there occur a large number or cases in which it is not to be detected. Thus in ovarian tumour of the multilocular type, in which the subdivision of the primal sac is effected either by very numerous secondary cysts, by the existence of septa, or copious deposition of bony or cartilaginous matter, fluctuation is often not discovered, although the total quantity of fluid contained within the tumour may be very large; cases moreover occur in which this sign is also lost from great thickness of the containing cyst; and also, sometimes, from extreme distension. Under certain conditions of unusual accumulation of fluid, in a cyst the parietes of which have failed to enlarge proportionally with the rapidity of the secretion within, fluctuation cannot be detected. Also, in certain instances in which the secretion is remarkably viscid, fluctuation may be absent.

Before entering upon the description of fluctuation as a sign of ovarian tumour, it may be well to allude briefly to a physical sign often mistaken for fluctuation—impulse. The observance of a simple rule will suffice to distinguish between them. The error has, however, often been committed, and has sometimes led to unfortunate results in practice; such has been particularly the case in instances of colloid tumour of the ovary, in which impulse can always be readily produced, but not fluctuation. I may allude to an example in the case of a poor woman who applied to me on account of great abdominal distension from ovarian tumour; it had formed with remarkable rapidity, as colloid ovarian tumours, so far as I have seen, always do, and she was in great suffering. The diagnosis was easy, but little could be done to give relief to the symptoms. She was anxious to be tapped, but, of course, her desire could not be acceded to. Manipulation of the abdomen detected marked impulse throughout the whole tumour, but there was no fluctuation. Shortly afterwards she became a patient of a neighbouring dispensary; the medical officers in attendance smiled at the opinion of the absence of fluid; a trocar was introduced in three different places, but no fluid passed. She soon died, but not from the punctures, and the autopsy revealed an enormous tumour of the ovary of the colloid class. The rule to which I have adverted is a very simple one, and will be best understood by first describing the method in which the abdomen should be examined for fluctuation in cases of ovarian tumour. The patient lying on the back, the legs slightly raised, the respiration tranquil, and the stomach empty, the hands of the examiner should be placed firstly on either side of the upper part of the abdominal enlargement, or in other words in the hypochondriac regions, taking especial care that the hands are not only placed parallell to each other, but also at exactly corresponding opposite points of the abdomen; keeping one hand flatly on the abdomen, the fingers of the other should be employed in striking gently against the exactly opposite point of the parietes; if the sensation of fluctuation be then conveyed to the other hand, it is sufficent; if otherwise, more powerful and sharper tapping or striking should be made; if no fluctuation be then felt, let the hands change offices, but not their position, and the one hand will often feel the wave that was not conveyed to the other. If both hands equally feel fluctuation, it may at once be inferred, that at that part of the tumour at least fluid alone exists; but, if one hand only perceive fluctuation,

then the part covered by the opposite hand is the seat of some secondary cyst, or condensed or solid structure. Step by step the hands alternately percussing and perceiving, are to be slowly carried to the iliac regions until all the lateral boundaries have been carefully examined. If fluctuation has been everywhere and equally felt by either hand, the conclusion may be arrived at, that the ovarian tumour, if it be such, consists chiefly of one large cyst or is what is termed unilocular; if, on the contrary, the fluctuation has not been felt at certain points, although distinct at others, the parts at which it failed to be detected should be separately examined as to their inelastic hardness and dullness on percussion, and thus their precise character be determined, whether, as condensed or solid formations within and attached to the parent sac, or as collateral disease or displacement of healthy structures externally to it. It has already been stated, that it is very rare to find any viscus, with the exception of the urinary bladder, interposed between the tumour and the abdominal parietes; but it has in a few instances happened that a portion of intestine has passed in front of the ovarian tumour, and in such cases would, equally with solid growth, destroy the evidence of fluctuation at such parts; the diagnosis could even then be but little obscured, since marked resonance on percussion would, in the former case, take the place of dullness in the latter. The same method of examination should next be employed in the antero-posterior, by placing one hand far back in the lumbar region, and striking with the other at a corresponding point in front; and this should be similarly performed on either side. By such means, fluctuation will be detected, and thus evidence the presence of fluid, while the absence of that sign in certain points will determine the association of secondary cysts or solid growths. The character of the fluctuation is also not without value, so far, at least, as the dignosis between ovarian tumour and ascites is concerned; in the former case being, if not interrupted by secondary formations, sharp, quick, and distinct; while, in the latter, it is more dull, diffused over a greater space, and not so quickly induced. The fluctuation in the majority of instances of ovarian tumors is sufficiently distinct to prevent its being mistaken for the impulse before alluded to, which is usually observed in a small class of cases in which the contents of the cyst are semi-solid, or gelatinous, as in the example of colloid disease. When fluid is present in large quantity, the fluctuation is easily caused and distinctly felt; but, carefully observed it will be remarked, that a certain interval of time elapses from the moment when the one hand of the examiner percusses, before the wave so produced is felt by the opposite hand; the interval is necessarily extremely brief, but is yet distinctly appreciable. It is otherwise in the instance of semi-solid accumulations; for, although in such cases the impulse caused by percussion is so manifest as often to be mistaken for fluctuation, yet, tested by the same rule, its precise character can be determined. The impulse caused by striking with one hand is instantly communicated to the other. The interval observed in fluctuation has no existence in impulse. The application of this fact is not only of importance in determining the contents of ovarian cysts, but also in distinguishing between ovarian disease and ascites, in which the interval is, probably from the interposition of the floating viscera, rendered still more evident. Percussion may be said to perfect the diagnosis; the ovoid outline determined by its agency in ovarian tumour, has no relation with ascites, while the uniform dullness elicited in the former gives place to unequal dullness in the latter, interrupted by local resonance in whatever part the hollow and floating intestine may displace the effused fluid

Resonance on percussion about the umbilical region is often observed in ascites,—in ovarian tumour, never. Unlike ovarian tumour, also, change in position will vary the relative position of the dullness and resonance elicited by percussion; while also the shape of the distended abdomen will, under the same conditions, often present some variation, accordingly as the erect, supine, or lateral position may be assumed. As a common rule, it may be added, that fluctuation is more obvious in ascites when the patient is in the erect, in ovararian tumour when in the recumbent, posture.

Exclusive of the evidence afforded by the physical signs, the diagnosis of ovarian tumor from ascites is still further assisted by the previous history, and by the association of some form of organic disease sufficient to give rise to dropsical effusion. The absence of urgent syptoms in the earlier progress of ovarian tumor has been already alluded to, and their dependence, even when present, upon some co-existing disease, demonstrated by Cases 12, 13; for it is only when accumulated secretion in ovarian tumors has become excessive, that syptoms of extreme suffering supervene.

Less in importance, because less constant, are the indications afforded by vaginal examination. In many cases, certainly in the majority, valuable evidences of the existence of ovarian tumour are presented by the examination of the pelvic cavity; but there are others in which no departure from the natural condition and relative position of parts can be directed; and it is, perhaps, a nearer approximation to a fixed rule to assume, that ovarian tumour commonly gives rise to some change in the condition of the pelvic viscera than to particularise the form of such change. In the greater number of instances of ovarian tumour of large size, the vagina is elongated, and the uterus drawn up, its os inclining more to one side than the other, or it may be pushed behind the pubis, or lodged posteriorly beneath the sacral promontory; such variations being for the most part dependent upon the uncertain length of the pedicle, and, to some extent, upon subsequent adhesions and the particular organ affected. In other cases, the vagina is found greatly shortened, the uterus prolapsed, or even procident; and thus, beyond the fact of the pelvic viscera being generally displaced in some manner, no common rule can be inferred. Such displacements, however, are of far greater utility in the special diagnosis of ovarian tumor, and avail much when the question of treatment by extirpation arises---a subject to which reference has hereafter to be made. In ascites no displacement of the pelvic viscera is observed; and to this differential sign may be added, the frequent presence of flatulent distension in various parts of the intestinal tube in ascites, readily detected by ascultation or examination by the hand. Some assistance may also be derived from the state of the integuments of the abdomen, which, under great distention from ascitic accumulation, are smooth, tense, and shining, while, in an equal degree of intumescence from ovarian tumour, the skin may still present its usual appearance; in the former, the abdominal walls have to sustain unaided the pressure of the fluid; in the latter, the strong, fibrous, and unyielding cyst contains and supports the burden. In ascitic effusion, also, even when very great, there is seldom any enlargement or distension of the superficial veins. In the case of large ovarian tumors, particularly those of chronic growth, the veins ramifying over the surface of the abdomen are much increased in size, forming large tortuous trunks often becoming prominent, and lying upon, rather than in, the deep sulci, which the finger passing over readily detects.

Let it be remarked, that the diagnostic signs now described are often obscured by the complication of ascites with ovarian disease. In the greater number of such instances, the peritonæal effussion occurs as the consequence of pressure produced by the tumor, either by its impaction in the pelvic brim, or some other means of interruption to the abdominal circulation,—sometimes, however, resulting from causes distinct from disease of the ovary. It is not common to find so large a quantity of ascitic fluid present as to prevent the detection of the ovarian tumor by careful manipulation; the sudden and forcible depression of the fingers during an expiration, will cause them to press upon and feel the hard cyst beneath, although separated an inch or more by interposed fluid. In certain instances, ascites may be said to facilitate the detection of tumour of the ovary; for, when the abdominal walls are not very tense, and are relaxed by a favourable position of the patient, the tumor may be made to move from side to side, and its presence accurately determined. The general configuration of the abdominal distention is still that of uncomplicated ovarian tumour; for, although the lateral regions may be more distended, yet the characteristic prominence of the central portions is evident, especially in the erect posture. The results of vaginal examination are not alterated by the co-existence of peritonæal effusion.

A valuable addition to the diagnostic indictions, when ascites co-exists, is that arising from the conditions under which fluctuation may be detected. It has been previously observed, that the obdomen should, in such examination, be percussed while the hands are placed parallel to each other; and if this rule be carefully followed, it will happen, that fluctuation cannot be distinctly felt, although, from other evidences, there can be no doubt of the complication with asictes. But if the hands be then employed, so as to percuss in tangental lines of the arc formed by the abdominal intumescence, fluctuation will be felt. Thus, a hand placed on the umbilicus while percussion is made in the lateral region, will detect the wave of fluid, which could not be felt when the hands were placed opposite to each other,—a fact dependent upon the interruption of the tumor of the waves of fluid in diametrical lines.

PHYSIOLOGY.

ON THE BILE.

By Dr. H. Bence Jones, F.R.S., &c.

[The bile has long been a subject of the greatest speculation on the part of physiologists, and there is scarcely any theory which has been advanced on the properties of the bile which has not been defended by some writers. Dr. Jones compares it more to a kind of soap than anything else, and proceeds to enquire:]

What is the physiological action of the bile? The most opposite and the most important actions have been attributed to it. It has been said to promote digestion, and to stop digestion. Some say that it neutralises free acid, thus lessening irritation; others, that it increases the peristaltic action of the bowels, thus increasing irritation. It has been said to be partly absorbed into the system to support respiration, by furnishing a highly carbonaceous body. Some have

said that it promotes the absorption of fatty substances; and by others it has been said to have no action upon the fats at all. To solve these questions was the difficulty. Experiments were tried by tying the common duct through which the bile passed; but this is not the way to arrive at a satisfactory result. If the bile is not suffered to pass, a stoppage is put to the function of the liver; the whole order of the system is thrown out, and general disorder produced. In 1844 a new mode of experimenting was begun by Schwann, who collected the bile without allowing it to pass into the intestines, by means of an opening similar to that which I mentioned in the case of pancreatic duct. The action of the liver thus went on as usual, and all the functions of the body were performed without impediment. Twelve dogs lived from sixty-four to eighty days without any bile passing into the intestines: one dog, thus experimented upon, lived four months; and another, belonging, I believe, to M. Bernard, lived a year in this state. It was found that dogs thus treated ate much, and digested badly, partly on account of the unnatural fistulous opening. They did not lose much weight at first; but after a little time they lost their appetite, became thin, and ultimately died. The bowels acted as regularly and perfectly as if the bile had passed in the usual manner. Professor Nasse had a dog that lived from the 12th of August to the 24th of January. The quantity of bile varied with different kinds of food between 31 and 370 grains daily, with from 16·44 to 19·19 per cent. of solid constituents. Less was secreted when the dog was ill. The dog ate much; digested badly; did not lose weight at first; afterwads lost its appetite, and then became thin. M. Blondlot had a dog that flourished for three months. The bowels acted twice daily.*

Even in human subjects, it has been found that when a fistulous opening has been made, owing to perfect obstruction of the common duct, by inflammatory action, the bowels have continued to act when the bile did not pass,—showing that bile is by no means indispensible for their action.

Many experiments were tried with dogs, as to the quantity of bile secreted. The influence of medicine was also tried; and it is interesting to us to know that the action of mercury was decidedly to increase the quantity of bile secreted, as has long been held by medical men. If animals can live for a year, enjoy tolerable health, and digest their food, without any bile passing into the intestines, the importance of bile, and its necessity for the purposes of digestion, have been exagerated.

The action of bile out of the body on the different constitutions of food, tends to precisely the same results as we have seen obtained by experiments in the body. Bile, when mixed with neutral fat or with oil, is found to have no chemical action whatever. It makes a sort of emulsion only, not quite so good as that produced by the pancreatic fluid. I added to solutions both of pancreatic juice and bile equal quantities of water and oil, and then left them, after agitation for some time, to see which produced the most enduring emulsion. You see them here; both have caused the fatty matter to be minutely divided; but I think the pancreatic fluid has divided it and kept it divided the best. When fresh out of the body bile has no action on starch; it does not change it into

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* At the meeting of the French Academy, on the 23rd of June this year, M. Blondlot gave the history and post-mortem of a dog that lived for five years without bile passing into the intestines.

sugar, as we saw the saliva did When, however, it is allowed to decompose, it has a slight action upon starch; but not more than all animal substances have. It has no action on cane-sugar until after it has stood for a considerable length of time, and then the cane-sugar is converted into acid. With grape-sugar, if left for any length of time, it forms lactic acid; but so do all other animal substances when in contact with sugar. It has no action, even when acidulated on casein, or on the alouminous substances which constitute our food.

It has been said that the liver purifies the blood, by removing a large quantity of carbonaceous substance from it. To determine this by absolute experiment was a matter of great difficulty; but Schmidt has endeavoured to solve this question by experiments on forty cats, thirteen geese, many sheep and rabbits, in which he made fistulous openings into the gall-ducts for the purpose of collecting all the bile and determining the proportion between the quantity of carbonic acid thrown out by the lungs and the quantity of carbon in the bile. He passed a tube into the gall-duct, and could measure how much gall came out per hour; and he could determine the composition of the bile by burning it and collecting the carbonic acid. He made, at the same time, comparative experiments on the respiration, some of which I shall have to detail to you in a future lecture; and he came to the conclusion that not more than from one-tenth to one-fortieth of the carbon which passes out of the body passes by the liver, and that therefore the liver has no considerable action in freeing the blood from carbonic acid or carbon. He found that eight-ninths or nine-tenths of the carbonaceous matter remains in the circulation, and does not pass out by the bile at all, but is thrown out through the lungs; a small portion, however, must escape in the urine, probably not much less than passes out in the bile. But I am unable to give you the *proportion* of carbon in the urine and bile daily excreted, from want of experiments.

What, then, in conclusion, is the use of the bile? I have show you that it is an alkaline fluid, and a body resembling soap. If soap is brought into contact with an acid, you know what happens: the alkali of the soap and the acid combine, and the acid of the soap is set free and precipitated. So, also, is it in the bile. If I take human bile, and mix it with acid, (as you see in the experiment with sulphuric acid,) a greenish white precipitate is formed. Let me show you what would happen to human bile, if mixed with the acid secretion of the stomach. This I can do by adding dilute hydrochloric acid to a portion of bile, or better still by mixing some of the clear fluid obtained from the contents of the stomach, which I showed you in my lecture on the gastric juice; by both a precipitate will be immediately produced. The alkali which exists in the bile goes to the acid; it neutralizes so far, the acid reaction coming from the stomach; and it precipitates the insoluable acids, which give rise to choloidynic acid, and even to that still more insoluble substance, dyslysin, in its passage through the intestinal canal. It appears to me, then, that one great action of the bile is to furnish an alkaline fluid, which, when mixed with the acid secretion that has served the purpose of dissolving the albumen, will neutralize it, and lessen its acidity, so as to prevent it from producing irritation and increased action of the intestinal canal. That the stomach can actually bear much stronger acid than the bowels is known to most medical men. That the acid does not pass rapidly out of the stomach I am convinced by the following experiment:—To an adult man I gave 162 grains of dry, pure tartaric acid dissolved in two ounces of water. No pain was felt for three hours; no food

was taken during this time; and, without doubt, all the tartaric acid would in these three hours have been absorbed, or would have passed out to the stomach. At the end of this time a pain in the bowels began to be felt, and at the end of the fourth hour there was very considerable pain, coming on in paroxysms. At the lapse of about five hours, if they had been allowed to act, they would have acted from the acid taken. A repetition of the experiment, with 84 grains, gave precisely the same results. When the acid entered the bowels, pain began to be felt, and, if bile in plenty had been poured out, the acid would have been neutralized, in part at least; the alkali would have combined with the acid; the insoluble bile acid would have been formed as a percipitate, and been thrown out of the body. If this be so, sluggishness of the liver, a deficiency of alkali poured into the duodenum, becomes a reasonable cause of excessive acidity of the intestines; the gastric acid required to dissolve the albuminous food, if sufficient bile is not formed, will pass into the intestines, and produce irritation and increased action. The physician has long held, that want of action of the liver gives rise to acidity, and that alterative medicines correct this state.

But the very great size which the liver attains in the fœtus appears to indicate that it performs some additional action independent of food and of digestion. This additional action has been said, by German physiologists, to be the reparation and the formation of blood globules; but this is by no means proved. It seems to me much more probable that it is for the purpose of neutralizing the acid, and probably also, for the purpose of removing, when requisite, some of the carbonaceous substances; in certain states compensating for the action of the lungs, though, in ordinary states, removing much less carbon than has been said. The bile gives water, moreover, to dilute the chyle; it tends to the subdivision, in some degree, of the fat and oil of our food. It acts upon the free acid of the intestine; and some of it may be possibly absorbed, and pass into the circulation again, as Professor Liebig originally conjectured. It is not nearly so important as to the gastric juice, which dissolves the albuminous part of our food, or the pancreatic fluid, and the salivary fluid, which convert all the insoluble starch, as I have shown you, into soluble sugar. Lastly, the importance of the bile in forming sugar from fat, is one of those facts that cannot be overrated. By this discovery of M. Bernard's, very important knowledge relating to the physiology and pathology of man will be obtained during the next few years; at least there can be little doubt, that the disease know as diabetes, if not closely connected with this production of sugar in the liver, must at least be influenced by it to a considerable extent.—*eMdical Time, July 5*, 1851.

SELECTED MATTER.

MEDICINE.

SURGERY.

CASES OF HERNIA.

By Bransby B. Cooper, Esq., F.R.S.

Whatever may be the experience of a surgeon in the treatment of hernia, I think he will be obliged to admit that he has seldom seen two cases exactly resembling each other; there is almost always some difference in the symptoms, some peculiarity in the form of the tumour, or some anomaly in the concomitant circumstances; so that, when at length compelled to resort to the operation, it is generally set about rather as a step of exploration than with confidence as to the physical conditions of the case being perfectly understood. Although the anatomy of the outlets of the abdomen is well-known, the course of hernial protrusions frequently deviates from that of the natural channels, so that it is often difficult to ascertain by which outlet the viscus has passed from the abdomen. The difficulties arising from anomalous circumstances attendant upon cases of hernia, are often productive of so much trouble and anxiety to the practitioner, and interfere so materially in forming the diagnosis, that I have thought the following cases may prove useful, as they are illustrative of some of the points most likely to mislead and confound the judgment.

Mr. Porter, of Dublin, related a case of strangulated hernia to me, in which he had lately operated. Upon opening the hernial sac, and dividing the stricture, he was unable to return the protruded intestine into the abdomen. He sent his patient to bed, covering the intestine with linen dipped in warm water, to maintain as nearly as possible the natural temperature of the protruded bowel. Notwithstanding the stricture had been freely divided, all the symptoms of strangulation remained unabated, and in three days the patient died. In a post-mortem examination it was found that the ascending colon had protruded beneath Poupart's ligament into the crural sheath, passing over the ilium, which was so firmly compressed just at the point of its entrance into the cæcum that the ileo-colic valve was completely closed, and symptoms of strangulation were produced in consequence of the contents of the small intestines not being able to pass into the colon. Upon three or four occasions I met with a similar difficulty in returning the protruded intestine, after all the steps of the operation had been successfully passed through, up to the division of the stricture. This difficulty probably arose from the distension of the viscera within the abdomen. In one case I found it quite impossible to return the bowel, and I made a slight puncture into it, with the object of setting any gaseous matter at liberty, but I failed in producing the intended effect, as the mucous membrane closed the aperture, which I dared not enlarge. The patient died of peritonitis and I made up my mind never to try such an experiment again. In a similar diffi-

cult case, I had the patient put to bed, after trying ineffectually to reduce the hernia, and covered the protruded bowel with a large thin poultice. In a few hours the patient had a motion; the bowel was then easily returned, and he recovered. I consider this treatment far safer and more likely to be successful than long-continued attempts at reduction by taxis, puncturing, or any other violent means.

Anomalies of form in herniæ will often produce serious difficulties in the diagnosis. A bubonocele occurring in an individual in whom the external ring is abnormally small, may, for instance, develope itself not only in the inguinal canal, but also upwards and outwards as high as the anterior superior spinous process of the ilium, so as to prevent a most unusual appearance for a hernial protrusion.

A servant of Dr. Babington was admitted into Guy's Hospital, with a large tumour occupying the whole space between the anterior superior spinous process and the symphysis pubis, overlapping Poupart's ligament, assuming the appearance of a large femoral hernia; indeed, it was a somewhat doubtful point whether it was femoral or oblique inguinal hernia. The patient was suffering from symptoms of obstruction rather than of strangulation; he was put into a hot bath, and Mr. Morgan succeeded in reducing the hernia, using considerable force in the taxis. The patient perfectly recovered.

I have mentioned that Mr. Morgan reduced the hernia by somewhat forcible taxis. This gentleman was in the habit of employing considerable force in the reduction of herniæ; and although I must say that I should not myself adopt this practice, I confess that I have seen Mr. Morgan succeed in returning the intestine in cases in which the amount of force ordinarily employed would have completely failed. Sir Astley Cooper also rather leaned to this comparatively violent treatment. Some time ago Sir Alexander Crighton told me that in Russia it is an almost universal practice to force herniæ back by the taxis; and although by such measures they occasionally rupture the intestine, the general success is very considerable. Personally, however, I should never recommend such a practice, and should always prefer resorting to the operation in cases where a moderate amount of force is insufficient to secure the return of the protruded bowel.

A woman, aged seventy-two, the mother of several children, was admitted into Guy's Hospital, with severe symptoms of strangulated hernia, which had commenced three days before. She had had a rupture for several years. When she was examined a tumour was found in the left inguinal region; it had all the appearance of a bubonocele, and the finger could be placed on the spine of the pubis under the tumour, which circumstance might in itself almost have been considered a conclusive proof of its being an inguinal hernia. Upon close examination, however, the tumour could be felt issuing from the saphenous opening of the fascia lata, showing its femoral origin; the anomalous appearance of the hernia was, however, manifest to all who saw it. The operation was immediately proposed in this case; but the patient would not submit to it until the next day, when I operated; the sac could not be emptied without being laid open, when a large quantity of omentum was found in it, with a knuckle of intestine behind, entangled in the omentum. This was probably the reason why it was necessary to open the sac. Upon passing my finger to the stricture, I could feel the epigastric artery beating forcibly immediately across it. In con-

sequence of this I was enabled to divide the stricture without wounding the artery, which I could not have done had I not felt the vessel pulsating. After dividing the stricture, I easily returned the intestine; a portion of the omentum was removed, and the patient was put to bed. It was two days after the operation before any motion passed, but the patient recovered without a bad symptom.

On the 30th of October, 1846, a woman, aged forty, (mother of eight children) was admitted into the hospital with acute symptoms of strangulated femoral hernia. She had had hernia for four years; it had frequently come down, but she had always been able to return it. On the 28th instant, the hernia suddenly descended as she was coming down stairs. She was immediately seized with vomiting and a sensation of tightness across the scrobiculus cordis. The tumour was peculiar in form; instead of being of the usual shape of a femoral hernia, it was much elongated outwards, running under and parallel with Poupart's ligament, reaching almost as far as the anterior superior spinous process of the ilium. The elongated portion was extremely moveable, and felt as if it were immediately under the skin, but it could not be pressed towards the crural ring. It was difficult to believe that the coverings of the hernia could have so prolonged themselves, and it was, indeed, doubtful if there were not some adventitious swelling unconnected with the hernia. As the symptoms were very urgent I proposed to perform the operation immediately. The patient consented, and I commenced with a vertical incision in the seat of common femoral hernia. I made a second incision at right angles to the first. On dividing the skin and superficial fascia, I exposed the internal abdominal fascia, (the fascia propria of Sir Astley.) The incision was continued outwards over the prolongation of the swelling. I divided the fascia in the same direction as I had previously divided the skin. The hernial sac was thus exposed; and it was continuous with the whole of the swelling. I next divided the stricture, and the sac became immediately quite flaccid. In consequence of the anomalous form of the tumour, I determined, however, upon opening the sac, in order to ascertain the nature and condition of its contents. I found in it a knuckle of intestine, elongated in the direction corresponding to the tumour; it was quite healthy and fit to be returned into the cavity of the abdomen. A few hours after the operation, the patient had a free evacuation, without the use of a purgative. This patient also speedily recovered.

October 15th, 1851.—I was called to a case of hernia by Mr. Thornton, of Museum-street. The patient was housekeeper to a lady in St. James's-place. She was fifty years of age, and when I saw her had been suffering for three days from strangulated hernia. She was very sick, but there was no other urgent symptom; no pain nor swelling of the abdomen; the countenance not distressed; and the pulse, full, soft, and compressible. Upon examining the tumour I found it large and flaccid, extending down the thigh. It had the appearance, and was about the size of a calf's bladder, partly filled with fluid. By pressing the fingers deeply into the tumour, a substance very similar to intestine or omentum could be felt. This could be traced under Poupart's ligament. I could not succeed in reducing the swelling by the taxis. The patient stated that the hernia, which had existed for about five years, was sometimes twice as prominent. There was a second tumour, of small size, above Poupart's ligament. This was pendulous, and of the size of the end of the little finger. It had the

appearance of a ventral hernia, protruding between the separated tendinous fibres of the extended abdominal obligue muscle. I ordered a large dose of opium, ice to be applied to the tumour, and a castor-oil enema to be given. The next day the sickness was less urgent, and there was no aggravation of the other symptoms. I, however, recommended the operation, but to this the patient would not consent. On the following day she still remained much the same; but as there had been no relief to the bowels, I urged that the operation should be performed immediately. As the patient now began to feel that her situation was critical, she agreed to submit to it. I commenced my incision from the upper tumour, continuing it downwards below Poupart's ligament, as far as about the centre of the lower swelling, cutting through the skin and superficial fascia, exposing the free edge of Poupart's ligament, as well as the protrusion of the upper tumour through the tendon of the external obligue muscle, as I had expected. I next proceeded to explore the upper swelling, and for that purpose opened the sac. I could see nothing but omentum. I then divided the opening through which it had passed, and Poupart's ligament; this showed that the upper swelling was merely a portion of the lower, as they were now rendered quite continuous. I opened the sac of the femoral tumour, and found a large quantity of omentum, and, lying behind it, a knuckle of four or five inches of intestine. I then divided the stricture, and as the intestine, with the exception of slight congestion, was quite healthy, I returned it into the cavity of the abdomen, leaving the omentum within the sac; the edges of the wound were brought together by suture, and the woman put to bed. She said she was relieved from all sensation of constriction. On the 16th, she had a free evacuation of the bowels, the sickness had subsided, and the pulse was quite natural. On the 17th, purging came on, she was very much flushed, and symptoms, which were almost those of mania rather than delirium, supervened.* This might perhaps have arisen from an hereditary tendency, her mother having died mad. I have observed, in other cases, that delirium followed upon omentum being left in the sac; but in those the omentum sloughed, which did not happen in this case. After the 17th the patient went on improving; the wound healed rapidly, almost entirely by first intention; and on the 27th I left her quite cured.

Anomalies in the form of hernia may, as I have said already, render it very difficult to diagnose with accuracy the nature and direction of the protrusion, but anomaly of form is not the only source of difficulty; the circumstances concomitant with the hernial protrusion are sometimes sufficient to mislead the judgment very materially. I have selected the following cases to show how far accompanying symptoms may interfere with the diagnosis of hernia.

In May, 1838, Mr. Callaway was sent for to a case of inguinal hernia, on the right side of a lady, aged 36. The hernia had descended only the day before, but it had existed for nine years. The symptoms being urgent, Mr. Callaway determined upon operating immediately. On opening the sac he introduced his finger through the internal ring, and returned the protruded intestine, which was quite healthy. As soon as the intestine was returned, a fluid escaped by the opening in so large a quantity that it was thought at first that the bladder had formed the protrusion, and had been wounded; it was soon, however, seen that this was not the case, for the fluid coagulated on the sponge used in wiping it away, and when a portion was collected in a wine-glass it separated into a clot, which precipitated, and a serum which remained on the

top: it also coagulated by the application of heat. These circumstances seemed to show that it was an ovarian cyst which had been opened. Soon after the operation the bowels were relieved, and the patient recovered.

In the last case I have mentioned hernia of the urinary bladder; this is not a common occurrence; I have seen but one case, of which I have published a description. [*Vide* "Lectures on Surgery," *p.* 476.] In this case the bladder protruded through the external ring, and then passed partly downwards into scrotum, and partly upwards into the inguinal canal, so as to give it a very anomalous appearance. The interference with micturition, and the absence of any evidences of intestinal obstruction led to the formation of a correct diagnosis in this case.

In June, 1846, a country practitioner related to me the particulars of the following case:—He was called in to see a female who was suffering from all the symptoms of strangulated femoral hernia, which had existed for three days. Upon applying the taxis the tumour was found to be fluctuating and very tender to the touch. The patient stated that she had had a rigor a few days before. The surgeon therefore came to the conclusion that the swelling was an abscess; he consequently opened it, and let out a considerable quantity of pus. The symptoms of hernia were not, however, relieved, and continued, indeed, until the patient died. In the post-mortem examination a hernial sac was found lying behind the abscess; it contained strangulated intestine. This case offered very considerable difficulties, and it shews the necessity for invariably exploring a tumour existing in the seat of hernia, if there exist concomitantly with other symptoms indications of insuperable visceral obstruction.

In September, 1846, a man was brought into Guy's Hospital, suffering from what were supposed to be symptoms of strangulated hernia. A large scrotal hernia was found, which had been down for three days. The patient stated that he had had the rupture twenty years, but that he could always reduce about two-thirds of it himself, leaving, however, a tumour in the scrotum. When the man was brought into the ward, I remarked the extraordinary blueness of his face. I found his pulse very feeble and the extremities cold. I ordered a hot bath and a castor-oil enema, and three grains of opium, as soon as he came out of the bath. After the bath I again saw him; his extremities were still cold; pulse not to be felt; the vomiting, distress of countenance, and blueness remained unchanged. Ordered wine and brandy. In an hour after his admission he died. I did not propose the operation, as when the man was brought into the hospital he was *in articulo mortis.*

The day after his death, I examined the body. I proceeded as if going to operate for strangulated hernia. I did not lay open the whole length of the sac, but merely cut down to the neck to liberate the constriction, if any existed. I could not, however, find any. I then proceeded to examine the contents of the sac. I found in it black intestine in large quantity; unfolding this an adventitious cord of lymph was found, which was quite firm and fibrous; it encircled a large knuckle of intestine, producing the strangulation. Had I operated in this case, I believe I should not have discovered this adventitious constricting band, but have been satisfied with removing any constriction about the neck of the tumour, as is usually recommended in large hernia. This case is in this respect very instructive; it shows that in all anomalous cases we ought to examine into the state of the bowel, even although we may subsequently

determine not to return it into the abdomen. The adventitious band of lymph was of very long standing, and, I have no doubt, had formed within the abdomen and had come down into the scrotum with the protruded intestine.

The patient's account of his own case, in which he stated that he could only return part of the tumour, was proved to have been quite correct, as he was the subject of a hydrocele, which of course could not be returned into the abdomen with the hernia.

A gentleman, aged forty, a patient of Mr. Toulmin, was seized with symptoms of strangulated hernia. Upon one or two occasions before, he had had symptoms of the same kind, with a fullness in the right inguinal region, which gave way to the application of the taxis. A truss could not, however, be applied in this case, as the testicles had not descended into the scrotum.

On the 19th July, 1847, symptoms of strangulation again appeared, accompanied as before by swelling in the right inguinal region. The tumour was partially reduced by the taxis, but the symptoms remained unabated. I was then called in, and after careful examination of the patient, proposed to lay open the right inguinal canal. I performed the operation on the evening of the 23rd instant. No strangulated intestine was found, but what was supposed to be the spermatic cord was seen lying in the canal. The patient was not relieved in any respect by the operation; the constipation and other symptoms remained, and on the 24th he died.

The body was examined after death; upon opening the cavity of the abdomen, the intestines were found to be deep in colour, and glued together by recently effused lymph; the testes were at the external rings. The left internal ring would easily admit the little finger into the inguinal canal, which contained nothing abnormal. Upon laying open the right inguinal canal up to the abdominal cavity, the appendix cæci was found passing through the internal ring into the canal, and terminating by a bulbous extremity which adhered to the testis; the spermatic cord passed behind, and was covered by the appendix cæci. About eighteen inches of the ilium, several portions of which were empty and constricted, forming numerous intricate convolutions, formed a mass, which was firmly adherent to the rectum, and filled the pelvis; upon separating this mass, pus flowed out.

It was quite evident that this patient had been the subject of peritonitis, creating adhesions which had interfered with the action of the implicated portions of bowel, and produced the insuperable obstruction which had been attributed to the tumour in the right inguinal region.

October 6th, 1851, I was sent for to Watford, to operate on a patient who had had a small femoral hernia for three years. Four days before, a fresh portion of intestine had come down, immediately upon which all the symptoms of strangulation had come on. I attempted to return the tumour by the taxis, but failed; I therefore proposed to operate at once. The moment I had divided the internal abdominal fascia, the tumour became flaccid, but I could not empty the sac, which I consequently determined upon opening; in attempting to do this the whole hernial protrusion slipped up into the abdomen, *en bloc*. I made every effort to bring it down again, but could not succeed; and as the patient was relieved from the pain and sickness, and the expression of the countenance was much improved, whilst the pulse became stronger and more regular, I felt some confidence in the hope that, in passing up, the intestine had liberated

itself. The patient died, however, on the fourth day, the bowels not having been relieved, and there can be no doubt that the knuckle of intestine was returned into the abdomen in its constricted state; that, in fact, the external was converted into internal strangulated hernia. The friends of this patient would not permit a post-mortem examination.

The following case I think worthy of notice, from the difficulty there was in detecting the existence of any external tumour—in fact, in diagnosing whether the patient was the subject of an external or an internal hernia:—

On February 17th, 1852, Mr. Ward, of Watford, sent for me to see a patient, a lady about forty-five, who had suffered for four days with urgent symptoms of strangulated hernia, but he could not detect any external tumour. My attention being excited, I proceeded to examine the patient with increased care, and at last suspected I felt a small, circumscribed hardness in the left thigh, in the seat of femoral hernia; the patient experienced greater pain on pressure in that part than on the corresponding side, and I determined, under the impression resulting from these facts, to propose the operation at once. She consented; and upon laying open the femoral sheath, I discovered a small rounded protrusion, not larger than a marble; I then opened the sac, divided the stricture, and returned the small knuckle of intestine into the abdomen. I immediately gave the patient a grain and a half of opium. All her symptoms were alleviated; she had two evacuations the next day, and recovered without a bad symptom.

The next case affords a subject for reasoning in quite a different manner to the last. In this we observe symptoms of hernia, with an evident hernial tumour, but the nature of those symptoms showed the impropriety of surgical interference; whilst the last case demanded exploration, although the physical conditions tended to forbid it.

August 27th, 1850, I had a patient at Guy's Hospital, a young man, the subject of an inguinal hernia on the left side; the tumour was small, tense, and painful, but he had not been sick, nor had any other symptom of strangulated hernia, beyond constipation, attended with a general tenderness over the left iliac region. The state of his pulse and expression of countenance led me to suspect that he was the subject of peritonitis, and I therefore determined to try constitutional means before I proposed any operation. I ordered the patient to be put into a hot-bath, twenty leeches to be applied in the left illiac region, ice to the tumour, and gave him a grain and a half of calomel and half a grain of opium every three hours. On the 28th the pain in the abdomen was much relieved, the tenderness of the tumour nearly gone, and it had become small and quite soft; a purgative enema was administered in the evening, and during the night he had free evacuation. From this period he recovered, but was kept in the hospital in the recumbent posture; ice was applied to the tumour; and in the course of a week or ten days the hernia was reduced, a truss applied, and the patient got quite well.

The case with which I shall conclude this series of cases of hernia, is one in which the rupture occurred in an individual of hæmorrhagic diathesis, a circumstance which gave rise to some peculiar and anomalous symptoms:—

James C——, aged twenty-nine, by occupation a cab-driver, came into the hospital in December, 1850. He was a man of very intemperate habits. Ten days before, he had been admitted under Mr. Cock, suffering from symptoms of strangulated hernia. At that time he had a warm bath, took opium, and had

ice applied to the tumour; after which Mr. Cock succeeded in reducing the hernia, and the patient left the hospital quite relieved.

Upon his second admission under myself, he stated that he had been ruptured about four years. The hernia had been down about six hours, and the symptoms of strangulation were very urgent. Taxis, warm bath, full doses of opium, and ice to the part, were all tried, but the hernia could not be reduced. The treatment was persevered in without effect for twenty hours. It was then determined that the operation should be performed without any further delay. The operation was attended by an unusual diffused hæmorrhage; the sac was opened, and about eight inches of small intestine found protruding. This was of a dark red colour, and congested. It was returned with considerable difficulty. The next morning, symptoms of peritonitis showed themselves, and in spite of treatment with calomel and opium, leeches and poultices, it continued to increase, until the patient sunk, about forty-eight hours after the operation. After death the body was examined. There was considerable ecchymosis around the leech-bites upon the abdomen, the superficial veins all over the body were distinct and prominent, and dark blood was escaped from the mouth and nose. Upon cutting into the lungs, they were found curiously mottled, as in pulmonary appoplexy. Bloody serum was found in the cavities of the pleuræ and pericardium; about two pints of clotted blood were found in the cavity of the abdomen, leading Dr. Lloyd to suspect that the epigastric artery had been wounded; the artery and vein were injected, but they were found to be intact; kidneys healthy; liver pale. From the appearance of the tissues generally, and from the statement of his friends, that in his lifetime the slightest cut was attended with severe bleeding, and that his flesh became easily bruised and became black and blue, it was evident that this patient was the subject of hœmorrhagic diathesis.

PATHOLOGY.

ON THE DISEASES AND CLIMATE OF CALIFORNIA.

By James Blake, M.D., F.R.S.

During the months of October and November, 1850, the principal diseases were dysentery and diarrhœa: 85 per cent. of the cases I attended, during these months, were of that nature. The dysentry was of a very untractable character, wearing the patient down by frequent bloody discharges. and even when checked being constantly reproduced. It was very fatal; in the absence of any positive data, I believe about 30 per cent. of those attacked died. The great mortality was not so much owing to the virtulence of the disease itself as to the peculiarly unfavourable circumstances in which those attacked by it were placed. By far the larger part of the sick, probably 90 per cent., consisted of emigrants who had just crossed the plains, having accomplished a journey which, at all times fatiguing, was in the year 1850 attended with the most trying circumstances. By far the greater number of emigrants who arrived here in autumn had not only been broken down by fatigue, but had been forced to subsist on a short allowance of food, and that frequently of a bad quality. Under these circumstances, it is not surprising that the first opportunity that was afforded them for satisfying their appetites should have been an occasion for

overloading the stomach and thus producing diarrhœa. The sudden change from the dry, bracing atmosphere of the interior of the continent and the mountains, to the comparatively heavy, warm air of the valley, exerting a most depressing effect on the system. On this point I can myself testify; for, although without any apparant sickness, yet for the first three weeks after my arrival in the country, from crossing the plains, I was so prostrated that I could lie on my bed during the whole day, without the slightest energy, either physical ormental, and many of my friends were affected in the same way. Disease, I believe, is often brought on by men attempting to work while in this state of protrastion. The diet, also, was a fruitful source of disease, not only as regards its quantity, but even its quality. The only fresh meat to be had was beef, and this was generally fried, or rather boiled in rancid lard; the flour was to a great extent damaged, and the butter and salt meat were all more or less rancid. I state these facts to show that, althought diarrhœa and dysentery prevailed here to so great an extent, and so fatally, during the fall of 1850, the causes of its ravages are to be looked for prinipally in the unfavourable circumstances in which the population was placed rather than to any malarious influence. But not only were these circumstances fruitful sources of disease, but they presented an almost insurmountable obstacle to recovery, even when the more violent syptoms had been subdued. There was no such thing here as that valuable hygienic remedy called nursing. From the toast water of the sick room your patient had to go back to the beef, salmon, and rancid grease, of the boarding-house. There were none of those light and valuable edibles which gradually lead the invalid by a safe path back to his ordinary diet. Relapse after relapse occurred, and it wanted but little assistance from the diarrhœic influence which generally precedes the appearance of cholera, to produde a state of gastroenteric diseases among four-fifths of the newly arrived population.

In this state of the general health, the cholera made its appearance about the beginning of November, and never did it fall on a population so unprepared to yield to its ravages. Although it is impossible to obtain any accurate data as to the number of its victims, yet I am confident that, during the few days at which it was at its height, not less than one per cent. of the population was daily carried off by it. There was nothing peculiar to the disease as it occurred in this country; its apparent virulence can be accounted for by the previous state of those attacked. The tables appended, showing the returns from the State Hospital in this city, prove that, even during the summer and autumn months, there is very little tendency to gastroenteric disease, at least when the exciting causes above alluded to are absent.

The diseases more particularly characteristic of the winter months are typhus fever, rheumatism, erysipelas, and pneumonia. The cases of typhus that came under my care were of a mild form, although generally lasting from fourteen to twenty-one days. In none of the cases which I treated did any unfavourable symptoms present themselves. The treatment was purely expectant, with the exception of quinine and bark, and stimulants towards the latter period of the disease. In two instances the red-coloured spots were noticed on the sixth day of the disease. From the returns of the State Hospital, it would appear that this disease was prevalent during the autumnal months of last year, and the mortality was 39.7 per cent., a decidedly large mortality. Only three cases were admitted from the city, the others being brought from the country.

The cases of pneumonia showed themselves soon after the first rains, and could generally be traced to exposure to wet and cold: they were very amenable to treatment. But three cases came under my care, and in every case the patient made a rapid recovery, the time of treatment averaging nine days.

Rheumatism was one of the most common diseases during the winter months of '50 and '51, attacking principally those who had been previously affected by diarrhœa and dysentery; and in these it generally presented itself with very troublesome, and in fact alarming symptoms. The disease was generally of a low type, and in every case that came under my care was accompanied by inflammation in the muscles, or at least in the aponeuroses; this inflammation was the principal and most distressing symptom. In two of these cases suppuration took place; in one subject, in the calf of the leg, under the gluteus maximus and under the trapezius; and, in the other, in the calf of the leg. In three other cases that I treated, these swellings were present in the calf of the leg, and although I was led strongly to suspect that pus had formed, yet they recovered without any abscess being opened. Pericarditis existed in three cases, but, notwithstanding the unfavourable character of the general disease, this complication did not present itself in a severe form. One of the cases terminated fatally, that in which matter had formed so extensively. I believe the case would have been saved had it remained under my care; I was, however, superseded by an herb doctor, whose first care was to bind up the abscess as tightly as possible to prevent the discharge from weakening the patient. The treatment adopted the administration of colchicum and quinine, and sometimes sulphate of iron; recovery was slow; the average time of treatment being from forty-one days. I had an opportunity of seeing some other cases in which the same symptoms showed themselves. Rheumatic affections are not so common amongst the mining population as we might be led to expect, from the exposure to which they are subject, working, as too many of them do, for hours at a time in the water, and also frequently exposed to rain. But very few cases of this disease have been admitted into the hospital during the past year, and this is probably owing to the absence of diarrhœa and dysentery, which seem to exert a peculiar influence in predisposing the system to rheumatism.

Erysipelas is a disease which has been extremely fatal in the country during the last twelve months; but it has existed to a greater extent in the mountains than in the valley. During the course of last winter, five case came under my care; in all of these, the head was the only part involved, with the exception of one in which the inflammation spread to the shoulder and chest; three of these cases occurred in the same house, and the first case that showed itself was in a man who had been living in another house in the country, where there was a case of the disease. The most striking peculiarities of these cases were the inflammation of the mucous membranes communicating with the natural openings, and the absence of that amount of general and cerebral disturbance which so often renders erysipelas of the head a formidable disease. In every case, the mucous membrane of the mouth and fauces were inflamed; in four cases there were ulceration and discharge of fetid pus from the nostrils; in three cases, the palpebral conjunctiva was severely affected; and in four of the cases there was a discharge of pus from the ears. And yet, notwithstanding these symptoms, the cases in which they presented themselves were attended with as little cerebral and general disturbance as any I have ever seen, in which the head

was involved. The fever never rose high, nor was repletion required in any case. A dose blue pill and castor oil, and then quinine, combined with small doses of blue pill and rhubarb, was the treatment usually adopted. The only case that terminated fatally was a man of intemperate habits, who died from a complication of the disease with pneumonia. The average duration of the treatment was nine days. This disease is of frequent occurrence in the mountains, and I have seen many cases of it during summer. The localities where it mostly prevails are the deep valleys in the mountain districts, which are shut in on all sides by high hills, where the extremes of temperature are great, and the air has a tendency to become stagnant. In some of these places it has proved extremely fatal, the mortality being more than fifty per cent. This large mortality is to be accounted for partly by the intemperate habits of the subject attacked by it, but also in a great measure, I believe, from the too general use of calomel, and from not giving quinine and tonics at an early period. In two of the cases that came under my care, gradual conjunctivitis remained as a sequela of the disease, and I have seen some very severe cases of this affection that have been produced by it.

Diseases of the air passages are extremdly rare. A mild form of catarrh showed itself apparently in an epidemic in February, 1851, and the same effection is occasinnally met with during the winter months; but I have not yet seen a case of acute bronchitis, and the experience of many of my friends in the city is to the same effect. This is probably owing to the absence of cold dry winds during the winter months. Very few cases of phthisis are met with, considering the circumstances in which so large a portion of the population is placed, and which in other climates could not fail to develope the latent germs of this disease which exist in so many individuals. Speaking from my own limited experience, it is more amenable to treatment here than in any other country in which I have practised. Two cases of the disease in an early stage have come under my care, occurring to individuals who had a strong constitutional tendency to it; in each case, hæmoptypsis to some extent had occurred, and that partial solidification of the lung, indicated by the prolonged rough expiratory murmur, evidently existed. One of these cases was apparently quite recovered, the individual having enjoyed good health for some months. The other case is improving, having gained six pounds in weight during the last month; cod-liver oil, with quinine and iodide of iron, has been the remedy used. It is an interesting fact that in neither of these cases was any marked symptom of bronchial irritation present and I believe the exciting cause of the disease had, in each case, been improper diet, or living too long on salt meat, which probably was rancid. A better diet was ordered, and strictly adhered to; and it is the advantages derived from this, and also from the complete absence of any source of bronchial irritation, that have rendered the disease so amenable to treatment. Many cases prove fatal, as well here as elsewhere; but I believe that the climate of this country presents advantages such as are not to be found in any other part of the civilized world, for the treatment of phthisis in its earlier stages.

The diseases arising from malaria might be expected to be very prevalent in the valley of the Sacramento, containing, as it does, thousands of acres of lands subject to annual overflow, and from which there is no escape for the water except by the slow process of evaporation under the rays of an intensely hot sun; yet, notwithstanding this, cases of intermittent and remittent fever are compara-

tively rare in this part of the valley. A few mild cases of intermittent fever occurred in the neighbourhood of the city last spring, but they appeared to be confined to low localities which were situated in the immediate vicinity of stagnant water. I have heard of but two cases of congestive fever, and both of them on low bottoms near the American River, about three miles from the city. As a general rule, the city is exempt from any of the more violent forms of malarious disease; a few mild cases of intermittent and remittent fevers occurs, but they are comparatively rare. The whole number of cases admitted into the hospital in this city, which originated in Sacramento, was twelve during the last summer and autumn. But, although the influence of malaria does not show itself by producing any of the more marked forms of disease by which its presence is usually manifested, yet we have constant indications of its existence, by the character it impresses on almost every form of disease occurring in this locality. It would appear that, when in the usual state of health, there are very few individuals in the community who are susceptible to its influence, but when the resisting powers become weakened by any form of disease, the malarious influence then makes itself felt, modifying to a greater or less extent a very large amount of the cases of other diseases. This influence it is often difficult to detect; it is not manifest by any marked symptoms of intermission or even remission, but can only be suspected from the apparant obstinacy of the disease, and from the effects of treatment. Dysentery, diarrhœa, erysipelas, rheumatism, chronic gastritis, and even pyrosis, cases in which the remedy might appear to be strongly counter-indicated, were benefited by the use of quinine, and, in fact, would not yield without it. This modified existence of malaria, in a locality were the elements from which it is generated surround us on every side, is probably to be accounted for by the dryness of the atmosphere during the hottest months in the year; to the moderate temperature that generally prevails, when the amount of humidity in the atmosphere would favour its propogation or diffusion; to the prevalence of constant breezes during the summer months; and to the coolness of the nights, which conduce to refreshing and invigorating sleep.

As regards the number of cases of bilious remittent reported to have been admitted into the hospital, I believe they were for the most part cases of the mildest form of remittent fever. I understand from many medical gentlemen who practised here during the summer, that they did not see a single case of bilious remittent, and the short time the patients remained in the hospital (on an average eight days)* would indicate a very mild form of remittent fever. From the most accurate information I could acquire (for no records are kept,) the greater number of the cases come in from the upper part of the valley, more particularly above Marysville. Scrofulous diseases are rarely met with, and I believe the climate will be found to offer the most favourable conditions for the prevention and cure of this class of diseases.

Diseases of the skin are rare; eczema is the only one I have met with. Onychia is common among the mining population, and slight wounds on the hands are very apt to prove troublesome; this is probably owing to the nature of the diet, for even when wholesome article can be obtained, they are generally spoiled by boiling them in rancid grease. The quantity of rancid grease and salt meat [illegible] in this country with apparant impunity offers but a poor confirmation of the views of Liebig, of the injurious partially decomposed substances on the animal economy.

Diseases of the nervous system are not very common, although I believe forming a larger proportion in comparison to the whole amount of disease than in the Atlantic States. A very large number of cases of insanity have occurred, thirty-eight insane patients having been received into the hospital from May to December. This prevalence of insanity is undoubtedly owing to moral more than to physical causes, although the extreme dryness of the atmosphere during the summer undoubtedly renders the nervous system peculiarly excitable. There is one fact with which I have been struck whilst travelling in the mountains during the last summer; viz., the rare occurrence of *coup-de-soleil*. It might be supposed that the miners would be peculiarly liable to this disease, exposed as they are for hours to the burning rays of the sun, and frequently with their feet in ice-cold water; yet I have never seen or heard of an instance of sun-stroke, although the rays of the sun are certainly more powerful here than most of the Atlantic States.

The climate in the mining regions very much resembles that of the valley, and it is necessary to ascend some distance in the mountains before any marked difference is noted, even in the temperature. I have no correct data on this point, but the state of vegetation would indicate that there cannot be much difference in the temperature. When I left Placerville (about sixty miles south of Sacramento) about three weeks since, or in the middle of February, the young shoots of the buckeye were three inches long. The manzaneeta (a species of erica) was in full bloom. The humming birds were building their nests, and I believe vegetation was quite as forward, and, perhaps, more forward than in the valley. This was at an elevation of two or three thousand feet above the valley.

The mining regions are generally extremely healthy, with the exception of those localities which are situated in deep valleys surrounded on every side by high hills. Here the extreme of temperature is very great. As a general thing, the nights in summer are calm, and the great radiation that takes place under a perfectly clear sky rapidly cools the layer of air in direct contact with the ground. This cold air can be felt running down every gully and ravine leading into these deep hollows, and if there is not any large opening by which it can drain off, it accumulates and fills the air with a body of dense, stagnant air, the temperature of which descends very low before morning. In such places, typhus fever and erysipelas were very prevalent during last summer, and were also very fatal. With this exception, the mountain regions are very healthy; and it is a striking fact, in confirmation of this, that exposure can be borne here to almost any extent with impunity. During ten months in the year, the shelter of a tree is all that is required. When travelling in the mountains, I always slept in the open air, except when it rained, and I never experienced the slightest inconvenience from so doing; nor have I ever seen any instance in which bad results followed, except in wet weather. This is very different from what we are taught to believe as regards exposure to the night air in the Atlantic States, with how much reason I cannot pretend to say, never having tried the experiment. The general health of the community is, I believe, of a higher character than in any other part of the Union; the complexion here generally assumes that ruddy tint which is considered, and I think justly, as the most unequivocal sign of a high state of health in individuals of the Anglo-Saxon race. A residence of a few months in the country is almost always accompanied by a marked increase in weight, and there can be no doubt but that the climate is conducive to fertility

in the female. Although I may expose myself to the charge of enthusiasm as regards the climate of this country, yet I am but expressing my candid opinion when I state that I believe California will be found more conducive to the highest physical and intellectual developement of Anglo-Saxon race than any other part of the globe. There is not a day in the year in which the powers of the mind or the body are enervated by heat or numbed by cold. And when the agricultural resources of the country shall become developed, and the swamp land reclaimed and brought under cultivation, I believe that every external influence, detrimental to the preservation of health, will be reduced to a minimum.

Amer. Journ. Med. Sci.

MEDICINE.

ON THE CATARRHAL PNEUMONIA AND LOBAR PNEUMONIA OF CHILDREN.

By MM. Trousseau and Lasegue,

Catarrhal (or lobular) pneumonia is a disease as distinct from simple (lobar) as variola is from erythema. This is seen in their respective mortality. Of twenty children who have been admitted into the hospital clinique suffering from simple pneumonia, in six months all have recovered; of nearly thirty who were attacked with catarrhal pneumonia, not one survived. Most of the first class of cases exhibited an excessive degree of acuteness, which burnt out like a fire of straw; while several of the second, nothwithstanding their fatal termination, commenced with very mild symptoms.

Simple pneumonia hardly ever affects a child below two years of age, and rarely those of two or three, but becomes of more and more frequent occurrence as the child approaches adolescence. Its cause and symptoms resemble those of the adult, with some modifications. After twenty-four or thirty-six hours, the souffle and bronchophony can alone be heard; the crepitant râle, which is often observed in the adult when the patient coughs, even when much souffle is present, is hardly ever heard in the child. So afterwards, from day to day, without the crepitation of resolution, the souffle disappears, leaving only a feeble respiration. The progress of the disease is also more rapid than in the adult. In the mild form of the disease, recovery takes place rapidly, and in large proportion; but in its grave form, many cases are lost by any mode of treatment. M. Trousseau generally bleeds the child, gives it an emetic of sulphate of copper, and then a mixture containing Kerme's mineral and extract of digitalis.

Catarrhal pneumonia commences with a catarrh, which rapidly extends to the small bronchi, and then we hear numerous and small subcrepitant râles disseminated over both lungs, and especially posteriorly. These râles may persist for four, six, eight, or fifteen days, without any souffle becoming manifest; but sooner or later we hear a souffle, the resonance of the cries or the voice, or at least a prolonged respiratory murmur. While these latter sounds, common to simple and catarrhal pneumonia, are thus manifesting themselves, we find by the subcrepitant râles that the capillary catarrh is still persisting in the rest of the lung. The disease has extended from the mucous membrane to the parenchyma of the organ. Febrile action is less than in ordinary pneumonia, being predomi-

nant at some portions of the day and entirely ceasing at others; and these alterations of better and worse may continue for fifteen, twenty, or thirty days; the disease being originally a pulmonary catarrh, and partaking of the obstinacy and uncertainty of catarrhal complaints. As more and more of the parenchyma becomes implicated, the fever becomes more continuous and intense, and the respiration more difficult, until the children die exhausted. In other cases, in which the bronchial phlegmasia was very intense from the first, and the lung became rapidly invaded over a great extent, death takes place with rapidity. The progress of the disease has usually been more rapidly fatal when it has succeeded to measles, chronic disease of the skin, or laryngitis. All means of treatment that have been tried have proved impotent.

These two affections may be compared, *exceptis excipiendis*, with erysipelas and phlegmon. Erysipelas traverses the surface, like the catarrh; and when it persists too long, it induces ulcerations of the skin, furucles, and circumscribed subcutaneous abscess, just as the capillary catarrh induces suppuration of the lobules, little abscesses of the lungs, and circumscribed pneumonias. Simple pneumonia, on the other hand, progresses like simple phlegmon, violent in its febrile reaction, but terminating abruptly and rapidly.

It must not be supposed, from what has been said, that catarrhal pneumonia is almost invariably fatal. Although this is the case amidst the miasmata of an hospital, which exert effects at once so terrible and so difficult to avert, it is not so in private practice. In this, one-half the patients may be cured by repeated vomiting, flying blisters, antimonials, and digitalis; but how terrible are the ravages of a disease which, under the most favourable circumstances, kills one-half its subjects!—*Brit. and For. Med. Chir. Rev.*

SENILE GANGRENE TREATED BY LOCAL BLEEDING.

By Mr. Cowley of Winslow.

On Monday, May 17, 1852, I (being in the 74th year of my age) was attacked with an uneasy sensation in the second toe of my right foot, occasionally paining me like the sting of a nettle. Upon examination, the whole of the toe, to the first metatarsal joint, appeared of a red and inflamed colour, slightly tender to the touch, and somewhat enlarged. An application of diluted tincture of iodine produced no sensible effect. A second dressing of the same was applied on the 18th. On the 20th, the whole toe was considerably enlarged, vesication had taken place all along its dorsum, and the colour of the toe was assuming a dark purple hue. The iodine was discontinued, and ceratum album with a spirituous lotion were substituted, with the use of Markwick's epithem, until the 23rd, when the fourth and fifth toes became inflamed like unto the second, at its commencement, with a slight swelling without pain, but some irritation. From the fatal results I had witnessed of various cases during an apprenticeship and practice of fifty-nine years, and which cases had commenced in a similar manner to my own, I felt convinced (as no injury had been inflicted) that the disease was "senile mortification," and at once resolved to deviate from the long-continued practice of trusting for a cure to the use of cataplasms, opium, &c. (which rarely arrested the disease in its incipient stage, and seldom succeeded after sphacelus had taken place,) by trying the effect of local bloodletting. I therefore extracted eight ounces and two drachms of blood from the saphena minor vein immediately

below the ankle-joint. The direct beneficial effect produced was truely surprising; the redness of the fourth and fifth toes was almost removed, and the dark shining appearance of the second considerably improved in colour, and the swelling diminished. The same dressings were continued till the 28th, when a relapse took place, the fourth and fifth toes became inflamed to an equal extent with the third, the second toe was also increased in size, and the colour darker; also the great toe showed decided signs that the disease was spreading to that part of the foot. I again took away eight ounces of blood, and this from the saphena major vein, situated over the middle of the first metatarsal bone. A similar result to the first bleeding was shown, the same mode of dressings was continued to the 2nd of June, when all the symptoms of the disease recurred, but not in so aggravated a degree. Five more ounces of blood were extracted from the same vein, three inches above the ankle-joint, with equally good effect, making a total loss of 20 ounces of blood from the foot in the space of ten days.

The inflamed parts were now dressed with spt. terebinthinæ, previous to the application of white cerate and epithem, but not agreeing, a common bread-and-water poultice was tried. However, not finding any material alteration, the cataplasma fermenti, P.L., was the next remedy on the 12th of June, which was continued for six days with advantage, when, although no ulceration existed, the ung.. resinæ was applied over the surface of the whole foot as a warm dressing up to the present time: and now I hope a permanent cure is effected.

During the first fortnight of the above period, the saline mixture with the volatile alkali were freely taken, and since then quin. sulph. gr. v., bis die. Port wine and spirituous stimuli have been continued to this day, without increasing the number of pulsations beyond 66, and that only in the afternoon occasionally, Although particularly enjoined by my medical friends to adopt positive and constant rest to my foot, I have persisted in the use of a leg rest and exercise in a garden chair; my general health being very good, I considered fresh air and exercise essential to its maintenance.—*Prov. Med. Jour.*

CASE OF DISEASE OF THE AORTA, AND HYPERTROPHY OF THE LEFT VENTRICLE OF THE HEART.

WITH REMARKS.

By John Tropham, M.D., London.,

Physician to the South Staffordshire General Hospital, Wolverhampton.

M. N———, thirty-one years of age, is employed as a labourer in the coal mines, and from the nature of his occupation is continually exposed to mechanical injury, from the falling-in of portions of the roof of the subterranean galleries in which he works, and has from time to time received severe contusions about the thorax from this cause. He refers his present sensations to the consequences of a fall of coal upon his chest fifteen weeks previously to my first seeing him, as ever since that accident he has complained of difficulty of breathing, and has been sensible of a continual *beating sensation* in his chest, any act requiring great muscular exertion occasioning palpitation of the heart. During the last three weeks, violent cephalalgia and a slight cough have been added to the above symptoms. He is a married man, of temperate habits; has never had rheumatism; and the only severe illness he can recollect having undergone

occurred three years ago, when he suffered from hæmoptysis to a considerable extent, the blood expectorated being of a dark colour.

Physical signs.—The thorax is well-formed generally, but there is an evident *vaulting* over the region occupied by the heart. A measurement of the chest made in the situation of the fifth and sixth ribs, demonstrates that the left exceeds the right side of the thorax in its dimensions by about three-fourths of an inch. (The patient is right-handed.) On the right side of the chest, in the region corresponding to the articulation of the second and third ribs, there is a pulsation synchronous with that of the heart; and a similar phenomenon is perceptible in front of the neck, between the sternal articulation of the cavities There is dullness on percussion, extending from the left nipple downwards to the lower margin of the last true rib, and laterally to the left margin of the sternum. Over all this space, the performance of the act of percussion occasions pain to the patient. The right side of the chest is naturally reasonant, except over a portion of about an inch square, situated over the sternal attachment of the second and third ribs. In this locality the fingers discover a pulsation co-incident with that of the heart; the motion communicated is, however, *double*, the second impulse being longer in duration than the preceding one. A distant *fremissement* accompanies the second of these pulsations. Upon placing the head over the cardiac region, a strong heaving impulse is felt. Auscultation practiced over the front of the thorax demonstrates the respiratory sounds to be natural, save in the situations mentioned as existing on either side; for in these places there is an absence of respiratory murmur. The first sound of the heart is unusually dull, and the second is accompanied by a slight blowing murmur, so protracted, however, as to render this longer by one-half than the first sound. There is no vascular murmur observable in the back of the thorax, save a slight *bruit de soufflet* to the left of the first dorsal vertebra. The murmur described as existing to the right of the sternum, is audible in both carotid arteries, and most so in that of the right side; it is also heard between the sterno-clavicular articulation.

I had the patient admitted into the wards of the institution at which he had applied for relief (the Wolverhampton Dispensary) on the 26th of July, 1848; but he left at his own request at the expiration of three days. The diagnosis then made was aneurism of thoratic aorta, with hypertrophy of left ventricle. He came to see me at intervals until the 21st October, 1848, during which period there was little change observable in the physical signs, save that the inequality in the duration of the two cardiac sounds had to a great extent disappeared. He was enjoined to avoid all active pursuits as far as possible, with all excesses in diet, and was ordered a mixture containing tincture of digitalis and sulphate of magnesia.

From October the 21st, 1848, until the time of his decease, May 16th, 1851, he was entirely lost sight of, and the above history and observations are copied from notes made by me at the time.

Post-mortem appearances.—An examination of the contents of the thorax was made on the night of the patient's death. The left ventricle of the heart was found to be greatly enlarged, the corresponding cavity on the opposite side remaining of the natural dimension. The wall of the dilated ventricle was exactly one inch in thickness, measured near the septum ventriculorum; and the weight of the entire heart amounted to two pounds twelve ounces. The division

between two of the aortic sigmoid valves was destroyed, and the two fused, as it were, into one, so that an irregular, buttonhole-looking orifice remained. The other valves of the heart were healthy. The aorta was greatly dilated from immediately above the situation of the semilunar valves to the giving off of the brachio-cephalic trunk. An inch above the aortic valves, on the posterior aspect of the vessel, there was an aneurismal pouch, about the size and shape of a chesnut. There was no rupture of any of the coats of the aorta, but its inner surface was studded with cartilaginous-looking nodules. The particardium was not adherent, but traces of former inflammation of that membrane were shown by the presence of two white patches situated upon its visceral layer, upon the left ventricle, the larger of these being two inches in length. They were not raised above the surface of the heart. Each of the lungs exhibited marks of extensive recent inflammation, the patient having succumbed to an attack of pneumonia after a short illness.

Remarks.—It must ever add to its interest upon our minds, when, after having lost sight of a case of disease for many months, or even for years, the subject of it is again unexpectedly presented to our observation, and the notes and remarks registered, and made upon previous occasions, are rendered capable of being compared, and the accuracy of the views then taken, verified, as was enabled to be done in the instance under consideration.

The complaint under which the patient laboured was probably referred to its true cause by the sufferer himself—that of *mechnnical injury*, coupled with a life passed in laborious exertion. A very parallel instance is thus related in the graphic words of Dr. Latham:

"A man passing through Spa-fields one night was unmercefully beaten and plundered; and thrown into a ditch, and left to die. Die, however, he did not, but lay there he knew not how long; for he was insensible. The next day he was found, and taken home. He was disabled by the bruises he had received and by *palpitation* of the heart and dyspnœa, which he had never complained of before, and was never again able to return to his ordinary occupation. After some months he was admitted into St. Bartholomew's Hospital, dropsical, and bearing all the symptoms which denote hypertrophy and dilatation of the heart. He soon died, and his heart was found of a size which was almost incredible. All its muscular substance was enormously amplified, and all its cavities enormously dilated; its precardium, and lining membranes, and valves, free from disease. ("Lectures on Diseases of the Heart," vol. ii. p. 205.)

It has been proved, in various instances, that excessive muscular exertion is capable of occasioning injury to the mechanism of the heart. Thus in Dr. Latham's admirable work, is a case communicated by Dr. Bence Jones, in which one of the septa of the aortic valves "was torn away from its attachments, and thus two of its pouches were reduced to a single irregular one." When acting as house-surgeon to the University College Hospital, London, an instance, occasioned in a similar manner to the above, was shown to me by Mr. Mercer, of Deal; and the precise nature of the disorder has since been ascertained and described by my friend Dr. R. Quain, under whose care the man afterwards was placed. He lived for nearly two years, (this was in 1843), and after his death, Dr. Quain had an opportunity of examining the organs contained within the thorax when it was found that "all the cavities of the heart were enlarged and filled with blood. The arch of the aorta was somewhat dilated; the heart

weighed twenty-two ounces. The chief disease was found at the entrance of the aorta: here it was noticed that the conjointed attachments of two of the valves to the aorta had been separated from the wall of that vessel, and thus those valves were allowed to drop below the level of the third, which retained its connexions." When I saw the patient, the character of the lesion was plainly evident from the loud regurgitant aortic murmur which existed, and which was strongly audible, even along the course of the femoral arteries. This case, with other similar ones, was submitted by Dr. Quain to the Pathological Society of London, October 20th, 1846; and they are all recorded in the *Monthly Journal of Medical Science* for December of that year.

The weight of the heart in the case now under consideration, in place of being eight ounces and a half, as is stated by Dr. Clendinning to be the average in persons aged between thirty and fifty years, amounted to two pounds twelve ounces. It is to be regretted that a misroscopical examination of the morbid deposit observed in the aorta was not made: but owing to some carlessness, the parts were mislaid before I had time to institute such an investigation.

In respect to the physical signs observable during life, these were such as to admit of no doubt as to the nature and seat of the disease. The localization of these signs on the right of the sternum, at the seat of articulation of the second and third ribs, was sufficient to indicate that the ascending aorta was the part implicated. Besides the dullness on percussion and the impulse, the presence of two morbid sounds, neither of which existed prominently over the heart itself, (though there were most unequivocal signs of great disease of that organ,) showed that the origin of these must be referred to something extraneous to the to the great organ of the circulation. The natural resonance on percussion of the right side of the thorax (save over the situation above indicated,) went to prove that there was no tuberculization of the lung, since in that case the dullness would probably have extended up into the acromial angle, *the proper stethoscopic corner*, as it is called by Dr. C. J. B. Williams ("Library of Practical Medicine," vol. iii. p 176.) The necropsy demonstrated that there were no tubercles deposited in the lungs, and thus added another to the instances tending to show that pulmonary tuberclesis and aneurism are not frequent concomitants. The hæmoptysis which had occurred three years previously must be attributed to some other cause than the disease now described; perhaps to that given by the patient—viz., unusual bodily toil, occasioning rupture of some of the pulmonary tissues.

PHYSIOLOGY.

ON THE REPRODUCTION AND SUPPOSED EXISTENCE OF SEXUAL ORGANS IN THE CYPTOGAMOUS PLANTS.

By Arthur Henslow F.L.S.

Having been prevented by the pressure of other engagements from complying with the request which the Association did me the honor to make last year, that I should assist Prof. Lindley and Dr. Lankester in preparing a Report on Vegetable Physiology, I beg to present a fragmentary contribution on the subject, relating to a branch of the sciences to which my attention has been recently

strongly attracted, in the pursuit of my own investigations. I was the more induced to devote the short time at my disposal to drawing up a summary of the state of my knowledge of the reproduction of the higher flowerless plants, by the the importance of the discoveries which have recently been made in this department, tending completely to change the general views which have hitherto been entertained by most botanists as to the extent to which sexuality exists in the vegetable kingdom, and in connection with other new facts relating to the Thallophytes, to indicate that the existence of two sexes is universal.

Under the name of the higher Flowerless Plants, I include all those classes which are distinguished on the one hand from the Thallophytes or Cellular plants by the presence of a distinct stem bearing leaves, and on the other hand from the Monocotyledons and Dicotyledons by the absence of the organs constituting a true flower; they are, the Hepaticæ, Musci, Equisetaceæ, Filices, Lycopodiaceæ, Isoetaceæ, and Marsileaceæ or Rhizocarpeæ.

On no subject has more discussion been maintained than on the existence of sexes among the Cryptogamous families. The discovery of the two kinds of organs, the *antherdia* and *pistillida*, in the Mosses and Hepaticæ, and of the peculiar organs containing analogous spiral filaments in the Characæ, were for a long time the chief facts brought forward by those who supported the sexual hypothesis; and in the endeavour to carry out the view into the other tribes, a similar nature to that of the *antheridia* was attributed to most varied structures in the ferns and other plants. These attempts to find distinct sexual organs were in some instances pursued with so little judgment, that the opinion had of late years fallen in some degree into discredit, and two circumstances contributed still further to strengthen the doubts that were entertained. The first was the exact analogy, pointed out by Prof. Von Mohl, between the mode of development of the spores of the Cryptogamia and the pollen grains of the flowering plants, which interferred very importantly to prevent any comparison between the sporangia and ovaries, and apparently determined the analogy of the former to be with anthers. The second was the discovery by Prof. Nageli, of organs producing spiral filaments, therefore analogous to the *antheridia* of the mosses, on the germ from, or *pro-embryo* developed from the spores of the ferns.

At the same time, the facts observed in *Pilularia* were altogether eqivocal. Mr. Valentine traced the development of the larger spores, exhibiting in germination an evident analogy to ovules, from cells closely resembling the parent-cells of pollen and spores; while Prof. Schleiden stated that he observed a fertilization of these supposed ovules by the smaller spores resembling pollen-grains, and thus seemed to remove the ground for attributing a fertilizing influence to the spiral filaments contained in the so-called *antherdia* of the Cryptogams.

In this state the question remained until 1848, when Count Suminsk published his observations on the germination of ferns, showing that the researches of Nageli had been imperfect, and that two kinds of organs are produced upon the pro-embro of the ferns; one kind analogous to the *antherdia*, and the other to the *pistillidia* of mosses; from the latter of which the true fern stem is produced, like the seta and capsule from the same organ in the mosses; further stating that he had actually observed a process of fertilization. Soon after this, M. G. Thuret discovered *antheridia* like those of the ferns of the Equisetaceæ; Nageli had previously published, in opposition to Schleiden's observations, an account of the production of spiral filaments from the small spores of *Pilularia*, and finally M. Mettenius discovered them in the small

spores of *Isoetes*. Thus they were shown to exist in all the families above enumerated, with the exception of the Lycopodiaceæ, in which they have recently been stated to exist by M. Hofmeaster. Before entering into a detailed account of their discoveries, it may be mentioned, that, besides their well-known occurence in the Characeæ, which most authors consider as Thallophytes, antheridia are stated by Nageli to exist in the Florideæ, among the Algæ; and peculiar bodies to which the same nature has been attributed, were recently discovered by M. Itzigsohn in the lichens; a discovery confirmed by Messrs. Tulanse, who state that analogous bodies exist in many fungi Our knowledge of these latter points is, however, far less definite than that concerning the higher tribes, and I shall not include them in the followsng summary.

One of the most remarkable circumstances concerning the antheridia of the leaf-bearing cryptogams is the very varied nature of the time and place of their development; so great indeed is this, that it is only their essential structure, and the production of the moving spiral filaments in particular, which warrants the assumption of their identity of function in the different families. In order to make these observation clearly comprehensible, it will be necessary to describe the characters exhibited in the germination of the spores in each tribe, as it is only by this means that the important peculiarities of each case can be made evident. It will be most convenient to give a separate sketch of all that is know of the process of reproduction in each family, taking these separately and in succession; after this we shall be in a position to compare them together, and trace out their differences and analogies; the advantage of recalling all the essential facts to memory, will, I trust, serve as an apology for the introduction of much that is already familiar to Botanists.

Mosses.—The antheridia of the mosses occur in the axils of the leaves or collected into the head, enclosed by numerous variously modified leaves, at the summit of the stem. They are produced either on the same heads as the pistillidia, or in distinct heads on the same individuals, such mosses being called monœcious; or the heads are found only on distinct individuals, such mosses being termed diœcious. The structure of the antheridium is exceedingly simple; it consists of an elongate, cylindrical or club-shaped sac, the walls of which are composed of a single layer of cells, united to form a delicate membrane. Within this sac are developed vast numbers of minute cellules, completely filling it, and, the sac bursting at its apex at a certain period, these vesicles within appear to absorb water, and swell so as to burst the sac of the antheridium, and often adhering together, they collectively appear to form masses larger than the cavity from which they have emerged. Through the transparent walls may be seen a delicate filament with a thickened extremity, coiled up in the interior of each vesicle. Often before the extrusion, but always shortly after, a movement of this filament is to be observed when the object is viewed in water under the microscope. The filament is seen to be wheeling round and round rapidly within the cellule, the motion being rendered very evident by the distinctness of the thickened extremity of the filament, which appears to be coursing round the walls of the cellule in a circle. According to Unger, this filament breaks out of its parent cellule in Sphagnum, and then appears as a spiral filament moving freely in water, in fact, as one of the so-called spermatozoa.

The pistillidia of the mosses are the rudiments of the fruit or capsules. When young, they appear as flask-shapen bodies with long necks, composed of a simple cellular membrane. The long neck presents an open canal like a style,

leading to the enlarged cavity below, at the base of which, according to Mr. Valentine, is found a cell projecting free into the open space. This single cell is the germ of the future capsule; at a certain period it becomes divided into two by a horizonal partition, the upper one of these two again divides, and so on until the single cell is developed into a cellular filament, the young seta; the upper cells are subsequently developed into the urn and its appendages, and as this rises, it carries away with it, as the calyptra, the original membrane of the pistillidium, which separates by a circumscissile fissure from the lower part, the future faginnula. These observations of Valentine are not exactly borne out by those of Shimper in some of the detail points. According to this author, the lower part of the pistillidium (the germen of Dr. Brown) begins to swell at a certain time, when a capsule is to be produced, becoming filled with a quantity of what he terms "green granulations." As soon as the thickness has become about that of the future seta, the cell-development in the horizontal direction ceases, and its activity is directed chiefly to the upper part, which begins to elongate rapidly in the direction of the main axis. This elongation causes a sudden tear off at the base, or a little above it, of the cell-membrane enveloping the young fruit, and the upper part is carried onwards as the calpytra; the lower part when any is left, remains as a little tubular process surrounding the seta. While the young fruit is being raised up by the growth of the seta, the portion of the receptacle upon which the pistillidium is borne, becomes developed into a kind of collar, and at length into a sheath (the vaginula) surrounding the base of the seta which is articulated into it there.

M. Hofmeister again describes the details much in the same way as Mr. Valentine. He states that there exists at the point where the 'style' and 'germen' of the pistillidium join, a cell, developed before the canal of the style has become opened. In those pistillidia which produce capsules this cell begins at a certain period to exhibit very active increase; it becomes rapidly divided and subdivided by alternately directed oblique partitons into a somewhat spindle-shaped body formed of a row of large cells. Meanwhile the cells at the base of the germen are also rapidly multiplied, and the lower part of the pistillidium is greatly increased in size. The spindle-shaped body continues to increase in length by the subdivision of its uppermost cell by oblique transverse walls, and the opposition which is offered by the upper concave surface of the cavity of the germen, causes the lower conical extremity of the spindle-shaped body to penetrate into the mass of cellular trssue at the base of the germen, a process which resembles the penetration of the embryo into the endosperm in the embryo-sac of certain flowering plants. The base of the spindle-shaped body, which is in fact the rudiment of the fruit, at length reaches the base of the pistillidium, and penetrates even some distance into the tissue of the stem upon which this is seated. The growth of the upper part going on unceasingly, the walls of the germen are torn by a circular fissure and the upper half is carried upwards, bearing the calyptra, the lower part forms the vaginule. The upper cell of the spindle-shaped body then becomes developed into the capsule, and the calyptra often becoming originally connected with this, as the base of the seta does with the end of the stem, it in such cases undergoes further develnpement during the time it has been carried up by the growing fruit.

The view now entertained by Schimper, Hofmeister, and others, of the reproduction of the mosses is, that the antheridia are truly male organs, and that they exert, by means of the spiral filaments, a fertilizating influence upon the

pistillidia, it being assumed that those bodies, or the fluid which they are bathed in, penetrate down the canal of the style or neck-like portion of the pistillidium to reach the minute cell, the supposed embryonal cell, situated in the globular portion or 'germen' of the pistillidium, and thus render it capable of becoming developed into a perfect fruit.

No such process of fertilization has actually been observed in the mosses, and therefore all the evidence is at present merely circumstantial; but this is very strong. In the first place it is stated as an undoubted fact by Schimper and Bruch, that in the dicœcious mosses, those on which the antheridia and pistillidia occur in separate plants, fruit is never produced on the so-called male plants, and never on the so-called female unless the males occur in the vicinity; several examples are cited in the work of Schimper above referred to; when the sexes occur alone, the increase of the plant is wholly dependent on the propagation by gemmæ or inovations.

By the discovery of the antheridia and pistillida in the other higher Cryptogams, the arguments from analogy greatly strengthen the hypotheais of the sexuality of mosses.

Further obsevation is required, then, for the direct proof of the occurrence of a process of fertilization in the mosses; but the facts now before us all tend to prove their sexuality if we argue from analogy, and the probabilities deduced from the negative evidence above referred to in regard to the dicœcious species.

It is unnecessary to give any account of the well-known structure of the moss capsules; yet in order to render the comparison with the phenomena of the life of the mosses with those of the other leafy Cryptogams complete, it may be worth while to allude to the germination of the *spores*. The spore is a single cell, with a double coat, like a pollen-grain; this germinates by the protrusion of the inner coat in the form of a filamentous or rather tubular process, which grows out and becomes subdivided by septa so as to form a confervoid filament. The lateral branches bud out from some of the cells, some elongating into secondary filaments, others at once undergoing a more active developement, and by the multiplication of their cells, assuming the condition of conical cellular masses, upon which the forms of moss leaves may soon be detected; these cellular masses becoming buds from which the regular leafy stems arise.

Hepaticæ.—The genera comprehended in this family present a wonderful variety of structure in the reproductive organs, but in almost all of them the existence of the two kinds of organs called pistillidia and antheridia have long been demonstrated, and in most cases the developement of the sporangia from the so-called pistillidia has been traced. In those genera in which the plants most resemble the *mosses* in the vegetative portion, as in *Jungermanniæ*, the pistillidia are very like those of the mosses; this is also the case in *Marchantia*; but in *Pellia*, *Anthoceros*, and other genera, the rudiment of the sporangium bears a striking resemblance to the so-called ovules of the Ferns, Rhizocarpæ, &c., occurring upon the expanded fronds very much in the same way as those bodies do upon the pro-embryo of the said familids. It would occupy two much space to enter into a minute detail of the various conditions that are met with. It is sufficient to say that in all cases the physiological stages are analogous to those of the mosses; since the pistillidia produced upon the fronds or leaf-bearing stems developed directly from the spores, go on to produce a *sporangium alone*, in which the new spores are developed, without the intervention of the

stage of existence presented by the pro-embryo of the Ferns and Equisetaceæ, where the pistillidia and and antherida occur upon a temporary frond, and the former give origin to the regular stem and leaves of the plant.

Ferns.—This class formed for a long time the great stumbling-block to those who sought to demonstrate the existence of sexuality in the Cryptogamous plants. The young capsules were generally considered to be the analogues of the pistillidia of the mosses, and the young abortive capsules which frequently occur among the fertile ones were supposed by some authors to represent the antheridia. Mr. Griffith, shortly before his death, noticed a structure which he was inclined to regard as the analogue of the antheridium in certain of the ramenta upon the petioles.

In the year 1844, Prof. Nageli published an account of his observations on the germination of certain ferns, and announced the discovery of moving spiral filaments closely resembling those of the Charæ, on certain cellular structures developed upon the pro-embryo or cellular body first produced by the spore. It is not worth while to enter into an analysis of his observations, as they have since been clearly shown to have been very imperfect; it is sufficient to state that he only described *one* kind of organ, and from his description it is evident that he confounded the two kinds since discovered, regarding them as different stages of one structure. The announcement of this discovery seemed to destroy all ground for the assumption of distinct sexes, not only in the ferns but in the other Cryptogams, since it was argued that the existence of these cellular organs, producing moving spiral filaments, the so-called spermatozoa, upon the germinating fronds, proved that they were not to be regarded as in any way connected with the reproductive process.

But an essay published by the Count Suminski in 1848, totally changed the face of the question, and opened a wide field for speculation and investigation on this subject, just as it was beginning to fall into disfavor. Count Suminski's paper gives a minute history of the course of development of the ferns from the germination of the spore to the production of the regular fronds, and he found this development to exhibit phenomena as curious as they were unexpected. The cellular organs seen by Nageli were shown to be of two perfectly distinct kinds, and moreover to present characters which gave great plausibility to the hypothesis that they represented reproductive organs; moreover, thisauthor expressly stated that he had obtained absolute proof of sexuality by observing an actual process of fertilization to take place in the so-called ovules, through the agency of the spiral filaments or spermatozoa.

The main points of his paper may be briefly summed up as follows. The fern spore at first produces a filamentous process, in the end of which cell-development goes on until it is converted into a Marchantia-like frond of small size and exceedingly delicate texture, possessing hair-like radicle-hairs on its under side. On this under side become developed, in variable numbers, certain cellular organs of two distinct kinds. The first, which he terms antheridia, are the more numerous, and consist of somewhat globular cells, seated on and arising from single cells of the cellular marchantia-like frond. The globular cell produces in its interior a number of minute vesicles, in each of which is developed a spiral filament, coiled up in the interior. At a certain epoch the globular cells burst and discharges the vesicles, and the spiral filaments moving within the vesicles at length make their way out of them and swim about in the water, displaying a spiral or helical form, and consisting of a delicate filament with a

thickened cavate extremity; this, the so-called head, being said by Count Suminski to be a hollow vesicle, and to be furnished with six or eight cilia, by means of which the apparently voluntary movement of the filament is supposed to be effected.

The second kind of organ, the so-called "ovules," are fewer in number and present different characters in different stages. At first they appear as little round cavities in the cellular tissue of the pro-embryo, lying near its centre and opening on the under side. In the bottom of the cavity is seen a little globular cell, the so-called embryo-sac. It is stated by Count Suminski that while the ovule is in this state one or more of the spiral filaments make their way into the cavity, coming in contact with central globules cell. The four cells bounding the mouth of the orifice grow out from the general surface into a blunt cone-like process, formed of four parallel cells arranged in a squarish form and leaving an intercellular canal leading down to the cavity below. These four cells become divided by cross septra, and grow out until the so-called ovule exhibits externally a cylindrical form, composed of four tiers of cells, the uppermost of which gradually converge and close up the orifice of the canal leading down between them. Meanwhile the vesicular head of one of the spiral filaments has penetrated into the globular cellule or embryo-sac, enlarged in size and undergone multiplication, and in the course of time displays itself as the embryo, producing the first frond and the terminal bud whence the regular fern stem is developed. In considering the import of these phenomena, the author assumes the analogy here to be with the process of fertilization in flowering plants as described by Schleiden, regarding the introduction of the embryo from the vesicular head of the spermatozoa as representing the production of the phanerogamous embryo from the end of the pollen tube after it has penetrated into the embryo-sac.

The promulgation of these statements naturally attracted great attention, and since they appeared we have received several contributions to the history of these remarkable structures, some confirmatory, to a certain degree, of Suminski's views, others altogether opposed to them.

In the early part of 1849, Dr Wigand published a series of researches on this subject, in which he subjected the assertions of Suminski to a strict practical criticism; the conclusions he arrived at were altogether opposed to that author's views respecting the supposed formation of the organs, and he never observed the entrance of the spiral filaments into the cavity of the so-called ovule.

About the same time M. Thurett published an account of some observations on the antheridia of ferns. In these he merely confirmed and corrected the statements of Nageli respecting the antheridia, and did not notice the so-called ovules.

Towards the close of the same year, Hofmeister confirmed part of Suminski's statements and opposed others. He stated that he had observed distinctly the production of the plant (or rather the terminal bud for the new axis,) in the interior of the so-called "ovule," but believed the supposed origin of it from the end of the spiral filament to be a delusion. He regards the globular cell at the base of the canal of the "ovule" as itself the rudiment of the stem, or embryonal vesicle (the embryo originating from a *free* cell produced in this), analogous to that produced in the pistillidia of the mosses. He also describes the development of the ovule differently, saying that the canal and orifice are

opened only at a late period by the separation of the contiguous mass of the four rows of cells.

About the same time appeared an elaborate paper on the same subject by Dr. Hermann Schacht, whose results were almost identical. He found the young terminal bud to be developed in the cavity of one of the so-called ovules, which were developed exactly in the same way as the pistillidia of the mosses. He stated also that the cavity of the "ovule" is not open at first, and he declares against the probability of the entrance of a spiral filament into it, never having observed this, much less a conversion of one into an embryo.

In the essay of Dr. Mettenius already referred to, an account of the developement of the so-called ovules is given. His observations did not decide whether the canal of the "ovule," which he regards as an intercellular space, exists at first, or only subsequently, when it is entirely closed above. Some important points occur in reference to the contents of the canal.

The contents of the canal in a mature condition consist of a continuous mass homogeneous, tough membrane, in which fine granules, and here and there large corpuscles, are embedded. It reaches down to the globular cell or 'embryo sac,' and is in contact with this. This mass fills the canal or diminishes in diameter from the blind end of the canal down to the 'embryo-sac;' in other cases it possesses the form represented by Suminski, having a clavate enlargement at the blind end of the canal, and passing into a twisted filament below. In this latter shape it may frequently be pressed out of isolated 'ovules' under the microscope, and then a thin transparent membrane-like layer was several times observed on its surface. In other cases the contents consisted of nucleated vesicles, which emerged separately or connected together.

The embryo-sac consists of a globular cell containing a nucleus, and this author believes that the commencement of the development of the embryo consists in the division of this into two, which go on dividing to produce the cellular structure of the first frond.

With regard to the contents of the canal the author says,—

"Although I can give no information on many points, as in regard to the origin of the contents of the canal of the 'ovule,' yet my observations on the developement of the 'ovule' do not allow me to consider them, with Suminski, as spiral filaments in the course of solution; just as little have I been able to convince myself of the existence of the process of impregnation described by that author. It rather appears to me that the possibility of the entrance of the spiral filaments and the impregnation cannot exist until the tearing open of the blind end of the canal in the perfectly-formed ovule, as after the opening of the so-called 'canal of the style' in the pistillidia in the mosses."

Another communication has been furnished by Dr. Mercklin, the original of which I have not seen, but depend on analyses of it published in the 'Botanische Zeitung,' and the 'Flora' for 1851, and further in a letter from Dr. Mercklin to M. Schacht, which appeared in the 'Linnæa' at the close of last year.

He differs in a few subordinate particulars from M. Schacht in reference to the developement and structure of the *prothallium* or pro-embryo, and of the antheridia and spiral filaments; but these do not require especial mention, except in reference to the vesicular end of the spiral filament described by Schacht, which Mercklin regards as a remnant of the parent vesicle, from which

the filament had not become quite freed. The observations referring to the so-called ovule and the supposed process of impregnation are very important; they are as follows:

"1. The spiral filaments swarm round the 'ovule' in numbers, frequently returning to one and the same organ.

"2. They can penetrate into the 'ovule.' This was seen only three times in the course of a whole year, and under different circumstances; twice a spiral filament was seen to enter a still widely open young 'ovule,' then come to a state of rest, and after some time assume the appearance of a shapeless mass of mucilage: the third case of penetration occurred in a fully-developed 'ovule,' through its canal; it therefore does not seem to afford evidence of the import of the spiral filament, but certainly of the possibility of the penetration.

"3. In the tubular portion of the 'ovule,' almost in every case, peculiar club-shaped, granular mucilaginous filaments occur at a definite epoch, these filaments, like the spiral filaments, acquiring a brown colour with iodine. These mucilaginous bodies sometimes exhibit a twisted aspect, an opake nucleus, or a membranous layer, peculiarities which seem to indicate the existence of an organization.

"4. These club-shaped filaments are swollen at the lower capitate extremity, and have been found in contact with the 'embryo-sac' or globular cell which forms the rudiment of the future frond.

"5. The spiral filaments which cease to move and fall upon the prothallium, are metamorphosed, become granular and swell up."

Hence the author deduces the following conclusions:

"That their clavate filiform masses in the interior of the 'ovule' are transformed spiral filaments, which at an early period, while the ovule was open have penetrated into it; which leads to the probality that—

"1. The spiral filaments must regularly penetrate into the 'ovules,' and

"They probably contribute to the origin or development of the young fruit frond (or embryo). In what way this happens the author knows not, and the details given on this point by Count Suminski remain unconfirmed facts.

An important point in this essay is the view the author takes of the whole process of development in this case. He regards it as not analogous to the impregnation of the Phanerogamia, since the essential fact is merely the development of a *frond* from one cell of the *prothallium*, which he considers to be merely one of the changes of the individual plant; while all the other authors who have written on the subject, with the exception of Wigand, call the first frond, with its bud and root, an *embryo*, and regard it as a new individual, or at all events a distinct member of the series of form constituting collectively the representatives of the species.

Finally, Hofmeister, in his notice of this essay in the 'Flora,' declares that the developement of the so-called 'embryo' or first frond commences, not by the subdivision of the globular bell or 'embryo-sac, but by the development of a free cell or 'embryo vesicle' in this, like what occurs in the embryo-sac of the Phanerogamia; and he asserts that this is the first stage of the development from the globular cell in all the vascular Cryptogams, including that found in the pistillidia of the mosses.

Equisetaceæ.—The first discovery of the analogy between the developments from the spore in germination, in the Ferns and Equisetaceæ, is due to M. G. Thuret, who saw the spores of the latter produce a cellular pro-embryo

somewhat like that of the ferns, and in this were developed antheridia of analous structure, emitting cellules containing many spiral filaments.

This announcement was confirmed by M. Midle, whose observations extended over some months, during which time no 'ovule' was produced, but he saw what appeared to be the rudiment of one. Dr. Mettenius states that he has met with decaying 'ovules' precisely like those of the ferns, upon the pro-embryo of an equisetum, and thus the evidence is completed, so far as the occurrence of the kinds of organs is concerned.

PRACTICAL REMARKS ON DISEASE OF THE HEART, AND THE DROPSY FOLLOWING IT.

By Alexander Kilgour, M.D., one of the Physicians to the Aberdeen Infirmary.

In the thirty-ninth Number of this Journal, or that for September, 1843, I published some short "Notes on Diseases of the Heart," in which I stated as the result of my experience—but submitting my opinion with every deference to that of others—that the "*bruit de soufflet*" is of value as a sign to us, when permanently present, that disease does exist *somewhere* in the heart, pericardium, or large vessels, but that it is no sure guide to the special site of the lesion.

It was my conviction that authors, and particularly young authors, who meditated much on their cases at home, and were so ready to explain everything on mechanical principles, were apt to refine a little too much, and lay too great stress on a diagnosis from the abnormal sounds, call them by what name they may, alone.

Nearly seven years' longer experience, in which I have seen many additional cases of heart disease in hospital and private practice, has not brought me to materially alter that opinion. I allow that we may often make a pretty good *guess* as to the seat of the disease, but that is nearly all. "Guesses at truth," however, are often interesting and even instructive; and as an easy mode of impressing on my students in the clinical class the usual views, or guesses, as to the seat of the morbid sounds of the heart, and probable condition of the parts causing them, I have prepared a diagram. I have reason to think it has been convenient, for I have been applied to for copies of it by several of my medical brethren. There are not many—few, perhaps, but those who make heart diseases their hobby in their closets or in practice—that can always carry in their minds, or bring to their recollection, the seats and causes of the abnormal sounds of the heart as given by the writers on these diseases.

When so much has been written on the treatment of the diseases of the heart, it may be something like going over a more than twice-told tale to offer any remarks on that subject. I shall, however, be brief, and the want of novelty will perhaps be atoned for by the fact, that I speak from the results of considerable experience, and that I offer these observations to the young practitioner chiefly.

If any one were to ask me, "What can you do to cure disease of the heart?" I would honestly answer at once, "Nothing." But if any one were to ask, "What can you do to ward off the symptoms or feelings that too often become concomitants of that disease?"—I would say that, next to quietness of mind and body, and equable temperature, and moderate or rather low living, I know nothing equal to the use of small doses of colchicum wine, weak saline purges,

and inserting a seton over the region of the heart. It will not do to trust to these when the severe sequelæ of heart disease—distressing dysynœa, hæmoptysis, or dropsy—have more than once occurred; but I know no means equal to them in warding off these latter. I could mention several instances where this treatment has proved most beneficial. One case has been repeatedly under my notice. It was a female, a servant, past the middle period of life, who applied first for a slight attack of anasarca, arising from heart disease. The effusion was soon removed, and she was sent out of hospital with a seton in the side. She kept it open for some time; but, thinking herself well, withdrew it. In a few days after she returned, begging that it might be re-opened,—which was done; and painful experience has more than once taught this woman, in the course of eight years, the necessity of keeping up a discharge from a seton, renewed when necessary, and aiding its influence by the occasional use of the above medicines.

It has often been doubted whether blood should be abstracted in cases of advanced heart disease in elderly persons, when labouring under a sudden paroxysm of violent dyspnœa, giving the feeling of almost immediate suffocation. No doubt, mustard poultices over the heart, and the immersion of the feet and legs in hot water holding some mustard in it, will give some relief; but no means will be found equal to abstracting a small quantity—from two to four ounces—of blood from the arm.

In the treatmeant of cardiac dropsy, every practitioner has his favourite remedy, taken from the class chiefly of diuretics or purgatives, or both Of the former, I have found the following combination the most efficient. It pumps the patient out, so to speak, sometimes in a few hours; and it often will do so in repeated attacks of the anasarca.

℞ Infusi Digitalis, ℥iv. Acetatis Potassæ, ℥ij. Spiritus Ætheris Nitrosi, ℥ij. Aquœ Cassiæ, ℥iss. Capiat cochleare magnum quartâ quâque horâ.

At last there comes an attack in which this and other diuretics cease to act, and we must then fall back on purgatives. Of the latter, unquestionably the most powerful is elaterium. But there surely must be a very great diversity in the strength of this medicine. Some practitioners, from the days of Sydenham, and long before him, downwards, appear to have given it in the dose of two grains, or even more; but I have found a single pill, according to the following formula, generally very powerful:—

℞ Elaterii, gr. j. Extracti Colocynthidis Comp., ℈ij. ss. Extracti Hyoscyami, gr. xij. M. Divide in pilulas xij. Capiat unam nocte maneque.

The great objection to the elaterium is the intense sickness, even in this small dose, produced by it. Do the large doses produce less sickness than the smaller? It may be so; but, in the few instances in which I have tried large doses, the sickness was not less. Is there any mode by which this sickening property in this valuable medicine could be removed? The same effect was found by the ancients in the well-known and much-used, but undeservedly now almost discarded, hellebore; and for the sickness occasioned by it we find it recommended (*vide* "Oribasius, lib. viii, cap. v.) that, amongst other remedies, the patient should be entertained with a funny little story, or be tossed, like Sancho Panza, in a blanket. Something more efficient than the former, and less disturbing than the latter, would be a desideratum.

It is the opinion of some that the elaterium acts as a diuretic, as well as

a hydragogue catharitic. I remember, when in consultation with Dr. Adams, of Banchory, in a case of cardiac dropsy, having my attention called by him to a formula, where the elaterium was combined with a diuretic, which he had seen prescribed with very great success. I detected it at once as a formula given in Ferriar's valuable "Medical Histories." It acts most powerfully by stool and urine (being composed of several of the most powerful of the diuretics, along with the elaterium); but I always found it to cause much and violent sickness.

℞ Extracti Elaterii, gr. ij. Spiritus Ætheris Nitrosi, ʒij. Tincturæ Scillæ. Oxymellis Colchici, āā ʒss. Syrupi Rhamni, ʒi. M. Ft. Solutio. Capiat drachmam unam ex aquæ pauxillo, ter, quarterve in die.

The combination of a bitter purgative with a saline one composed of the vegetable alkali and a vegetable acid, is in my experience much more efficient than any single purgative, or than a bitter with a salt formed of a mineral acid.

The old compound powder of jalap is a well-known instance of a mixture of this kind, and is still one of our best purgatives in all dropsies where this class of medicine may be suitable. Ferriar used, as did also Home, a combination of half an ounce of the bitartrate of potass with two grains of gamboge. The infusion of senna with bitartrate of potass is also an old-fashioned and valuable remedy; but the insolubility of the salt is an impediment to the efficiency of this formula. The senna infusion with tartrate of potass, or with the tartrate of potass and soda, is not liable to the same objection; and the advantage of the frequent use of this combination in cardiac disease having a tendency to dropsy, or in the dropsy itself attending that complaint, has been in my hands, and those of my brethren to whom I have recommended it, so unequivocal, that I can speak for it in the highest terms.

The preparations of mercury have proven, no doubt, very successful in the treatment of this form of dropsy, and consequently many practitioners give them a prefereuce. Without wishing at all to detract from the merits of this valuable agent, I must confess that, in chronic diseases of the heart, I have the same objection to it, and founded on the same grounds, as was that to the celebrated Dr. Fell, or, to use the more classic words of Martial,—

> "Non amo te, Sabidi, nec possum dicere quare;
> Hoc tantum possum dicere, non amo te."

I do not like the mercury, and cannot speak from experience of its efficacy in cardiac dropsy.

There comes a time in the treatment of this complaint when not only diuretics in all forms, but even purgatives cease to remove or even keep in check the anasarca. And this brings me to speak of another mode of treatment, which often proves palliative for a time,—viz., puncturing thr lowering extremities, and thereby draining off the fluid.

This is not a new mode of treating the disease, though it has at various times fallen into unmerited neglect, and perhaps at the present time more so than any other. Freind, in his "Historia Medicinæ," refers to the passage in Ætius which treats of this method of curing dropsy. Ætius is quoting from Asclepiades, and says that "an incision is to be made in the internal part of the leg, about four finger's breadth above the ankle, and that it is to be of the same depth as in venesection. A small quantity of blood flows first, and then there is a continuous discharge of water; and, without inflammation, the

wound remains open until the whole dropsy has run off, no internal medicine being used." * * *

Mead commences his notice of the treatment of dropsy ("Monita," p. 130: Lond., 1771) with an account of this operation. He directs an incision to be made, two fingers' breadth above the ankle, down to the cellular membrane, and no further; and he orders the leg to be fomented with a decoction of emollient herbs, to which some spirits of wine and camphor have been added. He tells us that he has often found this mode of treatment, not only in this disease (anasarca), but also in ascites, of great service, and sometimes curative, the water running out for many days to an extent to exceed all belief. He carefully cautions us to support the patient's strength under a serous discharge from this or any other wound. He then gives a case, apparently hopeless, of anasarca, combined with ascites, where, by a wound made in this manner in each leg, followed by a combination of bitters, squills, and such purgatives as elaterium, calomel, and jalap, the patient recovered, and died five years afterwards of another disease.

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As to another topical means of treatment in this disease—viz., the application of blisters to the anasarcous legs, with the view of draining off the serum—I would have scarcely thought it necessary to speak, believing that almost none, now-a-days, would adopt this practice; but lately I met a very intelligent country practitioner, who told me that he occasionally had recourse to this application. Sydenham has condemned the practice, and branded it as a favorite application of empirics; and he states that "blisters entirely extinguish the natural heat, already almost overpowered by the water and deficiency of the animal spirits, and bring on a gangrene—too common in such cases." Sydenham's authority (and, by the way, he also condemns acupuncture) is great; but I have seen a small vesicle, which had formed on one of the lower limbs, burst and drain off the whole serum from a person affected with extensive anasarca. The case alluded to is one of interest otherwise, though more properly suited to an article on renal dropsy. The patient, a lady, had the most albuminous urine I ever examined. Every remedy for dropsy had been tried by her medical attendant but mercury, and it was only left to me to suggest that, before the patient died, this medicine, which I had never seen more dangerous in this form of dropsy, as has been asserted by writers, than in any other, should be cautiously tried. This was done. Two days afterwards the vesicle above alluded to showed itself and burst, and our patient, much to our surprise, recovered, and is still in good health—seven years after the above attack. What nature does may not always be successful in the hands of art; and the exciting of a vesication, by means of a blister, is much more likely to be followed by dangerous, than by curative, effects in all anasarcous limbs, from whatever cause the dropsical effusion may have arisen.

MIDWIFERY,

AND THE DISEASES OF WOMEN.

ON THE DISTRIBUTION OF THE NERVES OF THE UTERUS.

By Dr. T. Snow Beck.

Before describing the distribution of the nerves of the uterus, vagina, and and neighbouring organs, it is necessary briefly to recal the constitution of the sympathetic nervous system, as attention to this is essential to the full understanding of the subject.

The sympathetic nervous system is described as consisting of three parts: 1. The two gangliated cords which extend down each side of the vertebral column. 2. A series of plexuses in front of the vertebral column, as the cardiac plexus, the solar plexus, the aortic plexus, the hypograstric plexus, &c., from which branches are sent to the different viscera. 3. Small ganglia, dispersed through the cranium, as the ophthalmic ganglia, the otic ganglia, the spheno-maxillary ganglia, the submaxillary ganglia, &c.

This system of nerves is also said to be connected with the spinal nerves by two roots—a white root and a grey root—and to be composed of an admixture of tubular nervous fibres and gelatinous nervous fibres. This description, however, gives a very imperfect idea of the constitution of the sympathetic nervous system.

Passing over the various opinions which have been expressed respecting part of the anatomy, as being foreign to the present object, and as requiring too much space, I will sketch the structure of the sympathetic nervous system, as I believe it to be.

The nerves distributed over the body, to which the name sympathetic is applied, are composed of two kinds of nervous fibres very distinct from each other: the tubular nervous fibre, and the gelatinous nervous fibre. The tubular nervous fibre is derived from the brain and spinal cord, and, in whatever part of the body it is found, can always be traced to one of these organs. The gelatinous nervous fibre has its origin in the various ganglia of the sympathetic, whether these ganglia be situated at the sides of the vertebræ, on the posterior roots of the spinal nerves, in front of the vertebræ, dispersed through the cranium, or elsewhere situated in the body. The branches of nerves which have been called roots of the sympathetic system, are,

(*a*) The white branch, which is a branch from the spinal nerve. It is composed of two parts, one half from the posterior, and the other half from the anterior root of the spinal nerve; then passes inwards to the sympathetic ganglia, and becomes associated with the gelatinous nervous fibre proceeding from it, and these are together distributed to the different viscera.

(*b*) The grey branch, which arises from the ganglion, soon subdivides, and is finally distributed to the parts in the neighbourhood. It is a branch of the usually-called sympathetic system.

Hence there are two distinct systems of nerves; the one formed by the

gelatinous nervous fibre, which, together with the ganglia from which they arise, constitutes the true sympathetic system; the other, composed of the tubular nervous fibre, which, with the brain and spinal cord, forms the cerebro-spinal system. The gelatinous nervous fibre being chiefly distributed to the viscera, is found in greatest quantity near these organs; the tubular nervous fibre, being concerned in sensation and motion, exists in greatest amount in the skin, muscles, &c. These two systems, associated together in varying proportions in the middle part of their course, constitute the sympathetic system as it is generally described. Yet these two systems (although associated in the middle part of their course) are totally distinct from each other at their origin and at their termination. At their origin, where the tubular fibre arises separately from the brain and spinal cord, whilst the gelatinous fibre arises only from the sympathetic ganglia. At their termination, where the tubular fibre separates from the gelatinous fibre, and each is separately distributed to the elements of the organ they are destined to supply. Minute microscopic nerves being found, which consist solely of the gelatinous nervous fibre.

The plexuses, then, in front of the vertebral column, and which furnish nerves to the various viscera, are composed of this association of the two systems of nerves, and to this class belong the aortic plexuses with their prolongation into the pelvis—the hypogastric plexus, the composition and distribution of which it is our special object to examine.

The superior aortic plexus, lying between the superior and inferior mesenteric arteries, is composed of an assemblage of ganglia, giving origin to the gelatinous fibre and branches of spinal nerves derived from the lumbar and lower dorsal nerves, with some communicating branches from the solar plexus. From this association of the two nervous fibres, or nervous systems, branches are sent along the inferior mesenteric and hæmorrhoidal arteries to the descending colon and rectum, and branches are prolonged downwards, to form the inferior aortic plexus, which, at the margin of the pelvis, separates into two divisions, each of which receives the name of the hypogastric nerve or plexus. These plexuses pass round each side of the pelvis to reach the sides of the uterus, and there dividing and subdividing into minute branches, are finally distributed to the lower part of the organ. The middle part of the uterus is further supplied by a distinct branch which passes off from the upper part of the hypogastric plexus, and entering the broad ligament, so reaches the organ; whilst the upper past of the uterus is supplied by branches which come from the renal plexus, and, in company with the spermatic artery, descends to the ovary, furnishes branches to it, and finally is distributed to the upper part of the uterus.

Thus the uterus is supplied, in the inferior and middle part, by branches of gelatinous fibre, the true sympathetic nervous system, derived from the ganglia of the superior aortic plexus and lumbar ganglia, with some communicating branches from the solar plexus; and branches of spinal nerves, cerebro-spinal nervous system, derived from the lumbar and lower dorsal nerves. Nerves derived from the same sources are also distributed to the descending colon and rectum. The upper part of the uterus being supplied by gelatinous nervous fibres, derived from the solar plexus, and by spinal nerves derived from the splanchnic nerves, which are branches of the upper dorsal nerves. From the solar plexus, *i. e.*, from gelatinous fibres from the semilunar ganglia and dorsal ganglia, and branches of the dorsal spinal nerves, branches are also distributed to the stomach, small intestines, liver, kidneys, &c.

As the hypogastric nerve on each side passes to the side of the uterus, it sends down branches from the lower border, which, joining with branches from the sacral nerves, forms a complicated plexus on the side of the vagina—the pelvic plexus. Small ganglia of the sympathetic are found to this plexus, but it is chiefly composed of the cerebro-spinal nerves, and in this it differs essentially from the hypogastric plexus. From the pelvic plexus comparatively large nerves are distributed to the vagina; and considerable sized nerves are sent to the bladder; whilst small branches are furnished to the lower part of the rectum.

The nervous supply of the vagina, bladder, and lower part of the rectum differs in these points from the nerves distributed in the uterus. In the source from which they are derived: those to the uterus, arise from the lumbar nerves and gelatinous or sympathetic nerves coming from the superior aortic plexus and from the solar plexus; those to the vagina, bladder, and lower part of the rectum, take their origin from the sacral nerves, and the sympathetic fibres from the ganglia of the pelvic plexus, with some branches from the hypogastric plexus. In the composition: those to the uterus being composed of a large amount of sympathetic fibres, and a small amount of cerebro-spinal fibres: those to the vagina, bladder, &c., by a large amount of cerebro-spinal fibre, and a smaller amount of sympathetic fibre.

The muscles and skin of the perinœum and lower end of the rectum are supplied entirely by branches from the sacral nerves—the pudic nerve. Thus, as we proceed upwards, from the skin of the muscles of the perinæum to the vagina, and thence to the uterus, we meet with less and less of the cerebro-spinal system or tubular nervous fibre; and more and more of the sympathetic system, or the gelatinous nervous fibre. And, as a consequence, the effects of disease or injury to the tissues are less and less felt, as we pass from the skin to the uterus; and the actions of the organs are more and more, though not entirely, withdrawn from the influence of the will.

Briefly, then, to recapitulate the anatomy of the nerves of the uterus, we find the following arrangement:—

1. The lower and middle portions of the uterus are supplied by the sympathetic from the superior aortic plexus and the lumbar ganglia, and by cerebro-spinal nerves from the branches of the lumber and lower dorsal nerves.

2. The ovaries and upper part of the uterus are supplied by the sympathetic from the solar plexus and thoracic ganglia; and by spinal nerves, from the intercostal nerves, by means of the splanchnic nerves.

1. The vagina and bladder are supplied by branches from the sympathetic from the ganglia in the pelvic plexus, also from the superior aortic and lumbar ganglia, through the medium of the hypogastric plexus; and by spinal nerves from the branches of the sacral plexus.

4. The muscles, skin, &c., of the perinæum, are supplied by branches from the sacralplexus.—*Medical Times, November* 23, 1850.

ON THE MOVEMENTS OF THE UTERUS.

By Dr. Snow Beck.

[In his remarks upon this subject when treating of the physiology of the uterus, Dr. Beck observes:]

On considering the contraction of the uterus, I have always thought that the apt simile of the late Professor Reid, applied to another part of the nervous system, well illustrated the subject: "The movements of a horse are independent of the rider upon his back; in other words, the rider does not furnish the conditions necessary to the movements of the horse; but every one knows how much these movements may be influenced by the hand and heel of the rider." To apply this to the present subject, the contractions of the uterus are independent of the cerebro-spinal system, though every one knows how much these contractions may be influenced by the actions of this system. According to these views we have to consider—

1. The movements of the uterus; peristaltic in form, and effected through the agency of the true sympathetic system.

2. The influences which effect this movement—

a. Through the brain, as in volition and emotion

b. Through the spinal cord, as in reflex action.

1. *The movements of the Uterus; peristaltic in form, and effected through the true sympathetic.*—Although I do not mean to say that direct irritation, acting upon a portion of inorganic muscles, will not cause the point irritated to contract, independent of the influence of the nervous system, yet the vermicular or wave-like contractions of the uterus, which spread from one part irritated over the whole organ, appear to require the influence of the sympathetic system for their proper accomplishment. "In organs partly or wholly supplied by ganglionic nerves, as the heart the bladder, intestines, œsophagus, &c., the motion produced is of a peristaltic kind, spreading generally in a vermicular manner to a distance from the part irritated, and continuing for some time after the irritation has ceased. The uterus is eminently endowed with this form of contraction."

That the contractions of the heart and intestines are independent of the action of the cerebro-spinal system is shown by a variety of evidence, part of which is the continuance of the contractions for some length of time after these organs have been removed from the body; but that the contractions of the uterus at the full period of pregnancy, or when induced by other means, are equally independant of the cerebro-spinal system, and may of themselves effect the expulsion of the child, is more difficult to be shown. Although I cannot go the length of the reviewer I have so often quoted, in saying, "The muscular action of the uterus we regard as essentially peristaltic and independent of nervous stimulation," yet I agree with the opinions in the following quotations from the same:—"That the uterus is susceptible of being excited to contraction by reflected stimulation no more proves that its ordinary actions are dependent on the spinal cord, than the influence of remote excitants upon the action of the heart and alimentary canal proves that these actions are essentially reflex in their nature. In fact, it does not appear to us at all difficult to show, that the influence of the nervous system, on the uterus is precisely of the same character with the influence of any stimulus directly applied to that organ, and is very different from the ordinary forms of reflex action, as seen in the muscular system of animal life. Thus it appears, that when uterine contraction is excited through the nervous system, it is not by a new and powerful agency taking the place of

the feebler powers of the organ itself, but by the application of a new stimulus, which causes remote irritation to have the same effect as one of a direct or immediate kind; so that the peristaltic contractions of the uterus are realy its only mode of expelling its contents."—As, however, much difference of opinion still exists, whether the contractions of the uterus are dependent or not for their commencement and continuance upon the cerebro-spinal system, I will give hereafter the evidence in support of both opinions.

2. *The Influences which affect the Movements of the Uterus,*—(a) *Through the Brain—Volition.*—The influence of the will upon the contractions of the uterus is sufficiently well known. Although the will has no distinct power over the uterus to cause it to contract or to stop these contractions when once established, and although the contractions will continue when the power of the will is temporarily suspended, as during coma or complete anæsthetic agents, yet every practical accoucheur must have seen cases where the influence of the will was obvious in increasing the contractions of the uterus when deficient in force, or in retarding these contractions when violent, or when they are exceedingly painful. Increasing the contractions, as when, from dread of instrumental interference, the patient exerts her will, and, adding to the efficacy of the previous languid contractions, effects the expulsion of the child without the aid of the accoucheur. Retarding the contractions, when, from the dread of the suffering they produce, the patient exerts her volition to keep them away as much as possible—in the language of the lying-in-room, "keeping back her pains," Cases not unsimilar to the following are also not unfrequent. A lady who has had two or three children, is suddenly taken in labour, and the accoucheur being from home, has to be sent for; when she voluntarily restrains the force of the contractions, to retard for a while the birth of the child. As soon as the accoucher arrives the contractions are allowed to take their course, and the labour is quickly over, the patient telling the accoucheur that she "kept back the pains until he came to her assistance." The influence of the will is also said to effect the contractions of the uterus, when the patient voluntarily closes the glottis, and, drawing inwards the walls of the abdomen, is said to "bear down." It is undoubted that the force of the contractions is increased by this means, but it must be remembered that two influences are in action; the influence of the will, which voluntarily closes the glottis with a definite object in view, being transmitted to the uterus; and the influence of the local pressure of the uterus by the surrounding viscera, and which appears to be the more powerful of the two. It is thus the combined actions of the will and local irritation which produce the increased contractions of the uterus in this instance, and not the single influence of volition.

Emotion.—The influence of emotion upon the contractions of the uterus is too well known to require much illustration. The effect of the sudden introduction of a stranger into the lying-in-room, or the sudden communication of bad news, in stopping for a time the contractions of the uterus, has been frequently noticed. Whilst the cheering influence of hope, confidence in attendants, and cheerful conversation, and the depressing influence of despair, want of confidence, sudden dislike to attendants, or melancholy conversation, are all well known to the practical accoucheur. To these may be added the emotion of the mother on hearing the child's cry, or at the sight of her infant, in producing the complete contractions of the uterus. There is, however, another class of immotions which have been generally, though I believe erroneously, attributed to the influence of the reflex action of the spinal cord. I

allude to the influence which is produced on the contraction of the uterus, by suddenly plunging the hand into cold water, dashing cold water upon the face, taking a large draught of cold water, &c. In these cases the increased contractions of the uterus appear to depend on the influence produced on the system by the sudden application of the cold, and not, as is supposed, upon the reflex action of the cord, it being difficult to perceive through what media this latter influence can act.

(b) *Through the Spinal Cord—Reflex Action.*—The influence of reflex action of the spinal cord in increasing, and occasionally exciting the contractions of the uterus, has been abundantly recognized by most writers on the subject, and we shall see, when considering the pathology, that these reflex actions are the chief means by which the diseases of the uterus are made evident to the patient, and recognized by the practitioner. The contractions of the uterus are influenced through this means, by gently rubbing the abdominal surface at the commencement of a pain; by applying the hand previously cooled by immersion in cold water, to the same part; by dashing cold water on the upper part of the thighs, or abdomen; by the application of warm flannels to the abdomen; by the alternate application of cold and warmth to the abdominal surface, when it has become accustomed to the continued action of either, so as no longer to be excited by their single application; by the injection of cold or stimulating fluids into the rectum; or by the introduction of the finger of the accoucheur to the upper part of the vagina. When the finger passes to the uterus, it then becomes a local irritant, and the increased contractions are induced principally by this means, acting through the medium of ganglia of the sympathetic; yet there can be no doubt that some reflex influence is combined with that of the local irritation.

Combined Action.—It not unfrequently happens that two of these modes of influencing the contraction of the uterus are in operation at the same time, and produce more effect than either singly applied. For example *volition and local irritation* are combined in the instance already adduced, when the patient voluntarily closes the glottis, and contracts the abdominal muscles, by which means the uterus is subjected to local pressure from the surrounding viscera, and influenced by the same volition which closes the glottis and contracts the abdominal walls. It is a question, however, which I am not prepared to determine, whether reflex action is or is not added to this combined influence. That reflex action may be excited by the compression of the nerves of the abdominal muscles, in their contraction against the viscera, and so influence the uterus through the lumbar plexus, is possible; but I am not prepared to say how far the power of the will then in action may interfere with this reflex power.

Emotion and Reflex Action are combined when the child is applied to the nipple, and contraction of the uterus follows. Here, I have no doubt, the chief effect is produced by the emotion of seeing and feeling the child, together with the "singular feeling" which the child produces by sucking the nipple. At the same time, it is imposible to withhold the belief that some reflex action may also be induced through the inter-costal nerves acting along the splanchnic, and thence down the spermatic pleuxs to the upper third of the uterus. But as this reflex action can only excite one third of the organ, it is evident that the effect it will produce must be comparatively trifling. In the same manner I would explain the effect of a draught of cold water taken into the stomach, the chief effect being produced by the general emotion of the body, and a partial reflex action through the same media as in the last example.

Emotion and Local Irritation.—As in "taking a pain" during labour, after the patient has once felt the increased pain caused by the presence of the fingers on the upper part of the vagina. Here the emotion, caused by a remembrance of the increased pain induced, is added to the reflex action consequent upon the local irritation of the nerves at the upper part of the vagina.

The media through which the actions of the brain and spinal cord influence the contractions of the uterus are sufficiently evident on a reference to the distribution of the nerves of the uterus. There can be no doubt that it is chiefly through the branches of the lumbar plexus which pass inwards as the white communicating cords become mingled with the fibres of the true sympathetic, and reach the uterus the hypogastric plexus; and partly by the branches of the inter-costal nerves, transmitted to the organ by the splanhnic nerves and spermatic plexus, as already explained. It has been stated, though on insufficient grounds, that the endowment of these nerves from the cerebro-spinal system becomes affected by their association with the fibres of the sympathetic; and consequently that the influence of the brain and spinal cord cannot be so readily transmitted by them. To give the quotation at length: "No anatomist, so far as we are aware, has traced any branches of the sacral nerves into the uterus itself: although it is unquestionable that branches of several nerves of the cerebro-spinal system pass into the plexuses of the sympathetic, and are in this way distributed to the organ. It may be said that the distinction is unimportant; but we cannot regard it as being so, for it is obvious that the endowments of the cerebro-spinal fibres, which enter other parts of the sympathetic system, are greatly affected by their associations with it, neither sensory impressions nor motor impulses being as readily conveyed by these fibres as they are by the ordinary cerebro-spinal nerves."—Brit. and For. Medico Chirurg. Review, p. 6.

An important difference is here drawn in the endowments of the nerves, and consequently in the influences which they transmit, between the branches of spinal nerves which pass directly from the sacral plexus to the uterus itself, and those which reach the organ through the medium of the sympathetic. I am, however, unable to perceive any foundation for this opinion. It is said that motor impulses are not so readily conveyed by the cerebro-spinal fibres which enter into the sympathetic system as they are by the ordinary cerebro-spinal nerves. Certainly we have not the same motor power over the organs supplied by the sympathetic system as we have over the voluntary muscles. But it is nowhere shown, that I am aware of, that this depends on an alteration of the endowments of the cerebro-spinal fibres, and not on the anatomical condition of the organs themselves. We can easily understand that the voluntary striated muscle would be much more obedient to the moter impulses from the brain than the inorganic non-striated muscular fibre, although the media by which this impulse was transmitted remained the same in both cases.

It is also stated, that the transmission of sensory impressions is interfered with by the presence of ganglia upon the nerves which transmit these impressions, assigning to the ganglia the office of "cutting off sensation." As, in the transmission of motor influences, it is nowhere shown, so far as I am aware, that this diminished sensory impression does not depend upon the anatomical condition of the organs which furnish this impression, and not upon the presence of the ganglia in the course of the nerves. We can readily believe it possible that the ultimate distribution of the cerebro-spinal filaments upon an organ, chiefly supplied by the sympathetic nervous system, may be different from the distribution which is followed in an organ immediately under the influence of the brain, and chiefly

supplied by the cerebro-spinal fibres; and that this anatomical condition may influence the transmission of sensory impressions. It is certainly contrary to the view which assigns to the ganglia the office of "cutting off sensation," to find one of these ganglia always seated on the posterior or sensitive roots of the spinal nerves. In this situation, it in no way interferes with "sensory impression" transmitted from all parts of the body. Why, then, should it be considered to do so when seated in the abdomen?

In considering this question, we ought not to overlook, that the amount of cerebro-spinal filaments distributed to an organ supplied by what is ordinarily called the sympathetic system, is considerably smaller than that which is furnished to an organ supplied by the ordinary cerebro-spinal nerves. And as the degree of sensory impression transmitted from an organ, and the amount of motor influence which can be exerted over an organ, bears a close relation to the amount of cerebro-spinal filaments furnished to the part, so we have another reason for the fact under consideration, without attributing to the sympathetic ganglia the office of altering the endowments of the cerebro-spinal filaments with which they are commingled. During some of the diseases of the organs supplied by the sympathetic, or during the healthy actions of the uterus in parturition, the sensory impressions are transmitted with sufficient acuteness in the severe pains which are experienced. It cannot be considered that the ganglia exercise the office of "cutting off sensation" at these times; why, then, should it be considered that their office is altered during health?

But to pursue this subject further would lead to an examination of the comparative anatomy of the nervous system in the lower classes of animals, which forms no part of the present inquiry, but which I hope to take up on another opportunity.

To recapitulate what has been stated. The motor powers of the uterus consist solely of the peristaltic contractions of the organ, which are dependent for their full production upon the nerves and ganglia of the true sympathetic; yet these peristaltic contractions may be influenced by the brain, as in volition or emotion; by the spinal cord, as in reflex action; or by a varying combination of these influences. The media through which those influences act upon the uterus, being the branches of spinal nerves sent inwards to the viscera. This, however, introduces the question which has been deferred, viz. Are the contractions of the uterus dependent or not upon the cerebro-spinal system for their continuance?—*Med. Times January* 25, 1851, *p.* 89.

THE CAUSE OF THE CONTRACTIONS OF THE UTERUS.

By Dr. T. Snow Beck

[There seem to be two distinct opinions held upon this subject. 1st. That the contraction of the uterus depends upon the influence of the cerebro-spinal system. 2nd. That they are independent of the cerebro-spinal system, although they may be influenced by it.

On reviewing the evidences in favour of the first opinion, viz., their dependence upon the cerebro-spinal system, we have—]

(A) The experiments, which show—

(a) That the division of the spinal chord in the dorsal or cervical region will only cut off the influence of the brain, *i. e.*, volition and emotion from the actions of the uterus.

(b) That the lower portions of the spinal cord remaining entire, the reflex actions of the cord will not be withdrawn from the uterus.

(c) That with this condition the uterine contractions were arrested; hence,

(d) That the "shock" of the operation was sufficient to arrest the uterine contractions during the few hours which the animal lived.

(e) That by irritating the uterus and spinal cord by galvanism, or irritating the lumbar portion of the cord by the introduction of a stilette, the uterine contractions were partially recalled.

(f) That in the guinea-pig, integrity of the lumber portion of the spinal cord, and destruction of the sacral portion, did not prevent the contractions of the uterus, nor the process of parturition being accomplished.

(B) The cases of disease, which show—

(a) That disease of the cervical or upper dorsal regions of the spinal cord, sufficient to produce paralysis, prevents the pains of labour being felt.

(b) That, notwithstanding this, the contractions of the uterus proceeded regularly in their course, and apparently unaltered in force.

(c) That in these cases the influence of the brain alone would be cut off; the lower portion of the spinal cord remaining entire, the reflex action will not be destroyed.

(d) That disease, seated probably in the lower part of the cord, or cauda equina, sufficient to produce paralysis of the lower extremities, does not prevent the pains of labour being felt, nor interfere with the uterine contractions.

(C) The experiments and cases of disease together show—

(a) That in every case the reflex action of the spinal cord was present; hence

(b) That they supply not the least evidence "that the contractions of the uterus are dependent on the cerebro-spinal system:" hence

(c) That the solution of this problem must be sought by other means.—*Med. Times, Feb.* 1851, *p.* 178.

CASE OF DIFFICULT LABOUR IN CONSEQUENCE OF TWINS JOINED BY THE BREAST.

By Dr. Charles Stuart, Chirnside Berwickshire.

[This was a case in which Dr. Stuart found the head presenting in the first cranial position and every other feature of the labour apparently very favourable, though he found that the strength of the pains was remarkably decreased on the patients lying down, although the pains on her moving about in the erect position were very violent.]

The cranium advanced with extreme slowness, considering the strong nature of the pains; and it was only after the most severe straining that the head began to press on the perinæum, and after a very tedious passage was born about seven o'clock P.M. The pains previous to this were of the most frightful description, and they were now, if it were possible, increased. Some apparently insurmountable obstacle seeming at this stage to oppose the further exit of the infant, I tried by every means in my power to discover the cause of delay, but from the extreme tightness of the parts it was impossible to ascertain its nature. I dreaded, from the enormous straining, that the uterus would speedily rupture, unless delivery was immediately effected, so accordingly I applied at first gentle traction: but, when I found that unavailing, I was forced to increase it to what

previously I would have considered a most unwarrantable degree, and succeeded in delivering the shoulders, when for the first time I discovered something unusual. I continued, however, my traction as the only hope of getting the woman delivered; and after using the greatest force, I was in no small degree astonished when another head came down with the face considerably flattened. This second head lay twisted round upon the back of the first delivered infant. After further perseverance, I succeeded in extracting two males, still-born, and intimately joined from the sternum to the umbilicus, into which an umbilical cord, common to both, was inserted. The placenta speedily followed, and was not larger than is usually seen in cases of twins. The umbilical cord was rather thicker than usual. About an hour elapsed from the time the head was born till the delivery was completed. The pains during that period were of the most agonizing and alarming character, and made me regret exceedingly having no chloroform. After a careful examination of the external parts, I was very glad to find no perineal laceration, which I feared very much from the passage o such a mass. The twins were at the full time, and fifteen inches long. The band of connection extended from the upper part of the sternum to the umbilicus, and was seven inches broad and three long; and the diameter of the twins, when laid together, was six and a-half inches. They were perfectly and fully formed in other respects, but the head that presented first was the larger of the two. I failed in obtaining permission to make any more particular examination.

When we consider the breadth of the connecting band between the two children in the above case, we see more clearly how the head of the second child could assume the position that it did, and to what an extent the connecting band must have been stretched to have allowed of its being placed at the back of the shoulders of the other child when delivered.

My patient is a woman of slender figure, but well formed, and of good constitution. During her pregnancy she enjoyed excellent health, which in some measure strengthened her for the extreme trial she had to undergo, and which she endured with the greatest fortitude.

She has made a most excellent recovery, and is now quite strong.

No doubt the long delay before the head was born, in a great degree saved her from the danger of perineal laceration, as there was ample time for complete dilatation, which was so essential for the safe passage of such a mass as had to follow.—*Monthly Journal of Med. Science, January*, 1851, *p.* 5.

ON THE STATE OF THE NERVES DURING PREGNANCY.

By Dr. Snow Beck

[Dr. Beck states the following facts in support of the opinion that the nerves are not increased in size during pregnancy. He says:]

These facts are obtained from a series of dissections, which show that no enlargement of the nerves has taken place, and that a special provision exists for their distribution over the gravid uterus without any augmentation of their size.

The dissection consists of—1. The dissection of a gravid uterus in the full period of pregnancy, taken from the body of a well-proportioned healthy woman who had died from hæmorrhage within two hours after the birth of the child. 2. The dissection of the uterus of a woman who had previously borne children; care being taken to select one as near to the stature and as well-proportioned as the former, to avoid, as far as possible, any difference which might depend

on individual peculiarity. 3. The dissection of a virgin uterus, taken from the body of a young lady, who had died from phthisis pulmonalis, aged twenty-five years. 4. The verification upon recent specimens of the results of the previous dissections. 5. The dissections of the virgin uterus by Dr. Robert Lee.

These dissection being all made after the same method, *i. e.*, by the removal of the fibro-cellular neurilemma, when compared together incontestably show that the nerves are of the same size in all; or, in other words, that the nerves have undergone no alterations in size, either from age or pregnancy. Upon this point, I am glad to have the dissections of Dr. Lee to appeal to, as proving that the nerves of the virgin uterus are as large as the nerves of the gravid uterus at the full period of pregnancy, especially when it is remmembered, that these dissections were made to prove that the nerves of the virgin uterus were smaller than those of the gravid uterus. The means by which this demonstration was attempted have been already pointed out.

An objection has been made to this series of dissections, viz., that they are not sufficiently numerous to prove that no alteration takes place in the nerves from pregnancy. To those who are unaccustomed to the labour of minute dissection, this object may appear to have some weight; but when I know that it took me nearly twelve months working some hours every day, to make one dissection with that care requisite to make it any value, I am perfectly willing to leave the making of further dissections, in order to remove unnecessary objections, to those who are not satisfied with the present demonstration, begging them at the same time, to remember, that *one* dissection carefully made, can be depended on for the results of obtained; but *twenty* dissections, hastily made, and without due care, are worse than valueless for the eludication of the point at issue. They are valueless because they do not prove any thing satisfactorily; they are worse than valueless, because they lead to much loss of time, by provoking useless discussions.

An *a priori* objection has been raised to these proofs on the grounds, that the nerves of the virgin uterus, being as large as the nerves of the gravid uterus, shows that some change must have taken place otherwise "they would, if unchanged in structure, appear attenuated to an extreme degree," when stretched over the increased superficies of the gravid organ. Dr. Tyler Smith writes, (On Parturition and Obsieries, p. 71,) "Those who maintain that the nerves do not increase in size during gestation, must show how, not only that there is no such increase in the gravid, as compared with the virgin uterus, but they are bound to show that the nerves relatively diminish in breadth during pregnancy; for, when we consider the extent of superficies of the fully developed gravid organ, it it must be evident to the meanest capacity, that as the nerves of the virgin uterus remaining stationary as regards size, are merely stretched upon, drawn out, or unfolded over and in the enormously increased gravid organ, they ought proportionally to appear as much *diminished* as the growing tissues of the uterus are *increased* in size." Dr. T. Smith, however, appears to forget, that in order to make this a valid arrangement, it is necessary to show that the nerves of the virgin uterus are only of such a length as to allow their distribution to the virgin organ. Had he *really* inspected the preparations of Dr. Lee as much as in his writings he would wish to make it appear, he would have perceived that in the virgin uterus the nerves, when even only partially dissected, on the body of the organ, can be drawn out to a surprising length,—very much longer than is requisite to supply the virgin uterus.

Rightly considered, anatomy affords one of the best *à priori* arguments against the enlargement of the nerves during pregnancy. First it discloses a marked folded arrangement of the nerves, both in immediate neighbourhood of the uterus, and on the body of the organ; and, secondly it shows a peculiar distribution of the nerves of this organ, whereby these nerves can be distributed over the uterus, when considerably enlarged, with greater felicity than at first sight would appear. This peculiar mode of distribution consists of the lower third of the uterus being supplied by branches from the lower part of the hypogastric plexus; the middle third of the organ, by a distinct branch from the upper part of the hypogastric plexus, which entering the broad ligament, reaches the uterus without any communication with the nerves lower down than the pelvis; whilst the upper third derives its branches from the renal plexus, these come down with the spermatic artery, and, after supplying the ovary, are distributed to the upper part of the uterus. In consequence of this mode of distribution the enlarged gravid organ, when it rises in the abdomen, is brought nearer to the organ of the nerves distributed to it, and consequently, they (the nerves) can be spread over the enlarged organ with greater facility than if the nerves had been solely derived from the lower part of the hypogastric plexus.

Now, when the marked folded arrangement, already noticed, is added to this unusual mode of supply, we perceive that nature has furnished ample means, whereby the nerves can be distributed over the enormously increased gravid organ *without their undergoing any augmentation in size*; and has thereby furnished one of the best *a priori* arguments against the enlargement of the nerves during pregnancy. An examination of the preperations themselves, at present in my own possession, will make this much more evident than any mode of expressing it upon paper can possibly do.

The only *à priori* reason which, it appears to me, can be assigned that the nerves should enlarge during pregnancy, arises from the great increase in the whole of the tissues of the organ. But this withdraws the objection to the possibility of the nerves being disturbed to the greatly enlarged uterus, and places it upon a very different question, viz., the necessity for the enlargement of the nerves with the increased growth of the organ; or, in the language of a reviewer, "it certainly does seem to us anything but unreasonable to suppose that, with the enormous developement of the muscular and other structures, the vast increase of the supply of food, and the elevation in the entire functional activity of the wonderful organ, there *should* be a necessity for an augmentation of its nervous supply." Yet to admit this, except upon positive evidence, would be as unphilosophic as to deny it, except upon evidence equally strong. Any arguments drawn from the supposed functions of the sympathetic cannot be considered of much weight seeing, that with all our labours in that direction, we know, as, yet, very little about it. Even should we admit the supposition, that the functions of the sympathetic "would be to regulate, through their accordance with its functional activity," this does not of necessity, require the nerves to be enlarged, in order to exercise this influence on the arterial coats.—*Med. Times, January* 4, 1851, p 9

SURGERY.

NON-UNION AFTER OPERATION FOR FISTULA IN ANO, OCCURRING PRIOR TO ADMISSION INTO GUY'S HOSPITAL.

[In this case the patient had suffered from fistula for six months past; is a stout, flabby, and somewhat unhealthy looking young man, but states having enjoyed good health hitherto. Six months since an abscess appeared in the perineum, which gradually increased to the size of an egg, when it was opened and three ounces of exceedingly offensive pus was evacuated. The fistulous opening never properly healed; neither have the margins of the wound united.]

On examining the part, the anterior portion of the sphincter and rectum had been divided in a direction directly forwards towards the perineum, and still remains so, but little, if any, union having taken place between the divided edges. There are also two fistulous sinuses—one extending on either side of and nearly round, the anus; whilst the other passes for some distance towards the rectum, where the probe can be distinctly felt through the mucous membrane.—He was ordered caster-oil mixture twice a day, which acted freely on the bowels.

Mr. Hilton examined the rectum with his speculum ani, but was unable to discover any evidence of a communication between the external opening and the rectum, either by means of probe, or by injecting water with a syringe through the external aperture—most; if not all, of which returned by the same orifice. On December 10th the operation was performed by introducing a grooved director, and first dividing, by means of the sharp-pointed bistoury, that part of the fistula running up by the side of, though, as far as could be ascertained, not communicating with the rectum; after which, the fistulous canal running around the anus under the integument was treated in a similar manner. Oiled lint was now placed between the edges of the wound, and the patient was ordered to keep perfectly quiet in the recumbent posture.

11th.—He felt very uncomfortable; his bowels had not been moved, and there was no evidence of any constitutional disturbance.

12th.—The bowels have been moved, but without causing him much pain

14th.—The wound discharges but little, is granulating slowly and seems disposed to heal up from the bottom—Ordered potassæ bicarbonatis, ℥ss.; infusi cascarillæ, ℥j. ter die. Nitric acid lotion to be applied to the granulating surface in order to stimulate it slightly.

16th.—There is not so much discharge as previously, and the fistula is filling up as favourably as can be expected.

24th.—The divided parts are decidedly uniting, and he expresses himself as feling much better. From this time he continued daily improving up to Dec. 30th, when his furlough being expired he was compelled to leave the hospital to return to his regiment. The wound at this date had healed considerably, and there seemed every probability of the operation being attended with success.

In this case you will observe that the fistula was situated in front of the anus, and between it and the scrotum. The patient had undergone one operation for laying open the canal prior to his admission, the result of which was unfortunate, no union having followed,—an event happily not of very frequent occurrance. The termination, however, suggests to us one important fact—viz., never to perform the operation without having previously ascertained whether there are any other co-existing disease which may intefere with its success;

and on investigating the different causes of these cases of or non-union, they may be divided into the local and constitutional. On the present occasion, we shall only advert to the local; but do not forget the non-union after the operation for fistula in one is frequently associated with disease of the hepatic and pulmonary organs. Of the local causes, a communication with the urethra may be mentioned amongst some of the first. This is a complication of the greatest importance to investigate and make out prior to performing an operation. Many cases have come under my notice in which the operation for fistula in ano has failed for want of this precaution being duly attended to. One case came under my care in which a communication of this sort existed,—the result of an ulceration in the urethra behind a stricture; the complication had not been suspected and the perinæum had been divided into the rectum, so as to lay open the fistulous track, the consequence of which was that no union followed, and the patient could never retain his fæces after; but by passing the catheter, and keeping one constantly in his bladder, the opening in the urethra was closed, and the patient so far recovered.

Another case of this kind came under my care last May, where the fistulous opening communicated both with the urethra and the rectum: having made this out, I declined performing any operation, but recommended he should keep on his back, and that a catheter should be kept constantly in the bladder, so that urine should never be passed excepting through the catheter. My patient persevered in this plan of treatment, but was unable to keep the catheter in his bladder, in consequence of the irritation caused by it. The frequent introduction of the instrument has been attended with decided benefit; notwithstanding, there is every reason to believe that he is the subject of phthisis,—but on hearing from him a few days since, he says he is much better in every respect, although a few drops of urine occasionally escape through the opening in the perinæum, which is of a pin-hole size, and some urine still passes into the rectum at each period of micturition.

The non-union in this case with the soldier did not depend either on any communication with the urethra or on any pulmonic complication, as indicated by the absence of all symptoms of these organs being affected, when taken in conjunction with the patient's previous history whilst the healthy state of his chest was ascertained by a stethoscopic examination. The non-union, however appears probably to have been the result of a too extensive division of the perinæum into the rectum at the first operation. Such an extensive division of soft parts it is important if possible to avoid, in order to prevent an unpleasant termination, like that in the present case. If the fistula extends high up into the rectum—say for instance, three inches—I would not advise you to divide the entire extent of the canal, but to operate on a smaller scale,—that is to say, after passing a director along the sinus, to divide that portion of the canal lying external to the curve of the sphincter muscle: after which the remainder of the track will probably heal up of its own accord; but if on the other hand, the fistula does not extend more than an inch and a half up the rectum, the whole extent may be divided without any fear of such an untoward result. This extensive division is not generally considered a cause of failure in these operations. I am fully persuaded, however, that it occasionally is so; and it seems to be a very probable explanation of the soldier's case,—an opinion which is confirmed by the present condition of the parts. Another cause of non-union is when several small fistulæ communicate with each other. In the present case there

were two, both of which were laid open during the last operation; although, when these are quite superficial, the injection of a solution of nitrate of silver, black wash, or sulphate of zinc, will often be found sufficient to heal them. I am unable to complete the case as the final result is unknown. It is, however, one of great practical interest, from suggesting some important considerations in reference to the operation, tending to show us one cause of failure, as well as how the treatment must be modified by existing circumstances; especially by the length of the fistula, and its depth in the soft parts about the rectum.

[The second case of fistula in ano occurred in a patient of a robust and plethoric habit. He was a shoemaker of temperate habits, and enjoyed general good health.]

About fifteen years since, he became the subject of fistula in ano, for which he under went an operation; and the fiistula is said to have healed up, but there is no evidence of a cicatrix. From that time he has experienced an occasional aching pain around the anus, especially after going to stool. About three months since he first noticed a swelling in what he considered to be the position of the former fistula. This gradually increased in size and gave rise to some amount of constitutional disturbance until five weeks since, when it opened of its own accord, and discharged an exceedingly offensive purulent matter; since which it has remained open and continued discharging up to the period of admission; but, as far as he can judge neither gas nor feculent matter have ever passed through the fistula.

Ten days ago another swelling formed on the opposite buttock, which burst about four or five days after, discharged very freely at the time, and then gradually it healed up. On examination, there is a fistulous opening on the left side of the anus, distant about one inch from it along which a probe can be passed for some distance, though not into the rectum; and after repeated attempts it was found impossible to ascertain, by means of the probe, whether the fistula did or did not communicate with the rectum; this difficulty as regards diagnosis was overcome by injecting water somewhat forcibly into the external opening, the speculum ani having been previously introduced up the rectum, when by carefully examining the walls of the rectum through the deficient portion of the instrument as it was turned round, water was at length observed to pas into the posterior part of the rectum, through a very small aperture, which by the aid of the speculum, was rendered distinctly visible. The patient is quite free from inconvenience during defæcation, but complains of an aching pain around the anus afterwards, as also of general weakness and pains in his loins. His bowels are habitually constipated. Appetite very good, sleeps well; there is no evidence of phthisis or any other disease.

20th. He was ordered to keep in bed in the recumbent posture, and to take castor oil mixture twice a day.

26th. The fistula still discharges freely. His bowels are kept open every day.—Ordered a full dose of castor oil to-morrow morning, so as to clear out the bowels effectually prior to the operation.

27th. The patient having been drawn to the edge of the bed, where he was placed upon the bed with his pelvis well raised, Mr. Hilton proceeded to lay open the fistula into the rectum, as follows:—The speculum ani was first introduced up the rectum, with the slide opposite to the internal opening; a grooved director was then passed through the external opening as far as it would proceed along the fistula, and a grooved probe, bent at an acute angle for the purpose, was introduced into the internal opening by the aid of the speculum, and directed

downwards and outwards until the two instruments were close to each other. By means of a sharp-pointed bistory, which was passed along the groove in the director, the soft parts, including the sphincter muscle, were divided, and the lower part of the fistula was laid open: the extremities of the two probes were thus exposed, when the upper portion of the fistula was observed to extend off in an angular manner, though perfectly continuous with the lower half of the fistulous canal. The upper portion was then divided with the bistoury from above to below, thus completing the division of the soft parts between the two outlets. The upper part of the fistula formed a very narrow canal, the walls of which were distinctly visible after the operation by the aid of the speculum, and yielded a sensation of thickness and hardness of the touch, as if of a cartilaginous character. A piece of lint was placed between the edges of the wound, and a cold sponge was applied externally: there was no important hemorrhage after the operation. The patient lay down in his bed, and was ordered to keep perfectly quiet.

28th. The patient has slept well during the night, and feels very comfortable.

30th. He was suffering considerable febrile disturbance; tongue furred, hot skin, quick pulse, and felt very thirsty. There was no evidence of shivering, and the wound looked tolerably healthy. On the right side of the anus there is a red spot, as if a small abscess was forming, but at present there is no evidence of suppuration.—He was ordered poultice to the part, to keep in the recumbent position, and take a dose of castor oil. These symptoms, however, quickly passed off, and all trace of the superficial abscess disappeared. The wound healed up by granulations of a healthy character. He continued to do well in every other respect; so that, on January 11th, he was enabled to leave the hospital cured.

The patient was operated on for fistula about fifteen years ago, and states that he has always been subject to occasional pain and uneasiness about the rectum from that time. Judging from this, as well as from the thickened condition of the walls in the upper portion of the fistulous sinus,—evidently showing that that part was not of recent date. I am disposed to think that the fistula was not completely divided at the first operation; thus accounting for his uncomfortable sensations in the neighbourhood of the rectum since that time. When the abscess was opened, as has already been stated that the discharge was of a very offensive character; and I would wish you to bear in mind that this is generally the case when an abscess forms in any situation near the intestinal walls. This highly offensive character of the pus, so commonly observed in these cases, is no proof whatever that the abscess communicates with the rectum, even although it should have a decidedly feculent smell, or if gas should escape when an opening is made into the abscess externally. Pathological anatomy teaches us beyond all doubt that it may acquire this peculiar odour from its situation alone, especially when near the cæcum or colon, or rectum: this fact I have proved by frequent dissections made for the special purpose of ascertaining whether any communication existed between the abscess and the intestinal canal It appears to depend on the imbibition, or rather on the passage of those gases common to the intestines, through the delicate wall of gut, according to the law of endosmose or exosmose: the gas being once in the abcess, acts upon the pus globules, and thus engenders in the abscess, acts upon the pus globules, and thus a continuous process of decomposition, which ultimately leads to a highly fetid condition of the pus, and the formation of more gases. The knowledge that an

abscess does not necessarily communicate with the intestines, even although the fœtor of its contents would seem to indicate it, is a fact which may frequently prove a source of congratulation to yourself, as well as of satisfaction to your patient, especially when the abscess is in the lumbar or iliac regions.

The indications for treatment in these cases are to keep the patient in the recumbent posture for two or three days prior to the operation, the chief object being to get the parts in a more healthy condition, as not unfrequently, either from the irritation caused by standing or walking, the parts become hot, swollen, and somewhat inflammed; and if the operation was performed at once, without a due consideration of these circumstances, the chances of success would be very much diminished.

This I am particularly anxious to impress upon your minds, as you will find it contribute materially to your success in practice. Oleaginous purgatives should be exhibited for two or three days previously, and a full dose of castor oil on the morning of the intended operation, so as thoroughly to evacuate the bowels, and thus to do away with the necessity of an evacution for several days afterwards. The next thing to be done is to divide the sphincter and soft parts into the rectum, and so to lay open the fistulous track: whether this should be done through the extent would, as has been previously stated, depend on the length of the canal.

On applying our remarks to the case now under consideration, we observe that the external opening was distant about one inch from the anal aperture; and the first thing to be done was to endeavour to pass a probe from one aperture to the other,—that is, from the external opening through the fistulous canal into the rectum, so as to make certain whether the fistula did or did not communicate with the rectum: this could not be accomplished; and it did not at first seem improbable that the case was merely one of blind external fistula, having no communication with the rectum; whilst several points in his history—especially in his never having passed any gas or fæces through the fistula—seemed to favour such an opinion: but did this justify us in arriving at a conclusion that no communication existed between the rectum?—Certainly not: the passage of fæces might easily have been prevented by the character and direction of the internal opening. All doubt, however, on this point was speedily cleared up by injecting water with a syringe into the external opening, care being taken at the time to prevent it from returning immediately by the same orifice: by these means the existence of the opening into the rectum was easily detected, and, by the aid of the speculum, its exact position, small size, and peculiar character, were rendered distinctly visible. The injection of water is a test by which you can at the same time determine whether more than one opening exists in the gut. In the present case there was only one opening, through which the water quickly passed,—demonstrating all that was required, without causing any pain to the patient; clearly showing its advantages over the probe, which will not unfrequently be found to fail. The cause of failure with the probe in the present instance was quite apparant after the operation, and depended on the canal taking an irregular course; the upper portion appearing, as it were, to branch off at a right angle from the lower.

The internal opening of the fistula in this case was in that position where it is most frequently met with,—viz. about one inch above the internal sphincter; this was pointed out by Ribes and others, and has been more recently confirmed by surgeons of this country. Its exact position can now be more easily made out by means of the speculum, and injecting water into the external opening. If

you cannot see in what part of the rectum the water first makes its appearance, which sometimes occurs in consequence of the translucency of the water, some colored liquid should be substituted for it, when the escape through the internal opening will be immediately observed. But even after we had discovered the exact position of the two openings in this case, and proved that they communicated, as shewn by the passage of the water, still it was impossible to pass a probe from the one to the other; and the question then arose—What shall be done? as it was important for the success of the operation that the entire length of the fistula should be divided. This difficulty was overcome by passing a director through the external opening, and up the canal as high as it would go; when a probe previously curved on purpose nearly to an acute angle, was introduced through the internal opening, which was rendered visible by the speculum, and passed down the fistula until it came nearly in contact with the director; the probe was then steadied by the hand of an assistant, and the sphincter and soft parts between the lower part of the canal and the rectum were divided, immediately after which the upper or intestinal portion, which was quite superficial, and occupied by the grooved probe, was served in a similar manner. The walls of the fistula near its upper and lower openings were quite of a different character, those on the upper half being thick, hard, and almost cartilaginous, and lined by a thin, delicate, smooth, pesudo-mucous membrane; whilst those of the lower half were thinner, evidently of more recent date, and the lining membrane presented a minutely granular appearance.

If the fistula does not happen to extend deeper than one inch and a half up the rectum, you should always endeavour to divide its entire length, especially the intestinal end of it,—otherwise, in many cases, it remains open; and, when the rest of the sinus has healed, it allows a small quantity of feculent matter to enter, which becomes a source of irritation, leads to the formation of another abscess, and subsequently to another fistula; so that, unless this is discovered and laid open, you will not place the patient in a condition to expect a favourable termination, and at the same time deprive yourself of the conscientious satisfaction of knowing that you have done your duty towards him.

After the operation, provided there be no hæmorrhage, you may withdraw the speculum: a small piece of oiled lint should then be inserted between the edges of the wound, so as to cause the parts to heal up from the bottom by granulation. This was done in the case related. The patient is now nearly recovered, and there is every reason to believe that he will continue well, without any return of the fistula.

The intention of the operation for fistula in ano in dividing the sphincter is to allow the inner wall of the fistula to come in contact with the outer,—a circumstance which the sphincter, when entire, always tends to prevent. Within a short time after their approximation, the two walls granulate and become adherent, whilst the sphincter is subsequently repaired by the white fibrous tissue. If the precaution is not taken to introduce a piece of lint, the divided edges of the wound will be likely to unite by adhesion, without the fistula having become obliterated. Prior to performing this operation, I would always advise you to examine the rectum carefully, to ascertain if any large artery lies in your intended line of incision. Such a discovery as this would alter your method of operation, so as to avoid wounding it, if possible; and, in concluding my remarks on this operation, I am anxious to impress upon your memory that, although, in most cases, you should endeavour to lay open the entire extent of the fistula, by cutting through all the soft parts between the two openings, still that, under

occasional circumstances, certain modifications in the operation must be adopted: and the two conditions in which these modifications are especially called for are—

1st. When the fistula extends high up into the rectum: by dividing its entire length you will run the risk of dangerous hæmorrhage at the time, and of subsequent non-union. Under these circumstances you must be satisfied with dividing the sphincter and lower part of the fistula, after which the upper portion will most probably heal up of itself. The second condition under which the operation must be modified is when the fistula has a very extensive track in the soft parts external to the anus: thus, for instance, extending forwards towards the perinæum, or latterlly towards the buttocks; whilst its intestinal opening will, in all probability, be only just above the sphincter ani. Under these circumstances, do not divide the whole length of the sinus; but, having passed your director through the canal, as in the ordinary operation, cut into the director external to the sphincter, divide it and the intestinal end of the fistula, after which the remainder of the fistulous track—that is to say, the external portion which has not been laid open—will most probably heal up. In case, however, it should not do so, some stimulating injection—as black wash, nitrate of silver, or sulphate of zinc, in solution—may be thrown into it, for the purpose of favouring the process of granulation or adhesion. I have performed this modification of the usual operation in two cases: in one it was attended with speedy and complete success; the second case I lost sight of too soon to enable me to express a decided opinion; but so long, as I had an opportunity of observing it, it promised to do well. Some difference of opinion prevails respecting the primary cause of fistula in ano; it is the opinion with some surgeons that it originates primarily in an ulcer in the mucous membrane of the rectum, leading to inflammation and abscess external to the mucous membrane, which soon after opens into the intestine; and this permits the entrance of fæcal matter into the abscess; this again gives rise to the formation of another abscess, which burrows down in the cellular tissue external to the rectum, and at last leads to the completion of fistula. But it does not appear to me that this is the correct explanation of the primary case, inasmuch as facts, and analogy between this disease and diseases occurring in other parts similarly situated, do not tend to this conclusion. In the first place, we know that persons are very frequently the subjects of ulcer in the rectum without its ever being followed by absess,and that the ulcer may exist for months without giving rise to it; whilst the co-existence of abscess with ulcer of the rectum is a rare occurrence. These facts are important as a negative evidence, tending to show that this is not the primary cause. But there is another light in which it must be regarded: the internal aperture of the fistula is in general, if not always, exceedingly small. Now if the original cause was an ulcer in the mucous membrane of the rectum, it is not at all likely that the internal opening would be about the size of a pin's head or perhaps double that size. Another argument is, that we cannot discover any analogy between this supposed cause of fistula and ulcers occurring in other parts of the body similarly situated. Thus, for instance, an ulcer in the nose is not followed by abscess in the walls of the nose; nither is an ulcer in the pharynx followed by abscess in that part. The same remark holds good when applied to the urethra; an ulcer in the urethra is not followed by abscess near its walls, unless extravasation of urine takes place previously. Indeed there is no canal in the body in which a precisely analagous and supposed condition is observed. Again the kind of fistula generally called the blind external fistula—viz., when no com-

munication exists between it and the rectum—is a variety of the disease itself tending strongly to disprove this idea: so that judging from these facts, I think we may safely infer that an ulcer is not necessarily the primary cause of the abscess near the rectum which gives rise to fistula; and that, when the two exist at the same time, it must be regarded rather as a coincidence, than that they bare a direct relation to each other. The more probable cause of fistula, perhaps, stands in some relation to any inflammatory condition of the walls of hæmorrhoidal veins, owing to the circulation through them having been retarded for many hours, produced by standing or sitting for a long time on a hard seat, or by riding on horseback, or as I have known it to occur, from sitting on the damp ground —*Med. Gazette, February* 7, 1851, *p.* 222.

PATHOLOGY.

STATE OF THE BLOOD AND BLOOD-VESSELS IN INFLAMMATION.

(From a Review of the works of Messrs. Paget, Jones, and Simon, in the British and Foreign Medico-Chirurgical Review.

"The very difficulty of exactly defining the process of inflamation may be our guide to the most hopeful method of investigating it. When we see such gradual transitions, from the normal process of nutrition to the disease of inflamation, that we cannot draw a definition line between them, we may be sure that the main laws of physiology are the laws alike of the disease and of the healthy process; that the same forces are engaged in both; and that, though interfered with by the conditions of the disease, they are not supplanted or annulled,

"Now, such transitions from the normal processes to that of inflammation are not rare. We may trace them, for example, in the gradual passages from the active exercise of the brain, or of retina, to its 'irritation' when overworked and thence to its complete inflammation and impairment of structure, after long exposure to what had been a natural stimulus, or to what, in a less degree, might be so. Or on the introduction of medecines, such as certain diuretics, into the blood, we may trace gradations from the normal increase of the functions of the kidneys, under what is regarded as no morbid stimulus, to their intensest inflammations. Or, again, in the application of abnormal stimulus, such as that of a heat greater than the natural temperature of the body, where shall we mark the line at which inflammation begins to supervene on health? We may, indeed say that stagnofion of blood, or effusion of liquor sanguinis, shall be the condition *sine qua non*, inflammation; we may call whatever falls short of these, 'active congestion, 'irritation,' or by any other name; but in practice, such distinctions are often impossible, and sometimes untrue; and in study, the terms are convenient or the sake of brevity rather than of clearness."

[The conditions necessary to healthy nutrition are as follow:—1st. A regular and not far distant supply of blood. 2nd. A right state and composition of that blood. 3rd. A certain influence of the nervous force. 4th. A normal state of the parts in which nutricion is to be effected. All these are usually altered in inflammation. 1. Of the supply of blood. Now what changes take place in the condition of the walls of the blood-vessels in a part where the inflammatory state has developed itself?]

The effects of various kinds of artificial stimuli upon the calibre of the blood vessels, has been the subject of examination by a great number of experimen-

talists; and there is a very general accordance in the results. Mr. Paget has made much use of slight mechanical stimuli.

"If one is watching the movement of blood in a companion artery and vein the point of a fine needle be drawn across them three or four times, without apparently injuring them or the membane over them, they will both presently gradually contract and close. Then, after holding themselves in the contracted state for a few minutes, they will begin again to open and gradually dilating, will acquire a larger size than they had before the stimulus was applied.

"Simple as this observation is, it involves some cardinal facts in our pathology. It illustrates, first, the contractile power of both arteries and veins; and, by the manner of their contraction is like that of parts with simple or organic muscular fibres. And one may notice here the illustration of the stoppage of hœmorrhage from small vessels. In one cut we may divide a hundred such vessels as those on the surface of a stump, and they may cease to bleed in a few seconds: doubtless, the very stimulus of the knife, while dividing them, has made their walls contract and close.

"But, again, the experiment shows the vessels re-opening and becoming wider than they were before, either yielding more to the pressure of the blood which previously they resisted with more strength, or else dilating, as of their own force, with that which Mr. Hunter called active dilatation, and compared with the act of dilatation of the os uteri. In whichever way the dilatation is effected, whether it be active or passive, the vessels will not at once contract again under the same stimulus as before affected them. The needle may be now drawn across them much oftener and more forcibly, but no contraction ensues, or only a trivial one, which is quickly succeeded by dilatation. Yet, with a stronger stimulus, such as that of great heat, they will again contract and close. And such a contraction excited by a cautery may last more than a day, before the vessels again open and permit the flow of blood through them. So that in this we have an illustration of the secondary hemorrhages from vessels, which, after their first closure, have not been sealed by the coagulating blood, or the exudation of lymph,—as well as an illustration of the effect of the cautery or of hot water in again checking such hemorrhages, and more permanently closing up the vessels."

The influence of various agents upon the arteries in the web of the frog's foot has been carefully studied by Mr. Warton Jones, and the following are his general results:

"1. Constriction may slowly take place, and be slowly succeeded by the normal width. This I have found to be the effect of the action of the sulphate of atropa. A solution of this substance, in the proportion of three or four grains to the ounce of water, applied to the web, causes constriction of the arteries, in about the same time that it causes dilatation of the pupil when dropped into the eye. In other words, it causes contraction of the circular fibres of the muscular coat of the arteries in about the some time that it causes contraction of the radiating fibres of the iris. As in the case of the iris, the arteries slowly recover from the action of the atropa.

"2 Constriction may quickly take place, and be soon succeeded by the normal width, or a width not much exceeding the normal. This has been found to be the effect of the moderate application of cold, mechanical irritation, and galvanic irritation. When a drop of cold water is applied to the web, the arteries are seen to become constricted, but they soon resume their previous state. Slight pressure on the web, with a blunt point, for example, in the situation of an artery, excites constriction of it, but by and by the vessel becomes

dilated, as before. When the web is subjected to the influence of a single pair of plates, constriction of the arteries is induced, succeeded by dilatation, as in the preceeding cases.

"3. Constriction either does not take place at all, or, when it does, it very rapidly gives place to great dilatation. This is the effect of such agents as the following:—a solution of sulphate of copper (gr. xvj—℥j) with wine of opium (℥j) dropped on the web, I have generally found to cause primary dilatation of the arteries. A strong solution of common salt I have found to have a similar effect, only, perhaps momentary constriction has more frequently preceeded the dilitation. Battley's *liquor opii sedatives* usually first excites constriction of the arteries, but dilatation quickly supervenes. A drop of spirit of wine immediately causes constriction, but this is soon followed by dilatation of the arteries.

"4. A dilatation, preceeded or not by momentary constriction, may slowly yield to constriction, which remains permanent. This is the effect of sulphate of copper in a concentrated solution. When a point of bluestone is rubbed on the web, in the neighbourhood of, or over an artery, the vessel slowly but steadily becomes completely constricted, and remains so."

[The dilation is, however, by no means uniform, but presents all the varieties of form which we are accustomed to recognize in aneurisms or aneurismal dilatations of great arteries, and]

Besides their increase in calibre, we find that the vessels of inflamed parts are dilated in length; they consequently become more tortuous, and their pulsations are stronger than natural, each impulse increasing their tortuosity. This change may be well seen in the vessels of the conjunctiva; in which part, according to Mr. Warton Jones, the small arteries may be recognized by their pulsations, these being manifested by increased bendings as the pulse waves pass through them. It is felt in the violent throbbing, which is frequently one of the most painful sensations of an inflamed part.

On the whole, then, we may conclude, that the *dilatation of the vessels*, especially of the arteries, with a weakening of the resisting power of their walls, is one of the most constant phenomena of the inflammatory state.

[A change in the rate of movement of the blood affects the supply of blood to the tissues of an inflamed part. There is here also a want of agreement as to the cause and consequences of the retardation, which all believe to be present. There is first undoubtedly, a state of active congestion induced by the application of an irritant, and indeed we may easily imagine this to be the state of many internal organs, when actively discharging their functions. The passage of this state of active congestion into the normal condition, is indicated by the relaxation of the current of blood with a gradual contraction of the vessels, and]

The transition to the inflammatory state is marked by a further retardation of the current, without any diminution, but (it may be) with even an increase, in the diameter of the vessels. This may be observed as the consecutive result of the application of stimuli to the bat's wing; but for the sake of bringing all the phenomena into view at once, Mr. Paget recommends the infliction of such an injury as passing a fine red-hot needle through the membrane.

"The first effect of such an injury (in addition to the charring and searing of the membrane, the obliteration of its blood-vessels, and the puckering of the portion of it adjacent to the burn) is to produce contraction of the immediately adjacent arteries and veins. They may remain closed, or, as I have already described, after being long closed, may again open, and become wider than they were before. This dilatation follows more certainly, and perhaps without any

previous contraction in the arteries and veins at a little distance from the burn. In these there speedily ensues such a state of 'determination of blood, as I have already described: in arteries and veins alike the stream is full and rapid; and the greater accumulation, as well as the closer crowding of the red corpuscles, makes the vessels appear very deep coloured. The contrsst of two diagrams, showing the natural and stimulated conditions, illustrates this difference sufficiently well. The vessels of the one, nearly twice as large as those of the other, darker, and more turgid with blood; and, in the one, numerous capillaries which are not visible in the other. But diagrams cannot show the changes in the mode of movement. Close by the burn, the blood which has been flowing rapidly begins to move more slowly, or with an uncertain stream,—stopping, or sometimes ebbing, and then again flowing on, but, on the whole, becoming gradually slower. Thus it may, at length, become completely stagnant; and then, in the vessels in which it is at rest, it seems to diffuse and change its colour, so that its crowded corpuscles give the vessels a brilliant carmine appearance, by which, just as well as by the stillness of the blood, they may always be distinguished.

SURGERY.

CASES OF IMPERFECT DESCENT OF THE TESTICLE, CONNECTED WITH STRANGULATED HERNIA.

ST. BARTHOLOMEW'S HOSPITAL.

Encysted Hernia in the right Inguinal Canal; Abnormal Situation of the right Testicle; Strangulation; Operation; Recovery.

(*Under the care of Mr. Skey.*)

The groin is, in both sexes, a region in which operations should always be conducted with great caution, for the various tumours which make their appearance in the inguinal portion of the frame very often present great outward resemblance, with the most important differences as to their actual nature. We are acquainted with hospital surgeons who never open a bubo without making the patient cough, and applying the hand or stethoscope to the swelling. This may perhaps be looked upon as exaggerated prudence, but it is better to err on the safe side.

We need not remind our readers that some of the best operators have opened aneurisms in the groin which were thought to be abscesses. These unfortunate accidents have been sufficiently recorded; but we may cursorily mention, that we sometimes see hospital patients affected with swelling in the groin, or in the scrotum, with whom it is extremely difficult to determine of what structures the tumour is really composed. We have always found . at the most prudent course was to explore, when the symptoms were very severe, and to wait when no urgent signs are being manifested.

Among the anomalies which may be noticed in the groin, there is one which should always be borne in mind—viz. the incomplete descent of the testis into the scrotum. It would at first sight appear as if this abnormal state could hardly escape attention, as it is easy enough to ascertain whether the scrotum contain two testes, or only one; and yet such oversights *do* occur. It is especially when the abnormal situation of the testicle is connected with strangulated hernia, that the mistake is likely to be made. The attention is, in such

cases, exclusively and anxiously fixed on the reduction of the hernia, and the scrotum remains unnoticed. It would be well to recollect that the testis, when it is not found in the scrotum, may be looked for in any portion of the inguinal canal, and that it has also happened that the organ does not leave the abdominal cavity at all. The testicle may likewise take a very unusual course, as happened in the case which we are going to relate, for here it had lodged under the integuments of the thigh.

Strange to say, a case of a somewhat analagous kind was, a few days after the reception of the first, admitted into the same hospital, under the care of Mr. Lloyd; there was here, also, retarded descent of the testicle, hernia, and strangulation. We are happy to say that both cases did well; and we proceed to describe the first, from the notes of Mr. Smith, house-surgeon to the hospital.

John R——, a tall, overgrown, strumous-looking lad, aged fifteen, applied for admission on the 1st of March, 1832, at half-past 11 p. m., complaining of great pain in the right inguinal region, constipation of the bowels of four days and a half duration, and frequent vomiting of a green fluid. His countenance was pale and anxious; and on examination of the abdomen, the attention was at once arrested by the peculiar appearance of the organs of generation. The left testicle occupied its usual situation in the scrotum; but on the right side of the raphé the scrotum was undeveloped. On this latter side, quite distinct from the scrotum, and descending from the external abdominal ring, was an elongated oval swelling, the surface of which was formed by the ordinary integument of the thigh, presenting none of the rugose appearance natural to the scrotum.—Within this swelling, which the patient said had existed about five years, the right testicle was distinctly felt, seemingly somewhat smaller than the left.—The patient had always considered this tumour to be a rupture, and seemed quite unconscious of the malformation. On squeezing the swelling, however, he experienced a sensation exactly similar to the one produced by pressure on the left testicle. There was also a tumour in the course of the inguinal canal on the right side, excessively tender, but firm and unyielding. The external abdominal ring could be felt large and free, the tumour in the inguinal canal not having descended through it. The whole of the abdomen was intolerant of pressure.

The patient being questioned as to the history of the case, said that he had always been healthy, and was occupied in carrying out milk. His bowels had frequently been confined, and he once suffered from constipation for four days, but without any sickness. He continued in good health up to four days and a half before admission, when, having been engaged in his usual occupation, he suddenly experienced severe pain in the groin. The bowels had acted during the morning. From that time up to the evening before he came to the hospital, the patient kept his bed, suffering from pain, constipation, and, during the last twenty-four hours, from vomiting. He had taken some antibilious pills, which did not operate; and a doctor was then called in to see him. The latter said it was a rupture. He attempted to return the oval swelling described above (the testicle): and as it seemed to come down again, told the lad to put his hand on it, and hold it up; and gave him, in the meantime, some castor-oil. In the evening, after another attempt to reduce the swelling, which was of course equally unsuccessful, the boy was advised to come to the hospital.

On examining all the features of the case, on the patient's admission, it was supposed, from the absence of the right testicle in its normal position, coupled with the fact of the tumour in the thigh having existed five years, and present-

ing to the touch the ordinary feel of the testicle, that this was one of those cases in which this organ, failing to descend at the proper period, had deviated from its ordinary course, and formed for itself a pouch under the integument of the thigh. It was further conjectured that the tumour in the inguinal canal consisted of a knuckle of intestine, strangulated at the internal ring.

Taking into consideration the date of the strangulation, the frequent vomiting, the anxiety of countenance, depression of the system, and the great local tenderness, probably caused by the efforts to reduce the supposed hernia, it was thought advisable, after finding no good result from the warm bath, at once to have recourse to an operation.

The patient was put under the influence of chloroform on his bed, and the operation commenced by an excision over the external abdominal ring, about two inches in length. The several layers were cautiously divided down to the ring through which the hernial sac protruded. On pulling the sac upwards, it was found to be continued down to the testicle, forming the swelling in the thigh. The finger could be passed readily into the canal, in which could be felt a portion of very firm intestine: and the stricture was found to exist, as supposed, at the internal ring. Considering the length of time the intestine had been strangulated, it was deemed advisable to open the sac, whence a considerable quantity of sanious fluid escaped. The portion of intestine now exposed was of a dark claret colour; and behind the sac the spermatic cord could be distinctly seen and felt. There was no communication downwards through the sac to the testicle. After some difficulty, a director was slipped between the intestine and the stricture, which latter was divided with a probe-pointed bistoury. The gut was then readily returned. One vessel required a ligature. The wound was closed, and the patient made comfortable in bed. Before fully recovering from the effects of the chloroform, he fell into a slumber, which lasted nearly an hour, when he was found relieved of much of his pain. The pulse was 76, of moderate volume, and the anxiety of countenance had quite subsided.

Two hours and half after the operation the bowels acted freely, (a dark brown extremely liquid motion) He complained of some griping pains when the bowels acted, possibly from the purgative medicines administered previous to his admission. One grain of opium was now given.

Second day.—The patient went to sleep again directly after the last visit, and only woke three times, when the bowels acted. There has been no sickness. He still complains of griping pains; the abdomen is tender and tympanitic, tongue red and rather dry in the centre; pulse 88, not at all sharp. Ordered ten minims of tincture of opium at once, and to be repeated every third hour. He continued to sleep a good deal during the day, and in the evening took some little nourishment. He complains of no pain, but the abdomen is still intolerant of pressure; tongue rather dry; bowels open two or three times during the day: no sickness: pulse 96, with rather more power.

Third day.—The boy passed a comfortable night: the abdomen is less tender, and pain is felt chiefly in the situation of the right testicle, which latter is inflamed, this state of things having probably been produced by the efforts that had been made to return it into the abdomen; tongue clean and moist; the bowels have not acted during the night; pulse 84, soft and compressible. He was ordered to discontinue the opium, and to apply two leeches to the right, abnormally placed testicle. When seen the last thing at night, he was quite easy; the bowels had acted, and the pain in the testicle had been relieved by the leeches.

Fourth day—The patient has slept well: he looks brighter about the eye and expresses himself as feeling better; his pulse is quiet, and he has less tenderness of the abdomen, The bandage and pad were removed from the groin, and the sutures taken out; the wound looked exceedingly well, and had partly closed. From this time the progress was favourable, excepting a small collection of matter which formed over the testicle lying on the thigh. This abscess was opened, and twenty-five days after the operation the patient was discharged, with complete cicatrization of the wound, and protected by a truss.

One is naturally led to ask, after reviewing the facts of this case, by what agency the testicle found its way under the integuments of the thigh. Probably from some fault about the gubernaculum, which failed in this instance to lead the organ as usually into the scrotum. The deviation was probably then regulated by the more or less laxity in the parts about the external ring. We need not say one word touching the efforts made previous to the patient's entrance into the hospital to return the mal-placed testicle into the abdomen; it was an oversight, the recording of which will act as a warning to others.

We would in the last place refer to the necessity, when patients apply for a truss, of examining the parts very carefully, and not depending on their simple statements. Suppose this boy had purchased a truss at a chemist's, as is often done; the mentioning of a swelling in the groin would have been sufficient, the truss applied on the undescended testis, and the patient consigned to a series of evils, which might have led to a very unpleasant termination. It is probable that the actual hernia with which he became subsequently affected never produced enough protrusion to attract his attention. In Mr. Lloyd's patient, with whom the testicle was likewise abnormally placed, the disposition was different, for the organ had not descended lower than the inguinal canal. Here follows the case, from the notes of Mr. Stretton, Mr. Lloyd's house-surgeon.

Inguinal Hernia on the right side; Imperfect Descent of the Testicle on the Testicle on the same side; Strangulation; Formation of a Watery Cyst; Operatoin; Recovery.

(*Under the care of Mr. Lloyd.*)

William D——, aged therty-seven, and a baker by trade was admitted into Bently ward, March 11th, 1852, under the care of Mr. Lloyd. The patient's countenance was pale and anxious, and he complained of much pain in the abdomen: the skin was cool; pulse 72, of moderate volume; tongue clean, and bowels said to have been freely open on the evening before admission. While walking in the street on the morning of his reception into the hospital, he had been suddenly seized with copious vomiting.

On examination, a large swelling was found in the right groin, measuring about seven inches in length and four in width; its surface was uneven and tense except at one point, near the anterior superior spinous process of the ilium. Here was situated a smaller tumour, about the size of a walnut, and giving the hand the sensation yielded by a bag of fluid. This swelling, according to the patient's statements, used to remain unchanged in size when the hernia, before the present attack, was returned into the abdomen. The external ring is free, but the right testis is not in the scrotum, and is supposed to lie in the inguinal canal with the herniated intestine. This latter circumstance was not thoroughly ascertained, as the slightest manipulation gave the patient exquisite pain, radiating all over the abdomen.

The man said on being questioned, that he had been subject to hernia for the last fifteen years, that he had always worn a truss, and had never had any difficulty in returning the intestine until the morning of his admission. On that day, while in the street, he had suddenly been seized with a pain in the right groin, and violent sickness and upon examining that region, he found that the tumour had escaped from under the pad of the truss, and was much larger than usual. A surgeon was immediately consulted, who employed the taxis for *two hours* without success, and then advised the patient to seek admission into this hospital.

Immediately after the pstient's reception, chloroform was administered, and gentle attempts were made, when he was fully narcotized, to reduce the hernia; but these proving unsuccessful, the operation was at once performed.

After the usual incisions, the testicle was found in front of, and close to the external ring; the sac was much congested, and the stricture situated at its neck, just at the internal ring. It was now thought necessary to open the sac, which brought into view a large coil of intestine of a dark-red colour. This was easily returned as soon as the stricture was divided, and the wound was carefully closed up over the undescended testicle. Mr. Lloyd now punctured the small swelling situated over the crista ilii; it at once collapsed, discharging a transparent citron-coloured fluid. The patient was conveyed to bed, and took five grains of calomel and one of opium.

The progress was pretty favourable until next day in the afternoon, when the man complained of considerable pain in the chest; his respiration was hurried, the skin hot and dry; tongue moist; pulse 120, small and jerking, and abdominal tenderness considerably increased. There had been copious pultaceous evacuations. Mr. Lloyd ordered twenty-four leeches to be applied to the abdomen, and two grains of calomel with half a grain of opium to be given every sixth hour.

It was found on the next day that the leeches had considerably relieved the pain; the patient had slept well and was altogether in a more favourable condition. The sutures were removed in the evening of this, the third day, and the wound looked very healthy. The further features of the case do not call for any special description, for matters proceeded in the usual way, and the patient was discharged in good condition about five weeks after admission.

As, very probably, the entrance into the scrotum is obliterated in both the foregoing cases on the side where the testis has failed to descend, it would be useless, during operations of the above-described kind, to attempt giving the arrested testicle its normal situation. The patients must thus continue to remain in the state in which they were found, at least as far as the position of the testicle is concerned; but they now have the immense advantage of knowing the real state of things, and will be able to take the proper precautions, in order to shield the delicate and useful organ from injury. Indeed, a special protective apparatus should, we venture to suggest, be contrived for patients of this kind.

When it is considered how obnoxious to injury is the testis in both the situations which it holds in the two preceeding cases, one is inclined to pay new tribute of admiration to the arrangement of parts in the human organism. The scrotum, with its power of rugose contraction, the tunica vaginalis, ever lubricated, and allowing the organ to accommodate itself to the most varied positions &c., appear in great contrast with the situation of the testis in the instances just related—one at the upper part of the thigh, the other in the groin. It

might justly be said that the testis would be, perhaps, more protected by remaining in the abdomen, in which cavity they are lodged before birth; but it will be seen at a glance, that all the derangements occurring in the abdominal viscera would then have a direct influence on these organs, and that by being shut out from the peritonæal cavity, they have an independent existence, favourable to the performance of their functions.

The patient of Mr. Lloyd is said to have worn a truss; we suspect it was one producing very gentle pressure, as it is not easy to understand how the apparatus might, without inflicting pain or injury, have pressed both on the hernial sac and the testicle. It is probable that the latter organ, by the facility with which it glides, even in an abnormal situation, eluded the pad of the truss. We are inclined to think that the cyst by the crista ilii was caused by pressure from the steel of the truss, and cannot conclude without adverting to the long efforts at the taxis which are sometimes made in private practice. In the above case, for instance, if the patient's statements are correct, these efforts were continued for *two hours;* this is decidedly too much, and the evil was so much, the greater in the present case, that the testicle was included in the mass which was for such a long time subjected to the manipulations of the taxis.

Cases of non-descent of the testicle are by no means so very rare as is sometimes thought; we saw, short time ago, a man, among Mr. Curling's patients, at the London Hospital, with whom the same imperfection existed. Here also there were inguinal hernia and strangulation, but the herniated mass was returned without operation.

KING'S COLLEGE HOSPITAL.

Trephining of the Tibia in Two Cases; in one for Abscess in the interior of the Bone, and in the other for Necrosis unconnected with any tegumentary solution of continuity

(*Under the care of Mr. Henry Lee.*)

Abscess of bone is a pathological event with which Mr. Lee's name has of late been connected. The fact of these abscesses existing for a long time without involving the dense portion of the bone, and without therefore any tegumentary manifestations, is not a little strange, because it presents us with an exception to a well known law; we mean, of course, the tendency of any morbid process which takes its origin in some internal locality to make its way to the surface.

Mr. Lee has lately had under his care one of these cases of abscess of bone. The existance of this lesion was diagnosed from the peculiar symptoms which shall be detailed below; and the use of the trephine gave considerable relief. But besides this instance of pyogenic action within bone, Mr. Lee has had under his care another case where the trephine proved very useful, and in which the entire thickness of the tibia towards the middle of its shaft became necrosed, without any abscess being formed, or any solution of continuity of the skin, sinuses, or fistulous tracts. This second case offers a good instance of the services which may be rendered by a good and sound diagnosis; for the detection or suspicion of the kind of lesion which had taken place naturally led to the application of the means best calculated to benefit the patient. We subjoin, in a few words, the description of both these cases.

A married woman, about twenty-five years of age, applied to Mr. Lee, at

this hospital, as far back as 1849, for pain and uneasiness in the left knee-joint. The latter was then tumefied, and externally to the ligament of the patella a swelling of a yeilding and soft consistence was noticed. There was great pain in the articulation, especially when the patient attempted to walk, or when the joint was being flexed; but she suffered most when warm in bed, and had slept badly for some time past. The woman stated that she had been treated for syphilis in this institution three years previous to her present application, and had taken mercury several times.

Mr. Lee, hearing from this patient that she was unwilling to seek for admission into the house, ordered a splint to be applied, so as to keep the joint at rest, and prescribed iodide of potassium.

Rapid improvement now took place, the joint could be more freely handled, and it was perceived that the head of the tibia was the part principally affected. But the patient had soon a relapse, as she left off the medicine immediately she was slightly relieved; a return to the iodide would set matters right again, but the pain always returned on the omission of the medicine. The woman thus continued for about three years, when her patience became exhausted, and she requested to be admitted into the hospital. Mr. Lee explained to her when she had been received (in the beginning of May 1852,) that trephining the tibia might give her great relief, and she willingly submitted to the operation. He thought that the tension probably existing in the bone might be removed by the operation, and the pain greatly diminished.

The head of tibia on the left side was somewhat larger than on the right and the pain on pressure was principally situated toward the insertion of the ligament of the patella. The shaft of the bone was unaffected, but the swelling on the external aspect of the joint still existed, and an enlargement on the inner side of the head of the tibia could be easily made out. It might thus be inferred that the epiphysis of the bone was the chief portion involved in this evidently tertiary syphilitic affection.

On the 29th of May, 1852, Mr. Lee proceeded, whilst the patient was insensible with cloroform, to make a T-shaped incision over the internal tuberosity of the tibia; and when the bone was exposed, he separated the periosteum with the handle of the knife, and applied the trephine. The latter had a deep crown, and was extremely small in diameter, measuring no more than one-third of an inch in that direction. The instrument was easily worked almost to the opposite side of the head of the bone; the cancellous structure was found very soft, and a few particles of white matter escaped with the blood which was issuing in very moderate quantity. The object being thus accomplished, water dressing was applied, and the patient removed.

The effect of this operation was soon made apparent, and the woman stated that she had not felt so easy for years. For several weeks after the trephining the freedom from the pain which the patient had formerly experienced was maintained, the knee became occasionally stiff and uncomfortable, and the swelling on the outer part of the joint re-appeared, after having quite subsided; but small doses of iodide of potassium with citrate of iron, and an opiate at night, had a very beneficial effect. The patient now began to leave her bed, and left the hospital one month after the operation, her health radidly improving and the knee free from pain.

Two months afterwards, (about three months after the trephining,) she presented herself again, and it was assertained that she could now walk with

great comfort, and had not had any return of the peculiar pain which for years had been causing her so much distress.

Trephining of the Tibia for Necrosis, unconnected with any tegumentary solution of continuity,

(Under the care of Mr. Lee.)

This patient, about twenty-four years of age, was admitted, May 21, complaining of severe pain in the left tibia. She had been suffering in the leg for the last twelve months, and had been under trhatment at Guy's Hospital, where active antiphlogistic means had been used at first. She remained in that institution for twenty-two weeks, underwent various kinds of treatment, and after staying at home for some time, without medical aid, she applied to this hospital.

The left tibia was evidently enlarged through its two lower thirds, there was redness of skin and pain, which latter was sometimes so severe as to drive the patient from her bed. The skin wae not broken, and there was nothing in the woman's history to point to syphilis or hereditary taint. For about five weeks the treatment consisted principally in emollient applications, rest, and tonics; but the tegumentary inflamation, which had somewhat subsided. soon re-appeared, and the leg became œdematous. Mr. Lee, therefore, resolved upon trephining the bone, as he felt confident that the irritation was seated in its interior.

On the 1st of July the patient was narcotized with chloroform, and Mr. Lee applied a small trephine on the most projecting portion of the affected tibia after the skin had been reflected. The walls of the bone were found considerably thickened, and much force was required to perforate them with the instrument. When the latter had brought away a circular piece of bone, the cavity thus formed was felt to be rough at the fundus. It was therefore plain that caries and necrosis were going on within, and that the plate of bone just perforated was the newly-formed shell. Mr. Lee determined, under these circumstances, to remove some more of the shell, so as to get easy access to the diseased portions of the bone.

The trephine was consequenty applied in three more places, forming, with the first hole, a long square, and the bone was cut through with the saw along the lines of junction. A pretty large piece of new shell was thus removed, which, in its thinnest portion, was half an inch think. A rough and plainly necrosed piece of bone was now brought into view, and taken away in the same manner as had been used for the outer shell. But at this stage of the operation it was found that the morbid process had extended to the opposite side of the tibia; both the elevator and the trephine were now used, and Mr. Lee succeded in removing all the dead bone that could be felt and seen. The wound was dressed in the usual way, and the patient carried away after an operation which was necessarily protracted.

The patient's symptoms became now very favourble, the redness of the skin disappearing, the pain left her, and she slept soundly. For the next few weeks a certain amount of osseous detritus and portions of the outer shell were successively detached, but in about two month's time the cavity left by the operation was almost filled up, and the patient discharged in a very satisfactory condition.

We beg to direct attention to the first of the above cases, both on account of the peculiarity of the tertiary lesion of bone, and the good result which

attended Mr. Lee's treatment. It is interesting to notice how regularly the iodide of potassium removed the pain; and this fact would go far to make many surgeons doubt whether this pain depends really on distention, for it does not appear very plain how the iodide could remedy the pressure, except it were in checking further secretions. In tension is not the whole secret of the pain, and it is extremely likely that the inflamation and morbid chages going on in the bone, which are both excited by the presence of the virus, have no small share in the production of pain. The very small amount of matter evacuated by means of the trephine, would in some degree strengthen this supposition.

The above-described operations must be looked upon as of an extraordinary kind; and though the symptoms may have been very obstinate, and have withstood all ordinary remedies, the surgeon is not sure of the condition in which he will find the parts when he introduces his trephine. The most encouraging circumstance is, however, that were even the pain merely nervous and no abscess found, the patient would not be the worse off for the operation. We must nevertheless confess, that when we saw Mr. Lee apply the trephine in the first of the above cases, in which the morbid changes had taken place in the head of the tibia, we felt a little apprehensive as to the fate of the joint, and we are now happy to find that the articulation did not materially suffer.

The second case is especially valuable in a diagnostic point of view, and proves that considerable alterations may take place in a bone situated close to the surface, without the formation of abscess opening externally. Surgeons are so thoroughly accustomed to see caries and necrosis connected with a train of abscesses and consequent fistulous tracts, that cases of this kind should be remembered, they aid us in our diagnosis when called upon to treat long-continued pain in bone.

LONDON HOSPITAL.

Inguinal Hernia; Imperfect Descent of the Testicle; Strangulation; Reduction without Operation.

(*Under the care of Mr. Curling.*)

Norman U——, aged twenty-six, of a pretty healthy aspect, light hair and eyes, and a shoemaker by trade, was admitted Sep. 1, 1852, under the care of Mr. Curling, with very severe symptoms of strangulated inguinal hernia.

It was elicited from the patient, that inguinal hernia of the left side had existed since he was five years of age, and that he had never worn a truss. His health has always been extremely good and he stated incidently that he had never taken any medecine in his life, (with the exception, very probably, of the indespensible and unavoidable teaspoonful of caster-oil soon after birth.) He had never found any difficulty in returning the greater portion of the hernia when it happened to assume upon some exertion, a greater bulk than usual; but the size of the tumour was generally equal to a small fist. Thus the patient had reached his twenty-sixth year without suffering very great inconvenience from the state of his left groin, but he had, for the last few years, perceived that his scrotum contained but the right testicle, and that the left was situated in the inguinal canal.

About a fortnight before his admission the tumour suddenly increased to double its ordinary size; he had much trouble in returning it, succeeded but partially, and was seized with much pain in the abdomen, and obstinate constipation. At last he affected reduction by his own efforts, and had several alvine evacuations without the aid of any medicine.

This attack occurred on the evening before admission in an aggrivated form without any straining effort, or irregularity that he could remember. The tumour now increased to an enormous size, the pain in the abdomen was extremely severe, and the vomiting incessant. Under these circumstrances, a neighbouring surgeon was sent for, who endeavoured to return the hernial protrusion, but could not succeed. It was then thought advisable to transfer the patien to this hospital.

On admission, a tumour, about the size of a double fist, was noticed in the left groin; the swelling was very painful, the bowels constipated, and the vomating almost constant. On manipulating the tumour, the left testicle, which could not be felt in the scrotum, was traced in the inguinal canal, In comparing the left organ with the right, it was found that the former had not reached half the size of the latter, and that it did not possess the resisting structure of the normal testicle.

The poor man was placed into a warm bath, and when the parts were completely relaxed gentle efforts at the taxis were made. These proved successful and a great portion of the herniated mass was returned into the abdomen. A purgative injection was subsequently given, and this brought away a copious evacuation. The symptoms immediately improved and the patient has progressed very favourably since that period. The left testicle lying in front of the external ring; it feels, as above stated, very small, and the hernial tumour, which is now about the size of a small elongated cyst, seems to contain a trifling portion of the intestine and some omentum.

CHARING-CROSS HOSPITAL.

Medullary Tumour of large size, situated in the right Inguinal Region; Partial Removal; Death; Autopsy.

(*Under the care of Mr. Hancock.*)

The outward character and intimate structure of tumours have been of late carefully described, and the labours of such men as Muller, Rokitansky, Paget, and others, have certainly rendered the diagnosis of abnormal growths less difficult than it formerly was; But in spite of this excellent teaching, tumours are met with in practice, the actual nature of which is not easy to determine. Of course the surgeon follows in his investigation the nomenclature and divisions which are current in our times; he first classes the tumour to be diagnosed among the malignant or innocent growths, and afterwards ascertains to which subdivision of the former or the latter it belongs. But when from outward characters, and the history of the case, he has thus (with the assistance of a microscopical examination of the matter deposited on the exploring needle) given it a name, there comes still the question of removal or non-interference. And this applies both to malignant and innocent tumours. As to the former, an operation may be forbidden by a variety of circumstances; and so may it be with the latter, for an innocent tumour may be so situated as to render its removal hazardous; or it may have attained too large a size to be interfered with; or it may, lastly, have so encroached upon certain organs and cavities, that both the latter may suffer by an attempt at removal.

In the case before us, the removal of tumour was determined upon, as there existed some features which rendered its malignant nature doubtful, and

as it was considered that the growth was lying externally to the abdominal parietes. It will be seen by the following details that both the external characters and the use of the exploring needle were well calculated to lead the surgeon astray.

John R——, aged twenty-seven, of dark complexion, and having led an irregular life, was admitted August the 8th, 1852, under the care of Mr. Hancock, with a tumour completely filling the right inguinal region, and of the size of an adult head. It appears that about twelve years before admission, the patient first perceived a small tumour, about the size of a nut, at about the centre of a line drawn from the anterior superior process of the ilium to the umbilicus. This tumour he could easily take up with his fingers and thumb, and, as it were, remove it from the muscular parietes. The patient paid little attention to this growth, and for nearly eleven years it increased almost imperceptibly to about the size of a small orange. Even at that time the tumour could be moved about without any difficulty; but from this period to his admission into Horsemonger Goal (whence he was subsequently removed to Charing-cross Hospital,) a period of about twelve months, the tumour rapidly increased in size, and became a source of great inconvenience, though the patient suffered little or no pain, and allowed the growth to be handled without complaining.

While in prison he continued in this state for about three months, enjoying tolerable health, the tumour increasing in size at the rate of about a quarter of an inch in circumference a month, as evidenced by measurement. The tumour became now a source of great annoyance, as it rendered walking difficult, and pain was complained of. The lower portion of the growth became every week more pointed, and the whole tumour assumed a somewhat conical form. Towards the lower portion some fluctuation was now detected, and an exploring needle was introduced, when about a dessert-spoonful of serum exuded. The upper and outer portions were also tried with the needle, but here the parts were more solid, as only a drop or two of blood escaped.

Mr. Wilkinson, surgeon to the prison, to whom we are indebted for these details, considered, after this puncturing, that the tumour was composed of a number of cysts, that some of these contained serum, and that the other portions were steatomatous. Mr. Wilkinson would have had no hesitation in exterpating the tumour, had he not been prevented by certain prison regulations, as he considered the tumour subcutaneous, and external to the muscular parietes.

The patients health remained good up to his release from prison, on which day he was received at Charing-cross Hospital.

The tumour was then found occupying, as we stated above, the whole inguinal region, from the crest of the ilium to the pubis, it was larger than an adult head, gave no pain on pressure, was elastic and yielded in the lower part, but nodulated and more resisting in the upper, and large veins were coursing in the integuments covering it. The patient's countenance was rather anxious, but his general health seemed pretty good. After he had been a few days in the hospital, Mr. Hancock made a puncture into the lower part of the tumour with an exploring-needle, and gave exit to a small quantity of serum, the latter was examined by Mr. Canton, and found to contain nothing but the usual constituents of that fluid; the exploring-needle being used a second time with the same results, the tumour was looked upon as a multilocular one, and Mr. Hancock, yielded to the request of the patient, consented to remove it.

On the 25th of August the man was placed under the influence of chloroform, and Mr Hancock made a longitudinal incision through the integuments

covering the tumour. After a little dissection a tough texture came into view, on dividing which a large quantity of clots and fibrinous concretions arrested the operation. A portion of the internal oblique muscle was now perceived, and as Mr. Hancock removed the clots and fibrinous masses, he found that he had penetrated into the abdomen. It now became evident that the tumour was mainly composed of these soft meterials, that it had destroyed the abdominal parietes. It was therefore thought prudent to carry the operation no further; the lips of the wound were brought together by sutures, and the man carried to bed. He rallied pretty well for a few days, but it was evident that the sloughing and partial hæmorrhage which were going on in the groin, were telling very unfavourably upon his frame. He died about a week after the operation.

On a post-mortem examination, it was ascertained that the tumour had originated in the walls of the abdomen, and had taken probably at the time it increased rapidly, a direction both towards the groin and abdominal cavity. The decomposition of parts and sloughing were considerable, the clots fibrinous masses, and proper substance of the tumour, had broken down to such an extent as to leave more a cavity than a swelling. It was however found that the parietes of the abdomen had been, in that region, quite destroyed, that the tumour lay on the omentum, and was attached to the cæcum and transverse colon. The adhesions were, however, easily broken down; and when the viscera were examined it was discovered that they were not involved in the disease. No morbid alteration was noticed in any of the larger vessels passing over the brim of the pelvis, and no tumour or deposit was made out in the principal cavaties of the body. Some of the substance forming the bulk of the growth was examined under the microscope, and found to contain numerous cancerous cells.

The deceptive character which was presented by this tumour were:—1st, the fact of its having originally been moveable and distinctly lodged in the subcutaneous tissue; 2nd, the very slow development of the growth (except for the last twelve months,) and the almost complete absence of pain: 3rd, the serum which escaped upon puncturing the lower portion of the tumour; 4th, the excellent state of health which the patient enjoyed up to the time of the operation; 5th the non-appearance of that tendency to ulceration which malignat tumours almost always show.

Now the first of these characters, it may be that the swelling was originally of a simple kind, but that it took on, at a late period, (when its increase became very rapid,) a malignant nature. The explanation of the second character is the same as that of the first, though it still remains a very striking exception, that a medullary tumour should have taken a rapid development for a twelvemonth, without giving rise to any pain.

The serum which we mentioned in the third place, was certainly very puzzling, for the idea of a fibrocystic tumour immediately presented itself, since these tumours have been known to take their origin from the subcutanious tissue of the abdomen. But now that we know that the tumour was malignant it may perhaps be inferred that its vessels having become very thin, had given way, that hæmorrhage had taken place, as happens with the medullary tumours formerly called fungus hæmatodes, and that the serum obtained had merely separated from the clots.

As to the good state of health which we mentioned in the fourth place it, must be looked upon as an exception, though patients are sometimes met with who do not exhibit the cancerous cachexia for a long time. Touching the etndency to ulceration, which was here absent, (fifth character,) it would seem

that the pressure, which genarally gives rise to it, was exerted towards the abdomen. The complete destruction of the walls of this cavity is certainly a feature which points very strongly to malignancy, but the existance of this destruction could hardly be suspected, as there arose no symtoms of any abdominal disturbance.

If we mistake not, our readers will rise from the perusal of this case with the conviction that the greatest care and attention do not shield from occasional error and that, as a rule, we ought to advocate and advise the early removal of tumours although they may not create very great inconvenience. This remark applies to growths both of an innocent and of a malignant kind.

MEDICINE.

DEGENERATION OF DIFFERENT PARTS OF THE SYSTEM.

By James Paget, Esq.,—(From a Review of Mr. Paget's Lectures on Inflammation, in the British and Foreign Medico Chirurgical Review.)

The history of the degenerations of lymph constitutes one of the most important parts of the whole pathology of inflammation. As Mr. Paget has justly remarked, degeneration is a part of the regular series of nutritive operations. for, "to degenerate and die, is as normal as to be developed and live, the expansion of growth, and the full strength of manhood, are not more natural than the decay and feebleness of a timely old age,—not more natural, because not more in accordance with constant laws, as observed in ordinary conditions.

"The study of development has always, however, had precedence in the choice of all the best workers in physiological science, and that of degeneration has been scarcely at all pursued. What little has been done in this department, has had reference solely to the human body; and "almost the only essays at a general illustration of the subject have issued in the ridiculous notion, that as the body grows old, so it retrogrades into a lower station in the scale of the animal creation." The study of the changes of natural degeneration in old age is important, as affording a basis for the interpretation of many phenomina, which are to be regarded as morbid rather in their prematureness than in their essential nature; and these are grouped by Mr. Paget under the following heads:—

1st. *Wasting* or *withering*, as in the ordinary emaciation of old age, some parts being entirely removed by absorption, whilst others are only decreased in size, and lose the succulency of earlier age.

2d. *Fatty degeneration*, as shown in the tendency to the accumulation of fat in many tissues, especially the bones; the *arcus senilis* of the cornea has been shown by Mr. Canton to possess this character; and the fatty degeneration of the arteries of the aged is well known to be a most ordinary occurrence.

3d. *Calcareous degenerations*, as shown in the gradually increasing proportion of earthy matter in the bones, in the ossification of parts that remained cartilaginous during the period of vigour, and in the tendency to earthy deposits in the arteries and other parts.

4th. *Pigmental degenerations*, as shown in the gradual accumulation of black pigmental matter in the lungs, the mucous membrane of the alimentary canal, and even in the coats of arteries.

5th. *Thickening of primary membranes*, as seen in the tubules of the testes, the inner membrane of the blood-vessels, and the walls of the cartilage-cells.

The following are enumerated by Mr. Paget as the characters in which true degenerations are distinguished from disease.

"1st, and before all others, it should be a change naturally and usually occurring in one or more parts of the body, at the approach of the natural termination of life, or, if not then beginning, yet then regularly increasing.

"2. It should be a change in which the new material is of lower chemical composition, i. e., is less remote from the inorganic matter than that of which it takes the place. Thus e. g., fat is lower than any nitrogenious organic compound, and gelatine lower than albumen, and earthy matter lower than all these.

"3 In structure, the form should be less developed than that of which it takes the place: it should be either more like inorganic matter or less advanced beyond the form of the mere granule or the simplest cell. Thus the approach to crystalline form in the earthy matter of bones, and the crystals in certain old vegitable cells, are characteristic of degeneration; and so are the granules of pigment and of many granular degenerations, and the globules of oil that may replace mascular fibres or the contents of gland-cells.

"4. In functi , the part should have less power in its degenerate than in its natural state.

"6. In its nutrition it should be the seat of less frequent and less active change, and without capacity of growth, or of development."

There are many things which show that the assumption of these characters is to be ascribed, rather to a defect, than to a perversion of the vital force or of the condition of nutrition:

"Thus (*a*) these are all apt to occur in a part of which the functions are abrogated; a motionless limb wastes or becomes fatty as surely as an old one does. (*b*) They often occur, too, in parts that fail to attain the development for which they seemed to be intended. Thus, e. g., fatty degeneration usually ensues in the cells of unfruitful Graafian vesicles. (*c*) They bear also a certain general analogy to the changes that ensue in some of the materials that are habitually excreted from the body; in the construction of which materials one seems to have an instance of the gradual supervention of the ordinary or imitable process of chemistry. (*d*) Again, they display living parts tolerating the presence and incorporation of inorganic or dead matter, fat being commonly infiltrated about degenerate cells and membranes, and earthy matter with it, even in the crystaline form. (*e*) And lastly, and perhaps most clearly, the organs of degenerations from defective, rather than from perverted, conditions of nutrition, appears in the fact that one, at least, (namely degeneration), of them may be produced artificially.

The degeneration of lymph may commence at any period after its formation; and that which most obviously determines it, is the continued suspension of the conditions of nutrition. We shall first speak of its fibrinous element. In the first place, this may waste or whither, becoming firmer and drier passing into the state which Rockitansky has called "horny;" the fibrin in these cases shows no marks of development into tissue, but retains its ordinary structure, being only drier and more compact. *Secondly*, fibrin is subject to a degeneration, which is comparable to the fatty degeneration of ordinary tissues; this is an extremely frequent occurrence, presenting itself in the fibrinous effusions poured out in the lower forms of inflamation, or in those of cachectic individuals. The softening and disintegration of the clots within the heart, which Mr. Gulliver has described, correspond closely with the like processes

in inflammatory lymph. The whole substance is seen to be dotted with granules, which are known to be oil particles by their peculiar shining, black-edged appearance: and the fibrin, no longer rendered transparent by acetic acid, loses its toughness and elasticity. It appears to be usually by such a degeneration, that fibrinous matter, which has firmly coagulated, softens and becomes capable of absorption:

"I suppose it may be considered as a general truth, that the elements of a tissue cannot be absorbed so long as they retain their healthy state. There is no power of any absorbent vessels that can disintegrate or decompose a healthy portion of the body: for absorption there must be not only an absorbing power, but also a previous or concurrent change, as it were a consent, in the part to be absorbed; so that it may be reduced (or rather may reduce itself,) into minutest particles, or may be dissolved. And this change is probably one of degeneration, not death, in the part; for dead matter is rather discharged from the body than absorbed.

"Now there are some facts which indicate the probability that the fatty degeneration is that which commonly precedes the natural absorption of many normal parts; or rather, that, in the change which they undergo before absorption, fatty matter is one of the products, and that the principal evident difference between the atrophy of a part which is manifested by its wasting, and the atrophy which is manifested by fatty degeneration, is, that the fatty matter, which is absorbed in the former case, is retained in the latter. However this may be, it is certain that the disintegration and fatty degeneration of the fibrin products of inflammation bring them into a state most favourable for absorption. indeed, one may see in fibrin thus changed many things which in regard to the the fitness for absorption, make it parrellel with chyle. Of such absorption of fibrin we may find many instances. In rheumatic iritis we may believe the lymph to be fibrinous; but we see its complete absorption taking place; and the recent observations of Dr. Kirkes on the rarity of adhesions of the pericardium in comparison with the frequency of pericarditis, may be in the same manner explained. In rheumatic pericarditis we may be sure fibrin is effused and the observed friction-sound has, in at least one case, proved its coagulation yet in this case, when death occurred four months afterwards, scarce a trace of fibrin was found in the pericardium: it has been absorbed, and the degeneration I have been describing was probably the preparation for its absorption.

Thirdly, examples of the calcareous degeneration of fibrin are shown in the ordinary formation of phlebolithes from clots of blood, and in the calcareous deposits which are found imbedded in the fibrinous vegetations of the valves of the heart. This form of degeneration, however, is much more frequent in purulent fluids, and in later developments of lymph. *Fourthly*, pigmental degenerations are often seen in the fibrinous lymph effused in peritonitis, which presents shades of gray and black, that are due to the presence of pigmentary granules.

A similar series of degenerations is seen in the *corpuscular* elements of lymph. *First*, their withering is seen in certain elements of a dull ochre-yellow-coloured, and half dry material contained in lymphatic glands that have undergone chronic and nearly stationary scrofulous enlargements. In this substance are found abundant collapsed and shrivelled cells, which might be supposed to be dried pus-cells, or corpuscles of chronic tuberculous matter, were it not that some of them present an approach to the character of fibre-cells into which it is certain that neither pus nor tubercule-cells are ever developed

The corpuscles found in the pus of chronic abscesses, are believed by Mr. Paget, from their likeness to the foregoing, to be rather withered lymph-corpuscles, than true pus-cells. *Second*, the lymph-cells are changed by fatty degeneration into granule-cells, or exudation-corpuscles; which, as Reinhardt has shown may also be derived by a similar process from the primary cells of almost all other, both normal and abnormal, structures. This form of degeneration is particularly apt to occur in the products of inflammation in the nervous centres and in the lungs, but it is by no means confined to these organs; and it may take place alike in the early forms of lymph-cells, and after they have already elongated and attenuated themselves into fibre-cells, and also after they have degenerated into pus-cells. The following are, briefly, the stagesof this transition, which corresponds exactly with that so commonly obsesvable in the cells of the liver and the kidney:

"The lymph-cells, which may have at first quite normal characters, such as I have assigned to 'primordial cells,' present a gradual incaease of shining black-edged particles, like minute oil-drops, which accumulate in the cell-cavity, and increrse in number and sometimes in size also, till they nearly fill it. The The fatty nature of these particles is proved by their solubility in ether: and their accumulation is attended with a gradual enlargement of the cell, which also assumes a more oval form. Moreover, while the fatty matter accumulates the rest of the contents of the cell, becomes very clear, so that all the interspaces between the particles are very transparent; and coincidently with all these changes, the nucleus; if any had been formed, gradually fades and disappears."

This kind of degeneration, as in the case of the fibrinous element, appears to be preparatory to absorption, as probably happens in the "clearing up" of the solidified lung after an attack of pneumonia. The calcareons degeneration of the lymph-cells is not so often seen, but sometimes presents itself coincidently with the preceeding; of the pigmentary degeneration we have a very common example in the colouring of the cells of bronchial mucus already referred to.

The most frequent of all the degenerations of lymph, however, is into *pus*, this change ensuing in nearly all cases in which the lymph is placed in conditions unfavourable for its devolopment. That such a change does take place, there can now be no doubt, since every gradation can be seen, from the most characteristic form of lymph-cell to that of pus-cell; and it is very questionable whether pus is ever, what it was so long undoubtedly regarded, an original or primary product of inflammation. "We cannot," as Mr. Paget observes, "always discern a preliminary lymph stage: but neither can we always distinguish lymph-cells from pus-cells, nor can we see in how short a time the transformation may be accomplished." Other rudimental cells, besides those of the lymph, may be so altered as to take on the appearance of pus-cells; thus in many of the supposed cases of pus in the blood, the bodies taken for pus-cells were certainly only altered white corpuscles. When it occurs as a product of inflammation, however the pus-cell may be pretty safely regarded as an ill formed or degenerate lymph-cell; and the variety of form which it will present will partly depend on the previous quality and grade of development of the lymph-cell, and partly on the further degenerations which may have taken place, after the characters of the pus-cell have been acquired. The following is Mr. Paget's account of the typical conditions of the pus-cell; and of the principal degenerations which it may undergo:

"In specimens of what might be called 'good' pus, we may find three prin-

cipal forms There are—1st, some corpuscles presenting the peculiar and well known granulated or wrinkled appearance of pus-cells, but from which water will raise up no cell-wall; 2, from others, like these at first sight, water will raise a cell wall, and will show that the former kind consists of only such a substance as forms the contents of these. 3d, in others even when no water is added, a cell wall is visible, and within there are granulated contents, with a more or less distinct nucleus imbedded in them. In all these forms, moreover, the addition of acetic acid usually displays a single or bipartite or tripartit nucleus. Now, it may be that these represent three different stages of the pus-cell, either developing, or more probably, degenerating; but I think it is much more likely that these forms are the results of the purulent degeneration beginning in lymph cells at different stages of their development. There is so remarkable a correspondence between these three varieties of pus- corpuscles, and the three chief forms which I described as observable in the development of the primordial cell of lymph, that one cannot but suspect that the three forms in the pus represent corresponding and similar degenerations from the three forms in the lymph.

"When once formed, the pus-cells, if they are retained in the body, have no course but to degenerate further; it is characteristic of their being already degenerate, that they can neither increase nor develope themselves. The various corpuscles found in pus, besides those I have already mentioned, must find their interpretation, in these degenerations; for the pus-cells are prone to all the degenerations that I described as occurring in the lymph-cells.

"*a.* They may wither as in the scabbing of pustular eruptions, or in long retained and half dried strumous abscesses.

"*b,* Or they may be broken up, whether before or after passing into the fatty degeneration, which is one of the most common changes, and in which they are transformed into granule-cells. It is this breaking up into minute particles which probably, precedes the final absorption of pus.

"*c.* Or lastly both the cells and the fluid part of the pus may alike yield fatty and calcareous matter. and this may either remain diffused in fluid, or may dry in a firm and mortar-like substance.

"It is to such degenerations as these, in various degrees and combinations and variously modified by circumstances, that we must ascribe the diverse appearances of the contents of chronic abscesses and the substances left after their healing."

But the same kinds of degeneration may occur in the products of inflammation after they have advanced further towards complete organization. Thus adhesions not unfrequently degenerate by wasting, seldom or never by a passing into the fatty state; but very commonly by becoming calcified, the product having sometimes an approach to the character of true bone, but much more frequently having the earthy matter deposited in an amorphous form, as if, as Rockitansky has remarked, it was a residue of the transformation of the more organized tissue whose soluble parts have been absorbed after decomposition and lastly by developing pigmentary matter, which is sometimes seen in adhesions of the pleura, but much more commonly in those of the iris.

Such, then, according to Mr. Paget, are the principal forms of degeneration exhibited by the more or less developed products of inflammation. We consider that he has done good service in thus attempting to classify them, and to point out the direction for further enquiry. The whole subject of degenerations may be regarded, in fact, as of even more pathological importance than that of de-

velopment, and deserves the fullest investigation For it *is the essential* character of by far the greater number of morbid processes, that the nutrative material is applied to some purpose different from its regular destination; and that the product is, in relation to the normal one, of a degenerate kind. And even where an attempt is made (*so to speak*) to develope a normal structure, as in the organization of lymph thrown out for the repair of injuries (whether produced by external causes, or by the morbid actions of tissue itself), that structure until it is fully adopted into the system and made completely a part of it, is peculiarly prone to undergo degeneration. Now it has been from dwelling upon this very frequent destiny of fibrinous exudations, and from passing by the cases in which they do remain as constituted parts of the organism, that Mr Simon has been led to the notion that fibrin is itself a degenerating product. It may be that the last named instances are the exception rather than the rule; but this is because, in all ordinary cases in which these exudations are thrown out, causes of denenera tion are operating. It is in those simplest cases in which the reparation proceeds with the least amount of those disturbing influences which tend to produce degeneration,—as, for example, in the reunion of the two ends of a tendon after subcutaneous division,—that the osganization of the fibrin goes on most after the fashion of ordinary nutrition, and with the most complete result; and we seem, therefore, clearly entitled to affirm, that of all the elements of the blood, the fibrin is that which is the most prone to become organized, and is that which affords the ordinary pabulum for the development of the tissues.—*Brit. and For. Med. Chirurg. Review, April*, 1851, *p.* 487.

NATURE OF THE FIBRIN OF THE BLOOD, AND ITS RELATION TO DISEASES.

(From a Review of Mr. Simon's Lectures on the subject, by the Editor of the Brit. and For. Med Chirurg. Review.)

[One of the most important opinions in pathology lately advanced, is that of Mr. Simon and Dr. Zimmerman, viz., that "the Fibrin of the Blood is not to be regarded as its most organizable portion, but as an element resulting from the *derangement of the tissues*, and destined to speedy elimination from the circulating current," in opposition to the opinion commonly received among physiologists, viz., "that Fibrin is that ingredient of the blood, which, in the ascending scale of development, stands next for appropriation into the living textures of the body, and which represents the ripeness, perfection and nutritiveness of the blood." The reasons Mr. Simon advances in opposition to the commonly received doctrine that fibrin is the most organizable and nutritive part of the blood, are as follows:—]

"First, I find that fibrin is undiminished by bleeding, however, frequently repeated; nay, that it often or even usually increases under this debilitating treatment; its highest figure given in Andral's book (10·2) was at a fourth bleeding: and Schermer found it was high as 12·7 at the third venesection in a case of pneumonia. I find that under many other circumstances of exhaustion and weakness and inanition, during the progress of starvation, during diseases essentially anæmic, during violent fatigue, and the like, its proportion has been found at least as high, perhaps higher, than in the inflammatory process. And as in these respects I find its proceeding to be in direct contrast to that of the red globules (which we know to be potential elements in the blood, and which are at once reduced by bleeding or starvation, so also do I find a similar contrast in another striking particular. Messrs. Andral and Gavarret, in the course of their extensive researches in the comparative phisiology of the blood

ascertained that an improvement in the breed of an animal tended always to increase the proportion of its coloured blood-corpuscles; they found that the same improvement tended likewise to diminish the proportion of its fibrin. And I find further indications of the same inverse ratio between the fibrinousness and the perfection of the blood, in the facts—that there is little or no fibrin in the blood of the fœtus, none in the egg, none in the chyme, and less in the blood of the carnivora (who feed on it) than in that of the herbivora.

Some of these facts derived from very different sources, appear quite inexplicable on the theory that fibrin is essential to the progressive development of the tissues; and the opposite inference seems unavoidable, that it must be considered an excrementitious product, derived from the waste of the tissues or the oxydation of the blood, and in progress of elimination from the system. This conclusion, carried into the domain of pathology, would lead us to suppose that an augmented proportion of fibrin in the blood (whether occurring in active disease, or within the limits of apparent health) can be taken as an indication of increased labour and waste in certain elements of the body, not of an increased development in the resources and nutrition of the blood. And on the same grounds it would appear that a super fibrination of the blood, in acute inflammatory diseases, must be regarded as a consequence and effect of those diseases, not as their cause, and not as a primary affection."

Again when speaking on a subsequent occasion, of the ulterior development of fibrin, Mr Simon remarks:

"It appears, then, that fibrin may remain stationary, and be nourished, or it may degenerate, and decay: this much is certain. But, may it advance? may it be developed into any higher form? into any tissue?—Notwithstanding the prevalence of a very general opinion to the contrary, I believe I may venture to question its possession of this power, and may say, that I entertain extreme doubt whether, of itself, it ever shows the slightest disposition to cell-formation, or to any process of self-development.

"Unfortunately, our opportunities of watching its solitary behaviour are very few; for, in almost every instance that can be thought of, albumen (which is probably the real regenerator of the tissues) is likewise present; and that great developmental activity, so often and so glowingly ascribed to fibrin, may, with at least equal probability, be considered the work of this associated albumen, for (on this latter assumption) the fibrin could merely be considered to furnish an inert mechanical support. For think, if fibrin were that restless element of growth and vital expansion which some have fancied it, what a world of activity there would be in an aneurismal sac! A large aneurism, filled with laminated clot, has almost as much fibrin in it as the whole body put together; and yet it shows, on microscopical examination, no evidence of activity or of growth. At its circumference its pressure may have irritated surrounding parts, and may have provoked inflammatory effusion from them, but in the interior all is stationary and quiet. Towards the cavity, where the formation is most recent, lie the blood-corpuscles in a net-work of fibrin—the former in such numbers, that the latter can but very imperfectly be seen, but in passing outwards, as the corpuscles seem more and more wasted; the fibrin begins to show more distinctly, always adapting its meshes to the material within them so that innumerable blood-cells are seen, each in its separate setting of fibrin: in getting still nearer to the circumference of the sac, the arrangement becomes confused from the closer consolidation of the fibrin; but in no part of the structure have I been able to see any trace whatever of new organization.

There is a similar reluctance to the initiation of organic development in those other intra-vascular clots which form in tied arteries. They undergo changes referable to their blood-corpuscles, and they become pale and contracted; but their fibrin may remain for many weeks, or perhaps permanently, unaltered, except for some increase of density. I have seen it after the lapse of six weeks, showing only a vague appearance of longitudinal striation, with no essential change of physical character, and without the slightest trace of new development in its substance."

And after alluding to Dr. Zwichy's observations on the metamorphosis of arterial clots into fibrous tissue, which he considers as depending on "some new influence being imparted to the clot by the prolongation of blood-vessels into its substance, much more than on any specific faculty of organic development residing in the fibrin itself," he continues:

"I do not wish it to be understood as, in my opinion, a proved and certain thing, that fibrin is insusceptible of ulterior development; but I find, as yet, a want of sufficient evidence to establish its possession of this power; and in the examination, both of intra-vascular cots and of inflammatory exudations, I find several facts which apparently militate against such a conclusion. In all such products, the fibrin has shewn itself either stationary or retrogressive; either lying as first deposited, or contracting more and more densely; or altering, only to undergo degradation. So far as my knowledge extends of adhesive inflammation, and of the several reparative processes, I see no evidence that fibrin takes a more important part in them than that of holding the true albuminous blastema within its meshes, and thus occasionally serving as a provisional matrix and scaffolding for the development of cells, fibres and blood-vessels, and I cannot but suspect that those who have ascribed to fibrin so large a share in the process of growth, have been somewhat under the influence of that prejudice to which I alluded in a former lecture, and have promoted this material to so high a rank in their pathology, merely because of its physical tendency to settle in a solid form."

[These opinions of Mr. Simon are answered as follows by the Editor of the 'British and Foreign Medico-Chirurgical Review.']

1. "Fibrin is undiminished by bleeding; nay, it often, or even usually increases under this debilitating treatment." This only proves that the process, whatever be its nature, on which the generation of fibrin depends, is not checked by loss of blood, a fact which affords no indication whatever that the fibrin is a product of disintegration. If the fibrin be, as we believe, that element of the blood which is most directly and constantly required for the maintenance of the nutritive operations, it is easily conceivable that the All-wise Creator should have placed this process out of the reach of those accidents which affect the proportion of other less important elements of the blood; just as the action of the heart is entirely withdrawn from the control of the human will, and the respiratory movements are provided for by an instrumentality over which it can exert but a subordinate influence. Let it be remembered that if fibrin be (as we believe) albumen undergoing vitalization, its *material* will always be ready, so long as there is any albumen in the blood; and if (as seems not improbable) the very act of circulation through the living tissues is one means whereby the vitalizing influence is exerted, this will be perpetually going on, so long as the circulation continues.

It must be borne in mind, too, that a new supply of fibrin, as well as of albumen, is introduced into the blood after every act of digestion; for there is

distinct evidence that fibrin is generated (at the expense of albumen) during the passage of the chyle through the lacteals; and if the researches of M. C. Bernard are correct the liver exerts a similar elaborative agency upon the albuminous matter which has been received into the blood of the vena portæ. Further, in all the cases alluded to by Mr. Simon, a very active inflammation was somewhere going on in the system; and thus, according to the current doctrine, an unusually active production of fibrin was taking place at the expense of the albumen of the blood. If not generated from this material, but resulting (as Mr. Simon thinks) from the disintegration of the tissues, how is it that in an attack of pneumonia, or of acute rheumatism, in which there is no reason to suppose that any extraordinary disintegration of tissues takes place, the proportion of fibrin in the blood mounts up to three, four, or five times its normal average?

2. "During diseases essentially anæmic, during violent fatigue, and the like, the proportion of fibrin has been found at least as high, perhaps higher, than in inflammation." We very much question whether, in any of these cases, the increase in the proportion of fibrin took place without the presence of the inflammatory condition. Every one knows that inflammatory complaints are peculiarly liable to occur when the system is imperfectly nourished: and Andral distinctly states, that in the three dogs which he submitted to experiment, two of them being wholly deprived of food, and the third partially so, the elevation in the proportion of fibrin from 2 3,2·2, and 1 6 parts, in the three individuals respectively, to 4 5,4, and 3 3, parts, was *coincident with inflammatory changes in the stomach*. Further, as in these diseases "essentially anæmic," there is no reason to suppose that a peculiarly rapid disintegration of the tissues is going on, we cannot see how the increase of fibrin in the blood of patients suffering under them, is in the least degree confirmatory of Mr. Simons doctrine. That the deficiency of nutrient material does not prevent the generation of fibrin out of what the blood contains, may be explained as we have just shewn, in a manner very different from that which Mr. Simon would have us adopt.

3. The argument founded upon the comparative amount of fibrin in different breeds of animals, as determined by the observations of Andral and Gavarret, can scarcely be admitted to have much weight, when it is recollected, that the breeds here spoken of were *herbivorous* animals, to which it is natural to possess *more* fibrin and fewer corpuscles than the carnivora, and that they were *domesticated* animals, in regard to which the phrase "improvement of breed" often means something very different from an increase of the general vigour. We can fancy that a stall-fed cow, yielding her eighteen quarts of milk daily, would be regarded by the farmer as a much more valuable animal than a hardy mountain cow affording not one third of that quantity: but the latter is well known to be the one possessing the greatest amount of vigour, and to be the least susceptible of disease. It is, in fact, the object of the breeder, to produce a state of *artificial plethora;* and in this condition, as is well known, is characterized in man by an increase in the proportion of red corpuscles, without any corresponding increase, or even with a diminution, in that of the fibrin of the blood. If we extend our observations on comparative physiology a little further, we find that the red corpuscles, which Mr. Simon affirms to be "potential elements" of the blood, are altogether absent in the lowest known animal of the vertebrated subkingdom (we allude to the curious little *amphioxus* or lancet,) and that scarcely any approach to them is presented through the whole invertebrated series, notwithstanding that among some of these there is an extraordinary amount of vital activity; consequently we cannot help looking upon their

presence as connected with some function which it is to be specially, if not exclusively, performed by the vertebrata. On the other hand, the presence of fibrin is universal; and its proportion in the blood is found to bear a close relation to the formative activity, in cases in which this is subject to periodical variations. Thus Mr. Newport found, that in the larva of the insect, at the period immediately preceeding the exuviation of the skin and the formation of a new integument, the blood is extremely coagulable; whereas for some little time after this process has been performed, the blood is so deficient in coagulability, that it does not (as at other times) close the orifices of wounded vessels,—as if in consequence of the temporary exhaustion of the plastic material by the new production of the tegumentary tissue. In the pupa state, during which the formative activity is the greatest, and the disintegration of the living tissues must be reduced to its minimum by the complete inactivity of the animal the coagulability of the blood is great. But the imago state, in which little or no further growth or development takes place, and in which, on the other hand, the activity of movement might be expected to produce an unusual disintegration the plastic element seems to be almost entirely withdrawn.

4 "Further indications of the same inverse ratio between the fibrinousness and perfection of the blood," are found by Mr. Simon in the facts, "that there is little or no fibrin in the blood of the fœtus, none in the egg, none in the chyme, and less in the blood of the carnivora (who feed on it) than in that of the herbivora." We are at a loss to see what possible argument can be drawn from any of these facts, except the first; and of this—the absence of fibrin in the blood of the fœtus—we must take leave to express a doubt. Mr. Simon does not cite his authority for the statement; and it is entirely opposed to all we know of the condition of the blood in other organisms, in which formative actions are going on energetically—as in the larvæ of insects, which may be regarded as embryos fitted to maintain an independent existence, and to procure and ingest their own food. The albumen of the egg does not stand in the relation of blood, but in that of food, to the chick; this food has to be assimilated by the nutrient organs before it can be converted into solid tissue; indeed, it would seem to be the special purpose of the germinal membrane to convert this raw material into the plastic circulating fluid. To advance the absence of fibrin in the egg, therefore, is an argument for its non-essentiality in the blood, is just as unsatisfactory an argument as it would be to say, that because fibrin is not to be found in bread or milk, its presence in the blood has no relation to the nourishment of the tissues.

Mr. Simon does, indeed, make a near approximation to this absurdity, in advancing the absence of fibrin in the *chyme* as one of his "indications." But what does he say to the presence of fibrin in the *chyle;* to its progressive increase in proportion as the chyle advances along the lacteals, and more especially after its passage through the mesenteric glands; and this under circumstances which almost exclude the possibility of attributing its first presence and subsequent increase to the introduction of any disintegrating material,—to anything, in short, but to that assimilating operation which prepares the chyle for the part which the blood is to perform, and gradually converts it into a liquid which is fit to circulate through the sanguiferous system? We are equally astonished that Mr. Simon should think that the difference in the proportion of fibrin in the carnivora and herbivora, taken in connection with the nature of their food, furnishes any support to his views. Surely he must know full well, that the fibrin of muscular flesh is reduced back to the state of albumen in the digestive

process, and that there is no difference between chyme formed from animal food and that produced by the digestion of vegetable substances, except that which depends upon the different proportions of its protein compounds, its oleaginous matters, its saccharine and other constituents. So long as the food supplies albuminous matter in adequate amount, so long the proportion of fibrin in the blood is capable of being sustained; and, as we have just now remarked, we can see no reason why it should not be sustained, until all the albumen of the blood is exhausted. What is the explanation of the larger proportion of fibrin in the blood of herbivora than in that of carnivora, we do not pretend to say: but Mr. Simon's theory does not help him to accounting for it, since he will scarcely maintain that the disintegrating processes are more rapid in the former group than in the latter,—the very contrary, indeed, appearing to the fact. It is to be remembered, however, that little reliance can be placed upon the numercial proportion of this or any other of the elements of the blood, in estimating the share which it takes in the formative processes. If the current doctrine be correct, there is a continual production of fibrin, and as constant removal of it from the circulating current by the formative operations taking place throughout the body; and the amount which circulates in the blood may be considered as a sort of "floating balance," which affords no indication of the magnitude of the two sets of transactions which it serves to accommodate.

Such are the merits, in our apprehension, of the principal arguments assigned by Mr. Simon for abandoning the orthodox doctrine, and embracing the heresy newly imported from Germany. We trust that we are as much inclined as Mr. Simon to exchange old errors for new truth, provided the latter can be brought to our understandings; but we must be satisfied it is truth, before we abandon for it the doctrines that have been current among physiologists and pathologists ever since they began to reason correctly on such subjects. Of the objections which he raises to the current notions, on the basis afforded by the history of the ulterior development of fibrin, we shall not now stop to point out the fallacies.—*Brit. and For. Med.-Chirurg. Review, April.* 1851, *p.* 472.

MALIGNANT TUMOURS OR CANCERS.

By George Murray Humphry, *Esq., Downing College, Surgeon to Addenbroke's Hospital, Cambridge.*

[The word "destructive," if applied to these tumours, would probably explain their nature and tendencies better than the one usually employed. They are destructive, not only destroying the tissues in their immediate neighbourhood, but also by the new tissue deposited being peculiarly liable to decay, causing breaches or chasms which may increase to a very great extent. These, then, are the leading features of these growths; first, degeneration of the natural tissues around it; and secondly, the tendency to decay of the tumour itself. There is no doubt in the investigation of this class of growths the microscope is a very valuable means of assisting us; nevertheless, the many difficulties and sources of error necessarily intertwined with the use of this instrument, should warn us against too implicit a reliance on the results which it has hitherto afforded us in morbid anatomy. It cannot be relied upon till the observer has bestowed much attention and time to its use. Time may cause it to be regarded as another and surer test, but at present the science is too much in its infancy to enable us to dogmatise upon the information it affords, without giving legitimate weight to other considerations.]

It is commonly agreed that the greater number of cancerous growths consist of a fibrous stroma, disposed in meshes which contain nucleated cells, nuclei, and gradules, together with a thin serous or gelatinous fluid. The proportions in which these three elements are combined, vary greatly in the different species of cancer. In the scirrhous or hard cancer, for instance, the fibrous structure predominates: in the encephaloid or soft cancer the cells are more abundant; and the colloid or gelatiniform cancer is composed in great measure of jelly-like fluid. Neither of these elements examined separately are found to exhibit any decidedly distinctive characters, the peculiarity of the cancerous growth consisting not so much in the materials of which it is composed, as in the mode of their arrangement.

[The fibrous basis of cancer seems to resemble closely the common areolar or fibrous tissue, being composed of the peculiar compressed wavy filaments, intermixed often with well developed filaments of elastic tissue. In some cases, however, this fibrous basis does not possess so distinct a structure.]

The cell element of cancer has naturally proved an object of much interest, inasmuch as the essence of the disease would seem to be almost concentrated in it, the rapid growth and self destructive or decaying tendencies of cancer being to a great extent proportionate to, if not dependent on, the quantity of cells developed in the meshes of the fibrous stroma. Pathologists hoped, therefore, to find some distinctive structural peculiarities attaching to these cells whereby the nature of the disease might be recognised at once and with certainty. This expectation has not however at present been confirmed by observation: it is the opinion of those best qualified to judge, that cancer cells do not present any decidedly characteristic peculiarities, that they vary in appearance a good deal, that in each of their different forms they resemble some of the primary cells of other tissues, and that in the form, which from its frequent occurrence in specimens of well-marked malignant disease might be taken as the type, they are not to be distinguished from the cells of cartilage or the cells composing the deeper layers of epithelium. Indeed some of the structures which present in a sufficiently well-marked manner the practical tokens of malignancy, do not appear to contain the cancer cell at all as an ordinary constituent; such are some of the varities of cancer of the skin and cancer of the muscular coat of the intestinal canal.

The cells commonly found in cancers, which may be seen in the fluid scraped from a section of the morbid mass, and to which the name "cancer-cells" has been applied, are larger than blood-globules, of about the same size or rather larger than pus-globules, composed of a tolerably well-defined cell-wall, with one, two, or more nuclei, in which nucleoli may be often seen. The cell-wall is rendered transparent or dissolved by acetic acid, the nuclei being unaffected. The cells vary a good deal in shape, being round or oval, or elongated at one or both ends into a caudate or spindle form, or they may send out processes from various parts of their circumference. It is the general opinion, that they do not, at least ordinarily, advance beyond the cell-stage of organization, that they are not transitional to any other form of structure, but that having attained to a certain size they are disintegrated, and their constituent atoms are returned to the fluid from which they were derived: or by a process of endogenous growth they may become the parents of other secondary cells formed in their interior: that is to say, their nuclei and nucleoli enlarging and converted into cells may fill up the interior of the parent, and at the period of its dissolution may become set free and undergo like changes in their turn, the materials

for the growth of their nuclei being furnished by imbibition through their walls. Some authors have described the cells to increase by fissiparous gemmation, and Dr. Bennett speaks much of their occasionally undergoing a retrograding process; the nuclei disappearing instead of enlarging, the cell-wall shrinking, collapsing, becoming infiltrated or intermixed with oil, and ultimately resolved into fragments and granules. He finds the yellowish masses often seen in cancerous tumours to consist in a great measure of cells thus altered.

With regard to the nature of these cancer-cells, and their physiological relations to the fibrous element and to the natural tissues of the part affected it is not easy to form an opinion. By some authors they have been looked upon as independent existences, like entozoa or fungi, growing in the body and propagating themselves to distant parts through the medium of the blood by sporules, which find their way into the circulating current, either through the delicate walls of the capillaries, or through openings in the small vessels. It seems to me on the whole most probable that they are the result of some peculiar influence exerted upon the nutritive elements of the blood effused among the tissues by an altered or morbid state of the nutrition of the part, being in this respect, to a certain extent, analogous to pus-cells, which they also resemble, in their capacity to undergo further organization; and in their being interminal, not transitional, stages of development. The two morbid products differ, however, in the important particular that the cancer-cells possess the property of self-multiplication, absorbing and assimilating the elements of the blood, whereas the pus-cells are dependent for their increase on the continuance of the inflammatory process causing fresh exudations, endued with similar tendencies to those from which they sprang themselves. There are some circumstances such as the accumulations of pus in certain cavities or abscesses, which render it probable that even the pus-cells exert a certain assimilative influence upon the fluid or even the solid lymph effused in their immediate proximity, but they do so only in a slight degree, and with a force scarcely comparable to that of the cancer-cell. This point of difference between the products of inflammation and the elements of cancer, is of great importance, and constitutes one of the most marked features of their distinction.

Besides the cells just mentioned, there are commonly found floating in the same fluid nuclei and granules, with perhaps other cells, such as those which are in process of development into tissue, pus-cells, &c. Oily matter, in the form of minute globules dispersed through the mass, is also a very usual constitutent; and there are commonly found many elements not belonging to the cancer itself but to the structure in which it is produced, such as striated muscular fibre, fat, glandular tissue, &c. The blood-vessels are often of large size, and very numerous; they are generally admitted to be derived from an increased growth of the vessels naturally supplying the affected part.

It should be observed here, of the two chief elements of cancer, that they are not only intermixed in very different proportions, but that they do not invariably coexist. In some few instances the cells alone are present, being infiltrated among the tissues of the affected organ, or they may be intermixed with lymph effused in consequence of inflammation taking place in a person who is the subject of malignant disease, or in whom the cancerous diathesis is strong. So on the other hand cases now and then occur in which a tumour, consisting only of fibrous stroma, without the intermixture of any cancer-cells, and resembling, therefore, a fibrous tumour, may exhibit the destructive and inveterate qualities of malignant disease. As a general rule, the rapidity of growth and

the tendency to decay in a tumour are preportionate to the quantity of cells developed in it, they are greater in encephaloma,less marked in scirrhus, and least marked of all in those specimens of scirrhus where the fibrous element constitutes *the entire mass.*

The relation which the cancerous elements bear to the natural tissues, and their effects upon them, are points of so much interest, that we must again revert to them. It is evident that at first the natural and morbid structures are to say the least, very closely connected together. Most Pathologists agree that the cancerous matter is infiltrated among the original elementary parts of the parent tissues, and occupies nearly all their interstices; that in process of time the elements of the tissues become compressed. appear to be blended with the deposit into a homogenious mass, and gradually become atrophied and disappear. Indeed the connection between the natural and the morbid structure is in some cases so intimate, the one being scarcely diatinguishable from the other, as necessarily to suggest the idea that there may have been not merely a blending but an actual transformation of the healthy into the cancerous tissue. This is particularly true in some of the glandular organs. In the liver for instance, the first indication of the disease is commonly afforded by a mottled appearance, resulting from a slight discolouration of some of the lobules; they retain their proper shape and size, and their distinctness from the neighbouring lobules, but they are of white colour; and the natural components of the lobules are replaced by, if not transformed into, the elements of cancer. As the disease increases, adjacent lobules are affected, their structure is more completely altered, their individuality lost, and they are fused into one mass.

When once the cancerous change has commenced it is almost sure to spread. The assimilative energies of the morbid product are so intense that the neighbouring tissues yield under its superior force, their own nutritive powers are as it were prostrated, and they soon disappear in the same manner as the part first affected. Before being thus completely destroyed they are found in many instances to undergo certain alterations, such as atrophy or fatty degeneration, which indicate that their nutrition is impaired and that they are ready to fall an easy prey to the destructive influence which is encroaching upon them. Whether these alterations are entirely the result of that influence operating on them, though at a distance, or whether they are in part also the result of some inherent deficiency, which is the common cause both of the cancer and the atrophy, I cannot say, but am inclined to think the latter is the case. At any rate it is a common thing to find bones afflicted with cancer in some parts, and atrophied or greasy at others; the mammary gland, which is the seat of scirrhus, in some of its lobules is often shrunken, or loaded with fat in others; sometimes you see, as in the specimen shown you on a former occasion, a small scirrhus lump under the nipple imbedded in a mass of fat, which occupies the seat and retains the shape of the mammary gland. In like manner, when a muscle is affected with cancer, the fibres contiguous to the morbid product, are often observed to be in a state of more or less advanced fatty degeneration; their tranverse markings are indistinct or invisible, their nuclei imperfect, and they are prevaded with oil. Further, although the general wasting of the body, so commonly accompanying the progress of cancer, may be occasioned by pain, discharge and various accidental causes, it must be allowed that the disproportionate quantity of fat often found in such cases, both under the skin and about the internal organs,—that fat having a remarkable deep yel-

low colour—is attributable to some peculiar condition of the nutritive functions dependent on or associated with cancer.

It seems to be the general result that the higher or more organized tissues —the striped muscular, and the glandular—are commonly affected with atrophy and degeneration in connection with cancer, and the more simple and less organized tissues—the unstriped mascular, the cellular, and fibrous—being less amenable to the destructive influence of cancer, are often hypertrophied or thickened by its first impression. We frequently find the two effects simultaneous in the same organ; the glandular element wasting, becoming fatty, or replaced by cancer cells, while the fibrous element is increased to many times its natural thickness. You will not forget that the latter also does, after a time, yield to the destructive influences of the disease, being impregnated with cancer-cells, or infected with the general tendency to decay and ulceration.

When you cut open a cancerous tubercule, you will often find that, at or near the middle, it is softened or converted into a more or less diffluent pulp; and if such a part be examined more closely it is seen to have lost all regular structure and to be composed of fragments which appear to be the disorganized remnants of cells and tissue. The softening depends, probably, upon a failure of the nutritive powers taking place to such an extent that the component atoms are no longer held in the structural relations into which they had been thrown, and in which they had been maintained by the vital and nutritive forces of the growth. It commences naturally at the part first formed, viz., the centre of the tubercule, and is similar to the softening that takesplace occasionally in simple tumours, or more frequently in scrofulous deposits and tubercules, and is similar also to the softening and disintegration of the natural structures often induced by inflammation. Though occuring in cancers, for the most part as a regular or natural process, it may be induced by any cause which prematurely impairs their vital energies, such as an attack of inflammation or a blow. Strange and paradozical it seems that these same products should possess such irresistable assimilative influence over the surrounding tissues, and should be so unable to maintain their own existence, so liable to de, cay and dissolution.

The softening may commence at one or more points and sometimes leads to the formation of several small cavities in various parts of the mass, containing a turbid fluid which often looks like pus and may contain pus corpuscles. Indeed the process frequently resembles and is associated with suppuration; the smaller cavities coalescing into a large one which approaches the surface like an abcess, bursts through the skin, the softening commences on the superficial side, and takes place earlier than when it is deep-seated. This breaking up of the morbid mass is usually attended by some inflamation of the surrounding tissues with effusion of lymph uniting them together and rendering them a more easy prey to the ravages of the disease. At the same time the adjacent lympatic glands are commonly observed to become inflamed, and to participate in the malady, if they have not done so. It is by no means uncommon for inflammation in the surrounding tissues to precede the softening of the cancer; indeed it sometimes is contemporaneous with the earliest appearance of the disease, and attends it through all its progress so assiduously that some pathologists have regarded cancer to be only one of the multiform results of inflamation. The cavity or ulcer commencing in the manner just described continues to increase by the progressive softening and disintegration of the neighbouring cancerous tissue, so that particle after particle is seperated and forms a component in the discharge; the disease still advancing in the circumference while the dissolution of the mass

is thus going on at the centre. In this way huge caverns are sometimes formed and large parts of the body destroyed. Sometimes the work of demolition proceeds at a greater rate mortification succeeds to ulceration, and considerable masses are detached. Now and then, in consequence perhaps of a slight inflamatory attack, the mass appears to slough away and leave a clean surface behind; but the hopes of a cure thus excited are almost always doomed to be disappointed by the reappearance of the disease in the side of the chasm from which the slough has seperated.—*Provincial Medical and Surgical Jounal, Feb.* 19, 1851, *p* 85.

ASHTMA.

By Dr. R. B. Todd, F.R.S.

What is Asthma?—I shall answer this question by stating first that it cannot be properly called a disease of the lungs—in other words its primary seat is not in the lungs: it may be defined to be a constitutional disease, which manifests itself by paroxysms of difficult breathing, with intervals of various duration, in which the patient is completely or nearly in a healthy condition.

Let us look at the more prominent points in the clinical history of asthma, and inquire how far we may found upon them an explanation of its pathology.

The way in which the uncomplicated asthmatic attack commences is generally this. A patient, we will say, goes to bed quite well; soon after he finds a difficulty of breathing come on; he cannot lie down; he cannot go to sleep; the dyspnœa increases, and the attack becomes confirmed: or, what is very often the case, he goes to sleep quite well; and wakes in an hour or two with the attack on him. When once formed, the asthmatic paroxysm continues for some time, and passes off generally with some cough and expectoration but sometimes without either. The time of its duration is very variable: sometimes it lasts only a few hours, sometimes many days.

When suffering from the attack, the patient cannot lie down. All asthmatics show an instinctive repugnance to the horizontal posture while the attack is on, and even in the intervals of the attacks they like to lie high, and seldom lie quite flat: generally they are content with sitting up in bed or on a chair, or they may sometimes move about the room. The coachman of a neighbour of mine some years ago suffered so much from a paroxysm of asthma, which resisted all treatment, that he was obliged to stand leaning forward on a table for three days; and in this erect posture he passed the whole of this time: at last he became so exhausted that he was obliged to sit down from sheer inability to support himself. The erect or semi-erect posture is no doubt preferred because it enables the muscles of respiration to act with greater freedom, and with more mechanical power.

These attacks are very much influenced by weather, particularly cold and damp, and by locality, as high or low, humid or dry, relaxing or bracing: and there is a remarkable capriciousness in these respects, some persons liking a low, damp, smoky situation, some a high, dry, and clear; that which is fatal to one case will be the very best thing to another, and one person will be well where another cannot live. Sometimes persons living in London, and wishing to try the neighbourhood, will ask you, "Shall I go to Clapham, or shall I go to Highgate—will a high or a low situation be the best for me?" Now this is a question that it is impossible to answer with certainty: for very often that

which would seem the worst will turn out the best. I have known asthmatics better on the banks of the Thames than anywhere else; on the other hand I have known some greatly relieved by going to a high situation. Dr. Watson has some interesting remarks on this subject in his valuable lecture on asthma, and relates some curious cases in illustration of the uncertain influence of locality in checking or promoting the asthmatic paroxysm.

Then, sometimes we find that asthma is brought on by certain specific irritating agents. We all know of *hay-asthma*: that if certain individuals venture within the range of a hay field, they are seized with sneezing; coryza, profuse lacrymation, and other symptoms of irritation of the mucous membrane, accompanied with a distinct asthmatic paroxysm. Similar symptoms are brought on in other people (but such persons are much fewer) by the volatile effluvium of ipecacuan, or by the presence of very fine particles of dust floating in the atmosphere.

In asthma the respiratory efforts are greatly exaggerated, in consequence of the sensation of the want of breath—the *besoin de respirer* of the French; a sensation which any one may easily experience in his own person by simply putting his face into water for a few seconds. Under the influence of this a strong feeling of the want of breath, large quantities of air are drawn in, and so great is the effort of inspiration that the noise which it occasions may be heard for a considerable distance. What results? The air-cells of the lungs become dilated; and the whole lung experiences a proportionate enlargement, and the external configuration of the chest is altered.

The state of lung which is produced by asthma, is that which has been called by Laennec *emphysema*—a name not happily chosen, as it implies what really does not exist, namely the infiltration of the lung with air, the existance of the air in extravesicular tissue. If the asthmatic attacks continue for any great length of time, and are severe, we find further injury of the lungs taking place; the walls of the air-cells suffer in their nutrition, and some of them, already dilated to their utmost extent, give way; three or four, or more, become fused into one, and form large irregular cavities, which are distributed among the healthy pulmonary tissue.

But, besides these changes in the lungs, the repetition of the asthmatic paroxysms leads with equal certainty to morbid changes in the heart. From the obstruction that is offered to the circulation through the lungs an undue amount of work is thrown on the right ventricle, which of course becomes more or less hypertrophied; at the same time the meshes of the pulmonary capillaries become enlarged, and no doubt experience some change in their vital properties whereby the circulation in them is retarded. This affords an additional means of obstruction through the lungs, and therefore an additional cause for hypertrophy of the right side of the heart, but as the backward pressure of the blood on the heart becomes increased, the right ventricle becomes not only hypertrophied, but dilated, and the dilation extends in a retrograde course to the auricle, and thence to the large veins, as that in the advanced stages of this disease it is not uncommon to find venous regurgitation, and more or less congestion throughout the whole system. In the early stages, however, none of these conditions exist. You may have the most exquisite asthmatic dyspnœa without its leaving any perceptable deviation from the healthy standard either in the heart or lungs, or at most no more than may perfectly recover itself when the paroxysm passes off. This is more likely to occur in children, because their tissues have a greater power of recovery from their greater activity of nutrition.

Such cases as these would alone be sufficient to prove that asthma is a disease essentially independent of any organic lesions of heart or lungs, though frequently accompanied by them, aggravated by them when they exist, and always inducing them if it is of sufficiently long continuance. And it is remarkable how soon these lesions may be thus induced, how short a continuance of asthma will be sufficient to give rise to evident signs of organic change in the heart and lungs.

[In this case, though the patient only had asthma three month, and for the greater part of the time not severely, yet he is already found with a barrel-shaped and unusually resonant chest, dilated thoracic parietes, and the heart so displaced and dilated in its right cavities as to beat in the region of the scrobiculus cordis. Dr. Todd continues:]

I look upon this last sign as one of the most characteristic symptons of asthma, and I consider its presence in any case where I suspect asthma as a clear configuration of the correctness of those suspicions. In accordance with this view, in examining a patient whom I suspect to be asthmatic, one of my first steps is to apply my finger to his scrobulus cordis; if I find no beating of the heart there, my conclusion is a contingent negative; but if i find it beating there and not in its natural position under the nipple, my conclusion is a certain affirmative.

If you trace up the disease to the point of its first appearance, you will generally find that the first attack came on either without any assignable cause, or after some indiscretion as to diet—or after some imprudent exposure to weather; the patient went to bed well, and in every respect in his usual health, and woke up asthmatic—but once having made its appearance, it renders its victim ever after liable to its recurrence.

A remarkable circumstance is, that it is often inherited; the father or mother have had it, or it may have lept over a generation,—the grandfather may have been asthmatic, and the intervening generation not so. Once that the asthma has fairly established itself in any individual, it may be brought on by any slight cause, even the most trivial disturbance will be sufficient to excite it,—catarrh, indigestion, irregular hours, mental excitement, violent exercise, change of temperature, change of place—any one of these may bring on an attack. Now, if you inquire narrowly, you will generally find at the root of the disease some fault in the primary assimulation: you will find that the patient has learned to avoid certain things; that he is not at liberty like other men; there are some things of which he dare not partake, or, if he does, it is done at the price of an attack. And you will likewise find, on looking into the patients secretions that they are altered, that the urine is lithic, phosphatic, (most frequently the former), or presents some deviation from the healthy standard. Very often too, you will find the attack ushered in by a peculiar condition of the urine, either such as I have just mentioned, or urine resembling nervous hysterical urine, abundant, clear, and pale, and of very low specific gravity.

Now all these points,—the periodical recurrence of the attacks, the perfect or nearly perfect health in the interval, the absence of any organic change, the associated humoral disturbances,—all bear in the most interesting way on the pathology of this malady. They tend to establish a remarkable analogy between asthma, gout, and some other diseases. As in asthma, gout comes on quite suddenly—there is no warning: a man may go to bed, quite or nearly well, and he will wake up early in the morning with a fit of gout on his great toe. There is another disease, epilepsy, in which we have exactly the same

phenomenon; a patient with or without warning, falls down foaming, livid and convulsed; the paroxysm goes off, and leaves him in his ordinary good health and he may go on for years and not have another. Again we know that a fit of the gout leaves no organic leison if it occurs once or twice: but if it is often repeated, it leaves permanent injury in the joints that it attacks. We may observe the same with respstt to epilepsy. If a patient has suffered only one or two attacks, you will find no change in his brain; but if he has had several you will. The disease evidently consists essentially of something attached to the brain, and not existing in it. The same too of asthma, the organic changes are all secondary, and a few attacks leave no trace behind them.

All this leads us to suppose that the paroxysm of asthma has something in common with the paroxysm of gout and the paroxysm of epilepsy. Of the two, I prefer to take the analogy to gout, because we have more definite and coherent ideas about gout, and we are more acquainted with its exact pathology. The theory at present most in favour with regard to gout is that it is a disease of assimilation, and this defective or vitiated assimilation gives rise to some materia morbia. When this matter is eliminated from the system, the attack passes off; when it accumulates, the attack comes on. In asthma, defective assimilative power is a frequent coincident. Gout, too, and rheumatism, and all humoral diseases resemble asthma in being inherited.

When the materies morbi of asthma has been generated, its effect is to irritate the nervous system, not generally but certain parts of it, those parts being the nerves concerned in the function of respiration—viz., the pneumogastrick, and the nerves that supply the expiratory muscles, either at their peripheral extremities, or that of their central termination in the medulla oblongata and spinal cord; extreme difficulty of breathing is the result, and as a consequence of this, ultimate disease of the lungs.

Thus the occurrence of asthma in paroxysms would be accounted for much in the same way you would explain the occurrence of gout in paroxysms; and in the intervals between the atracks, the patient being of asthmatic constitution, so to speak, is easily thrown into the paroxysm of asthma by causes which would but slightly influence other men—as cold, impure air, mechanical irritation of the respiratary passages,—just as gouty men may readily be thrown into the gouty paroxysm by causes comparatively trivial.

This seems to me to be the most reasonable exposition of the pathology of asthma.

Now we often hear physicians of great and deserved repute speaking of *spasmodic* asthma. I need not tell you that the bronchi possesses a mascular coat, consisting of the circular fibres of the unstriped, for it has long been proved not only by microscopical observation, but by the most satisfactory experiments. It is in these circular mascular fibres of the bronchi that many pathologists localize the spasm, to which they ascribe all the phenomena of asthma.

The first link in the chain of efforts of the immediate exciting cause of asthma would be, according to them, spasm of the bronchial tubes, then dyspœa. Undoubtedly a state of spasm of the bronchial tubes would produce a great deal of dyspœna; but what I want to point out to you, is, that this state of spasm of the baronchial tubes ought rather to be regarded as one of the accompaniments, one of the phenomina of asthma, than its cause. The feeling of breathlessness, or, in other words, a peculiar state of certain nerves and of a certain nervous centre, the centre of respiration, is the first link in the chain of

asthmatic phenomena. The spasm of the bronchi follows sooner or later upon this and often it follows so quickly upon it as to appear to come simultaneously with it: does it ever precede it? I doubt this.

Undoubtedly you may have severe asthma without severe spasm of the bronchial tubes. I remember a well marked instance of this in a gentleman whom I attended for chronic disease, cancer as I thought, of the liver. For nearly a week beforehis death he suffered from the most frightfully distressing asthma, which nothing could control, and which lasted without interruption till he died I examined his chest repeatedly at all parts, and could hear nothing but the most perfect, loud and peuriie breathing, which is quite inconsistent with a state of spasm.

Again: section of the vagi nervea of animals produces phenomena exactly like those of asthma. Whatever be the cause of the dyspnœa in these cases, it is clear it cannot be bronchial spasm, as the muscles of the bronchi would be paralysed after section of their nerves.

There is one thing that I have obsrved, which has an important bearing on this subject; that in that particular form of spasmodic disease which I have no doubt in your future practice you will have many opportunities of witnessing laryngismus stridulus, the crowning inspiration of children, there is a ronchus all over the chest, simultanious with the convulsive attack. The moment the convulsion comes on, when the eyes become fixed, and the child begins to inspire with difficulty, if you put your ear to the chest you will hear a ronchus pervading the whole lung. The moment the recovery takes place, the ronchus ceases. And it is very interesting to notice that you may observe the very same thing in the ordinary covulsion of children, in which the larnyx is not prominently engaged; and I suppose no one would pretend to locate these diseases anywhere bnt in the nervous system.

Hence the conclusion that I draw is this, that the spasm is the accompaniment, and not the cause of the difficult breathing that accompanies or follows upon the nervous changes, just as it does in laryingismus and in the ordinary convulsions of infants.

So much for the pathology of asthma; as to the treatment of this disease our attention must be directed,—1. To obtain relief to the paroxysms, and,—2. To improve the patient's constitution in the intervals of the paroxysm.

First, in the asthmatic paroxysm you must inquire into the immediate exciting cause, and endeavour to remove it. With this view an emetic, by emptying the stomach, will be often found useful; or if the quality, not the quantity of the contents of the alimentary canal is the source of the evil, an alkali will be found beneficial. Stimulants are sometimes of essential service, especially when the long-continued circulation of imperfectly arterialised blood has deadened the sensibilities, and so for lowered the powers as to render the expectoration of mucus difficult; one of the best stimulants is sulphuric ether, or perhaps the chloric is even better, from its tasteand smell being so agreeable. Ether, in combination with opium, will often be found of great service. But perhaps *sedatives* constitute the class of remedies that has met most favour in the treatment of asthma, and epecially the different members of the order Solanaceæ,—as hyoscyamus, belladonna, tobacco; but before and above all, both in the frequency of its employment and its real value, stramonium. This medicine may be given in various forms,—as an extract of the seeds, in doses of from gr. ¼ to gr. iij., or in the form of a tincture, in from ten minims to half-drachm doses; but more frequently it is smoked, and for this purpose the whole plant,

leaves and stems, are used; it is dried and cut into small pieces, and smoked in a pipe, or the leaves alone are dried, and rolled up in the form of a cigar.

I here show you some of these cigars, which have been put into my hands by Mr. Savory, of Bond Street; but I must tell you that these are not, strictly speaking, *stramonium cigars*; being made, not of the Datura Stramonium, but of the kindred species, the Datura Tatula, which is said to be even more efficacious than the more generally used species.

Lastly, we find a valuable remedy for the asthmatic paroxysm in cloroform we know how in other cases it influences all those functions which are ministered to by nervous influence, and in its efficacy in asthma we have an interesting physiological experiment, and an important lesson with regard to the true pathology of the disease. But in the administration of chloroform I would give you this two-fold caution:—first to give it gradually and cautiously, and not in a full dose; not to produce insensibility, especially if there be anything like blueness on the surface, because, though remedial to the asthma, it will tend to increase those very consequences which are most to be feared from the circulation of veinous blood. Secondly, to impress upon your patient that he must never give it to himself, nor without the presence of a medical man. This case was related in the papers the other day:—A person who was in the habit of curing his attacks of asthma by inhaling chloroform, when administering it to himself one day, and when in a state of half subjection to its influence to produce the full effect placed his handkerchief on the table, and buried his mouth in it; his insencibility became deeper and deeper, till at last he was too far gone to be able to raise his head. He therefore continued inspiring it, his coma became more and more profound, and a short time after he was found in that position quite dead.

Secondly, the treatment of the intervals between the paroxysms should be directed to improve the digestive powers of the patient and the tone of his nervous system; the diet should be very carefully regulated both as to nature and quantity; and this is of all things the most important to occupy the attention of the medical attendant. The alimentary canal and the secretions geneally should be kept in a healthy condition; exercise, the hours of rest, and in fact every thing that bears on the general health, should be systematically and rigidly superintended; and I am sure that treatment of this sort will always be found of substantial advantage. Another thing having the same object is cold or tepid effusion, either by sponging or by shower-bath; but cold, while very servicable to those who can bear it, is often too much for many people, in whom no reaction take place, the coldness continues and drowsiness is produced. Such symptoms should always be looked upon as contra-indications to the use of cold affusion.

Now before I conclude, I will say a word on the relation of emphysema and asthma. Are these two diseases related to one another as cause and effect? and, if so, which is the cause, and which the consequence? To determine this let us see what emphysema is, It is a state in which the lungs are rendered more capacious, in which the relation of the ultimate element of the lungs are in some measure deranged, and the pulmonary capillaries altered. Now is this state of increased capacity a state capable of producing such a dyspnœa as we see in asthma? I cannot think that it is, although this increased capacity is attended with other conditions tending to diminish the efficient aeration of the blood, and, therefore, to the production of dyspnœa. But it will not explain the periodicity of the attack: emphysema, is constant asthma paroxysmal. On

the other hand, asthma is clearly an efficient cause of emphysema; the asthmatic condition is just such as produce those physical changes that constitute emphysema. It is confirmatory of this view, that emphysema comes on gradually, and that it bears proportion to the length of duration and severity of the asthma: if a person has had a few attacks of asthma he has no emphysema; if he has had many, he has; the asthma precedes, the emphysema follows.

This then is the conclusion I come to,—that asthma is primarily humoral; that it is caused by a poison or morbid matter acting on that portion of the nervous system which ministers to the function of respiration; that it leads to dilitation of the lungs and the walls of the chest, to emphysema, and ultimately being no longer created, the patient ceases to be asthmatic, just as a person ceases to be gouty or epileptic; and that, ceasing to be asthmatic the patient may remain, or may not remain emphysematous, according to the severity and duratton of his previous attacks.—*Medical Gazette, December* 13, 1850, *p.* 999.

GENERAL REMARKS ON THE DIAGNOSIS OF TUMOURS OF THE NECK.

By Dr. P. Redfern.

This always presents great difficulties when the tumours are of large size, only slightly painful, and fluctuate indistinctly. If a complete and accurate history of the case cannot be obtained, more than a general diagnosis may be impossible; but when the precise position of the tumour at its commencement can be made out, when its relations to the larynx and trachea, and its mobility in regard to them and the surrounding parts when it was of small size, can be ascertained, together with the rapidity and manner of its growth, there will be little difficulty in arriving at a satisfactory conclusion.

A *tumour* developed in the substance of *the thyroid body* presents itself in the front of the neck, is usually larger on one side than the other, is firmly connected with the larynx and trachea, moves freely with the larynx in deglutition, and when it is displaced laterally by manipulation. The other features vary with the nature of the tumour.

In ordinary bronchocele (hypertrophy) the swelling is soft projecting, elastic; without fluctuation, pain or tenderness on pressure; it occurs usually in early life, in the female sex, and in particular districts of country; it is simple in its nature throughout, and presents no tendancy to degeneration or change of structure; it in no way interferes with respiration deglutition, nor does it affect the patient's health or comfort until it becomes of very large size, when difficulty of respiration and deglutition, with frequent headaches, occasion the greatest distress and may end with the death of the sufferer.

In *cystic disease of the thyroid* the nature of the tumour becomes manifest, sooner or later by the presence of fluctuation in one or more cysts, by a glairy, serous, or sero-sanguineous fluin escapeing rapidly along a grooved needle when introduced, the fluid containing no cellular formations when examined microscopically, or having such a structure as is inconsistant with the idea of the existenee of cancer,—by the formation of the tumour taking plaee at or after the middle of life,—by its slow and painless growth, and by the slight inconvenience it occasions as long as its size is not very great.

In Cancerous disease of the thyroid (usually scyrrhus) the patient being between forty-five and sixty-five years of age, is of great and uniform density and generally painful; it is developed rapidly, and may attain a large size in the course of a few months; it accompanies the larynx in its movements, shortly limits their extent by attaching the organ to the surrounding parts; it occasions great difficulty of deglutition and respiration from an early period to hoarseness, cough, and spasmodic action of the muscles of the larynx, and pains come on and increase in their intensity,—the distress and anxiety of the patient, his sallow complexion and emaciation, marking him out as the subject of a steadily advancing and destructive malady.

In *medullary cancer of the thyroid* the surface of the tumour may be even and tense, or indistinct fluctuation may be perceived, the other characters depending on the steady infiltration of the surrounding textures, distinguishes the disease from other tumours of the same part.

Enchondromatous tumours are to be recognized by their great density, the slowness of their growth, and the absence of any signs of the extension of the affection to the surrounding parts, and of general evidence of the existence of malignant disease.

The diagnosis of *tumours of the neck, not connected with the thyroid body*, is to be established by reference to the general characters which distuinguish them in other situations, every particular of their history and mode of growth having been carefully ascertained as essential points, and sufficient care being exercised in the presence of a quantity of coagulable fluid, in the interior of a cancerous tumour, lead to the belief that it is of a cystic character.

I shall complete this paper by a short consideration of the important question,— *Whether or not fibre is an essential element in the structure of cancer?*

Much difference of opinion still exists as to what are the parts of a cancerous growth which are essential to it. Professor Bennett states that fibres, cells, and a viscous fluid, are the three essential elements of a cancerous formation, Labert regards the cancer-cell as the only distinctive, constant, and essential element, the predominance of one or other of the accidental and secondary elements determining the varieties of form and appearance; yet he regards the fibres as next in point of importance and frequency, and speaks of them in encephaloma as pale, fine, and in a small quantity. Muller says that the fibres of encephaloma are indistinct, and that the fusiform cells are arrested in their development into fibres; whilst Vogel states, that in encephaloma fibrous structures are wholly absent. As has been before remarked, it is excessively difficult to state whether the fibres which are found in many tumours are really cancerous, or whether they belong to the proper structure of the organ in which the tumour has been developed; and consequently, careful examinations of cancerous formations in organs which contain no fibrous tissue in their healthy state, become of extreme importance in leading to a true determination of the mode of developement and actual position of the fibrous element in cancer.

From the examination of cancer of the brain, and of numerous cases of encephaloma, I am led to believe that fibres are by no means invariably to be found in such growths, and that their fibrous element is accessary and non-essential. If this be so, the existence of fibrous tissue in most cancerous structures remains to be accounted for in either of two ways,—viz., by hypertrophy of the normal fibrous tissue of the part, or by a new development of fibre from

the recently-diffused blastema,—a development commenced and completed under the influence of the determining energy exercised by the fibrous tissue of the part itself, agreeably to the law of analogous formations. Presuming that the fibrous element of cancer is developed in either of the methods just indicated, the absence in cancer of the brain is readily accounted for, as in that organ there is no fibrous tissue to acquire an unusual development, or to determine the formation of new tissue of its own kind from cells. *If* proper *cancer cells*, of a fusiform shape, *ever become transformed into fibres*, their presence in cancer of the brain, unaccompanied by the fibrous element, may be owing to a deficiency of the stimulus necessary to ensure such development, and may possibly be dependent on the absence of fibres in the original and healthy texture of the organ.—*Monthly Journal of Med. Science, Dec.* 1850, *p.* 523.—(General remarks on the Diagnosis of Tumours of the neck. Continued from the last number.)

ON THE SPECIFIC GRAVITY OF CEREBRAL SUBSTANCE, AND ON ATROPHY OF THE BRAIN.

By John Charles Bucknill, M. D. London.,
Physician to the Devon County Lunatic Asylum.

Hitherto, in pathological description of the cerebral substance, the terms softening and induration have been used in the most loose and uncertain manner. When actual ramollissement proceeding to liquefaction occurs, there can be no doubt about the matter, and the senses of touch and sight require no adjuvants. These decided changes are found when limited portions only of the organ are affected: but when these pathological changes implicate the whole of the brain, death takes place before they have proceeded so far as to leave very sensible and appreciable alterations of structure.

M. Guislain, in his recent work on "Les Phrenopathies," remarks on this subject—"In taking our senses for guides we are liable to deceive ourselves. That which we call ramollissement is only a pathological state arrived at its 'summum' of disorganization; but does not this disorganization already exist in the intimate structure of the primitive fibres before having attained that visible point of softness which constitutes visible ramollissement?"

The brain of a patient who has died of delirium tremens, of the delirium of fever, or of some forms of insanity, is not obviously different from that of a man who has been cut off in the midst of health by some sudden accident; yet we are unavoidably impelled by our casualty to refer the death in the former instances to some change in the brain, which the perfection of our senses prevents us from observing. Perhaps this change is in great measure chemical; and observations now pending, may succeed in fixing the cause of several morbid conditions of the nervous system on the quantity and state of combination of the phosphorus contained in it.

Perhaps this change is for the most part molecular. The functions of the brain may only be perfected when a certain definite arrangement exists but were its vessels, cells, and tubules, favourable to the regulated passage of the contents of the one into the other, and to the developement and interchange of electrical affinities. This arrangement may be disarranged without addition to or abstraction from, the material of the organ, as in the sudden loss of all function

from concussion. Frequent concussion alters the molecular arrangements in the axles of locomotive engines producing brittleness fraught with the most perilous consequences, but giving no external sign that the tough metal has lost its temper. So a blow on the head will kill a man without leaving any change as yet discoverable either by the chemist or microscopist; perhaps this mystery will hereafter be unveiled to the latter.

I do not believe that the brain is nothing more than a galvanic battery "perfectionated," but the two have so many points in common, that for a long time past a legitimate analogy has been drawn between them. Pursuing this analogy, we find that the working of a galvanatic battery may be impaired by the liquid in the cells becoming neutralized, by the disarrangement of the plates, or by interposition of any substance having feeble powers of conduction; on the other hand, the functions of the brain may be impaired or interrupted by an alteration of the nutritive fluid; by a sudden shock which may disarrange the vesicles and tubules; or, thirdly, by the interposition of inert material between its active molecules. It is to this last condition in particular that I am anxious to direct attention.

The interposing material may be albuminous, or fatty, or serous. An albuminous deposit pervading the brain appears to be the condition known as hypertrophy of that organ. This condition is rare. Out of 240 autopsies of insane patients, I have only met with one.

The depositions of fatty material in the brain is a subject of deep importance and interest. In examining circumscribed softenings under the microscope, I have almost invariably found a great increase of fat-globules; but as yet I have been unable to satisfy myself whether, in any changes pervading the whole organ, the fatty material is increased.

The deposition of serous fluid throughout the substance of the organ, is a frequent condition, and is I think, constant in cases of general paralysis, dementia, and all forms of chronic mental disease accompanied by loss of power. Those cases, however, which are occasioned and accompanied by epilepsy, form an exception.

I have long felt the want of some trustworthy measure of the relative amount of the solid and fluid constituents of the brain. The best morbid anatomists have been in the habit of describing the brain as denser, or softer, or more watery than natural, with about as much accuracy as we say the day is cold, when we refer to our own sensations, and not to the indications of the thermometer. We know that these latter sensations often differ in different individuals, to the amount of a great-coat or two; and we may expect that the unassisted senses will not always be very accurate in the former case. If, when examining a diseased brain we could always have a healthy brain before us, as a standard of comparison, we might by the resistance opposed to the knife or to the finger, form a fair approximation to the truth; but facilities for this comparative examination are not often attainable.

For several years past I have sought to meet in some degree the difficulty, by endeavours to ascertain the specific gravity of healthy and diseased brain; and in my annual report, which I presented this time last year, I gave a tabular statement of thirty-two cases in which the specific gravity of the cerebrum and cerebellum, the weight of the brain, and the capacity of the cranial cavity, had been accurately ascertained. I append hereunto a similar table of thirty other cases which I have examined during the current year.

No. in the Admission Book.	Age at Death.	Sex	Form the Mental Disease when Admitted.	Apparent cause of Death.	Capacity of Cranial Cavity Water 60° F.	Weight of Brain.	Specific Gravity of Cerebrum.	Specific Gravity of Cerebellum.
					oz. (Apoth)	Oz. (Av.)		
933	72	F.	Dementia	Old age and decay	43	34½	1015	1042
628	47	F.	Imbecility	Phthisis	51½	45½	1041	1045
666	48	F.	Melancholia	Chronic gastritis; stricture of phyloric orifice	54	46½	1041	1042
382	78	F.	Dementia; shaking palsey.	Peritonitis	51	39½	1040	1043
722	39	M.	General paralysis	Gen.l. paralysis final symptoms, convulsion and coma	50⅞	44	1040	1045
688	75	F.	Chronic mania	Old age and decay	42½	32	1040	1045
452	53	F.	Melancholia	Phthisis	49	48	1044	1044
794		F.	Imbecility (epileptic)	Epilepsy	42¾	40	1044	1046
45	52	F.	Imbecility (congenital)	Phthisis	48½	47	1040	1042
911	29	M.	General paralysis	Gen. paralysis; final symptoms, convulsions and coma	55	46	1040	1043
914	78	M.	Mania (chronic)	Old age and gradual decay	48	41½	1040	1045
704	43	M.	Idiocy	Cerebral congestion, consequent on erysipelas	47½	47½	1046	1046
954	31	M.	General paralysis	Gen. paralysis; final symptoms, convulsions and coma.	53½	44¼	1040	1041
977	37	F.	Melancholia	Phthisis	47⅜	45	1040	1045
271	66	M.	Idiocy (deaf and dumb)	Fatty degeneration of heart and liver	52½	48	1042	1045
975	56	M.	Melancholia	Chronic gastritis	57½	50¾	1040	1040
288	27	M.	Idiocy	Gangrena oris	51½	52	1040	1045
1034	53	F.	Melancholia (acute)	Chronic gastritis	50⅜	46	1045	1046
1032	50	M.	Dementia	Phthisis	41	40¾	1042	1042
885	62	F.	General paralysis	General paralysis; final symptoms, asthenic	45½	37½	1039	1039
265	69	F.	Mania (chronic)	Disease of heart and lungs and curvate of the spine.	44⅜	42½	1043	1046
447	33	F.	Monomania	Phthisis	42⅜	42¾	1040	1042
1038	66	M.	Dementia	Pleurisy	46	43	1040	1042
966	62	M.	Mania (chronic)	Apoplexy	53	50½	1043	1045
930	58	F.	Dementia (epilepsy)	Disease of the heart	44½	45	1037	1041
770	57	F.	Dementia	Phithisis, and disease of the heart	40¾	40	1037	1040
871	59	M.	General paralysis	General paralysis; final symptoms, asthenic	54	43	1036	1042
789	24	M.	Edilepsy	Epilepsy	48	47¾	1040	1040
792	42	M.	Dementia, with hemiplegia.	Gradual exhaustation	49	42	1040	1040
1004	45	F.	Melancholia	Softening of the brain (circumscribed)	39¾	43	1041	1042

In commencing these investigations, the unfitness of the hydrostatic balance for the rough purposes of the post-mortem room, led me to think of and adopt the simple expedient thus described in my report: "The specific gravity of the cerebrum and cerebellum is ascertained by immersing a portion of each in a jar of water wherein a sufficient quantity of sulphate of magnesia has been dissolved to raise the density of the fluid to the point required, adding water or a strong solution of the salt until the cerebral mass hangs suspended in the fluid, without any tendency to float or sink; and then, by testing with the hydrometer, the specific gravity is thus found with great delicacy and facility, a difference of half a degree in the density of the fluid being indicated by the rise or fall of the substance immersed. The soluble salt is chosen for its possessing no stringent or condensing action upon the tissues. In these eight cases (of general paralysis) the average specific gravity of the cerebrum was 1039½; the highest, 1042; the lowest, 1038. The avarage of the cerebellum was 1042; the highest, 1045; the lowest, 1037. This is below the specific gravity of healthy cerebral substance, which may be taken at 1046. The only notice I find of the specific gravity of the brain is in the chemical analysis of M. John, quoted in Mr. Solly's work, and is stated at 1048. Upon these grounds I submit my right to assume, that in general paralysis the density of the brain is diminished."

It is not improbable that this new method of investigating the condition of the cerebrum may lead to important results. Some parts of the organ have a greater density than others; the figures, however, refer to large pieces of brain containing a fair proportion of vesicular and tubular substance. The cerebellum has generally a higher specific gravity than the cerebrum. In only two out of sixty-two cases has it been lower. In many instances I took the specific gravity of the whole organ, but finding that it was impossible to free so large a mass from air bubbles, I discarded the result as untrustworthy.

A low specific gravity does not necessarily indicate a diminution of cohesion or the commencement of ramollissement, although it points in that direction. A brain might acquire a low specific gravity from an increased quantity of fat-globules in its tissues, while retaining its normal consistance. I believe however that fat tends to accumulate only in softening brain, so that possibly this course of error may not exist; but it is nevertheless a point of the utmost importance to determine how much of the diminished specific gravity in brain-tissue is to be attributed to the effusion of serum, and how much to the accumulation of fatty matter. This question may be resolved by treating the substance with ether, and by evaporation. I am convinced that in circumscribed softening of the brain (true ramollissement) the low specific gravity is to a great extent owing to the amount of fatty matter deposited. In the last case on the preceding table, the specific gravity of the cerebrum generally was 1041, while that of the softened parts was 1035; and, on examination, this pultaceous substance was found pervaded with an immense quantity of fatty matter. The pursuit of these sources of fallacy will open up a new ground for investigation.

Is the serum effused into softening brain imbibed into the vessels, or does it remain interstitial? M. Guislain, states that "these cells of the fundamental tissues of the grey substance present themselves *ten times larger* than their normal state. In ramollissement the serum escaped from the vessels penetrates the interior of these cellules and provokes their distension. It is a true imbibition." After diligent search with a first-rate instrument, I have been unable to observe

these immense vesicles; and I think that the loss of cohesion in the brain substance would also indicate that for the most part the effused serum remains interstitial.

I have found that in some diseases which occasion a loss of specific gravity of the brain that organ also suffers actual loss of bulk—an actual as distinguished for an interstitial atrophy. The additional fluid which makes the brain light probably goes to make up for interstitial atrophy, but it does not wholly make up for it, and the brain shrinks from its bone-case. This fact is pretty evident of serous effusion into the meshes of the pia mater but I prove it more satisfactorily by pluging the foramina with clay, re-adjusting and luting on the calvarium, with the dura mater attached, and ascertaining the precise quantity of water at 60° Fahr. which the empty cranium thus prepared will contain, and by comparing this measurement with the weight of the brain.

In the diseases accompanied by low specific gravity, the absolute weight of the brain, as compared with the capacity of the cranium, is diminished to a greater degree than can be accounted for by loss of specific weight; and on the other hand, in epilepsy, apoplexy, and cerebritis, the weight of the brain, as thus compared with capacity of the cranium, exceeds the standard of health.

Professor Sharpey has kindly pointed out to me, that to perfect these comparisons, it is desirable to ascertain the quantity of water which the brain will displace; this compared with quantity which the cranium will hold, will show the amount of actual atrophy. This plan I now adopt.

I have in this place restricted myself to observations on the brain, reserving to a future occasion some remarks on the specific gravity of other organs. The simple and easily applied hydrostatic test I have described is extremely useful in pointing out the early stages of fatty degeneration of the heart liver and kidney; and with a handful of Epsom or Glauber salts, or even of sugar, and an hydrometer, the morbid anatomist need never be at a loss to decide whether or not this interesting change has taken place in any of these organs.

The cases in the preceding table were all of chronic character; the specific gravity of the cerebrum ranged from 1036 to 1046. The table I published last year contained a few acute cases, and the specific gravity of the cerebrum ranged from 1036 to 1052.

In the present table the average specific gravity of the cerebrum in 1040·9; that of the cerebellum, 1043. In three cases of general paralysis, the closing symptoms being convulsions and coma, the specific gravity of the cerebrum was 1040. In two other cases of the same disease, the closing symptoms being gradual failure of the powers of life the specific gravity was 1036 and 1039. Similar facts in epileptic cases would appear to indicate that the specific gravity of the brain is higher when life has terminated in coma or asphyxia than when it has ended in syncope or asthenia. No. 930, an epileptic patient, died suddenly of syncope from disease of the mitral valves, and the specific gravity of the cerebrum was only 1037. In other cases of epilepsy, with final symptoms compounded of asphyxia and coma, the specific gravity has never been below 1040, and has reached 1049.

In the preceding thirty cases, the average capacity of the cranial cavity for water at 60° Fahr. was 48·2 fluid ounces (apoth,); the average weight of the brain was 43·8 ounces (avoirds.)

These investigations are as yet too young to fructify into trustworthy

deductions; but I think they will establish the existance of two kinds of cerebral atrophy—namely, positive atrophy, and interstitial or relative atrophy, which may or may not be co-existent. By positive atrophy I wish to indicate an actual shrinking of the brain, and by relative atrophy an interstitial change, wherein the active cerebral molecules suffer diminution, and inert materials are deposited. It will be well to restrict the term ramollissement to the circumscribed and decided softening to which it was in the first instance applied.
—*London Lancet Feb.* 1853.

PRACTICAL REMARKS ON THE DISEASES OF THE EYE.

By Dr. James Dixon, Esq. F. R. C. S.,
Surgeon to the London Ophthalmic Hospital.

On Sub-Conjunctival Dislocation of the Lens.

Published accounts of the accident; singular case of eye so being retained after extensive injury. Certain parts of the sclerotic are more frequently ruptured than others; causes of this. Prognosis of ruptured globe not necessarily hopeless; treatment.

That the scletoric and choroid should be extensively ruptured the conjunctive remaining uninjured, and that the lens, slipping out through the rent, should become lodged beneath the unbroken conjunctiva, would *à priori* appear a most improbable occurrence. Such accidents, however sometimes come under the notice of ophthalmic surgeons.

The earliest history of the kind with which I am acquinted, is that briefly related by Edmonson, in his "Treatise on the varieties and consequences of Ophthalmia," &c. M'Kenzie, in the first edition of his well known work, described a case, and in a later edition, illustrated it by a sketch, taken shortly after the injury. Two other cases were seen by him; and Hunt, Middlemore, Van Onsenoort, Francke, Walker, Desmarres, Rivaud-Laudreau, Barrier, Pope, France, and Chadwick, have described with more or less detail, similar cases which have fallen under their notice.

I never myself had an opportunity of witnessing sub-conjunctival displacement of the lens until within the last year, during which period two cases have occurred at the Ophthalmic Hospital, Moorfields.

1. The first patient received a blow with a fist, which ruptured the sclerotic above the cornea, and forced out the lens under the conjunctiva at the same spot. The iris was not torn; but after the lens had been removed, and all irritation had subsided, the pupil remained drawn up towards the wound, and vision was limited to mere perception of light.

2. The second patient was struck on the eye with a piece of wood he was chopping. The rupture took place to the inner side of the cornea. The iris was in the same condition as in the preceding case, and vision impaired to almost the same degree.

It was hardly to be expected that an eye, after undergoing such extensive injury as rupture of choroid and sclerotic, with loss of lens, should still retain much sight; and yet a case came under my own observation, in which the organ not only sustained this amount of injury, but *loss of the whole iris* also, without the function of the retain being destroyed. I did not see the patient until eight months after the accident, the precise nature of which could only be deduced from the existing state of the eye. A description of the case was read before the Medico-Chirurgical Society, but was too short to be offered for publication in

their Transactions. I therefore subjoin the account, showing the appearnces presented when the patient came under my care.

Maria M'F——,aged forty-nine, received a blow with a fist on the left eye. The lids became much swollen, and she suffered great pain for some weeks; but she had no medical advice until she applied to me eight months after the accident. The cornea was then bright and clear, but all behind was dark, and no iris visible. On raising the upper lid I noticed a very faint bluish mark, about three lines long, just above the upper edge of the cornea. It seemed as if the sclerotic had been divided there, and afterwards repaired by substance rather less opaque than the original structure. Three or four little dots, like particles of black pigment, appeared beneath the conjunctiva, close to the mark in the sclerotic.

The patient kept her hand over the injured eye, finding that otherwise the light dazzeled it and so interfered with her making good use of the sound one. By means of a convex glass I threw light into the eye, to discover what had become of the iris. I could then see into the posterior chamber, and distinctly perceive the surface of the retina: but no vestige of the iris could be discovered. I held a lighted candle before the eye to ascertain the condition of the lens. A single upright image, reflected from the cornea, showed that the iris was also wanting. Vision was limited to the preception of large objects. She could distinguish the form of a sheet of paper, but could not see letters printed on it. I made her look through a magnifying glass: to her surprise she could then make outsome of the larger capitals. I added to the glass a card, perforated by a small hole, and she saw every object distinctly, and read a "brevier" type. By thesetwo expedients I had temporarily supplied the lost parts of the organ, the glass acting as a crystaline lens, while the perforated card screened the retina in the manner of an iris.

It appears probable, therefore, that the blow *she* received had ruptured the coasts of her eye—perhaps the conjunctive also—*and* at the same time,had completely torn the iris from its ciliary attachment; *both* lens and iris escaping through the wound, and the rent in the sclerotic afterwards healing up.

The most curious feature in the case is this—that after so extensive an injury the function of the retina was preserved, while the vitreous humour had been so far retained that the figure of the globe was but slightly altered, and its bulk not appreciably diminished.

Some of those who saw this patient, although unable to account in any other way than I had done for the manifest loss both of lens and iris, had difficulty in reconciling my expressed opinion of the nature of the accident with the very faint traces of injury visible in the sclerotic. But an excellent illustration of the extent to which a breach in the coats of an eye may be effaced was afforded by case 1, related above, in which the lens was disclosed under the conjunctiva, and removed from that situation by one of my colleagues; and yet eight months after the accident the position of the wound in the sclerotic could scarcely be traced, except by a few minute dots of black pigment which had had been carried out with the lens, and become lodged under the conjunctiva. The appearance of this man's sclerotic resembled so closely that of my patient, Maria M'F——, even to the scattered dots of pigment, that to give a drawing to his eye would, in this respect be almost to copy hers.

From the recorded cases of sub-conjunctival dislocation of the lens, it

would appear that the point where the sclerotic usually gives way is either *above* the cornea, or to its *inner* side. Among twenty-six patients, I do not find one instance in which the sclerotic has been torn below the cornea or to its outer side; nor have I myself ever observed such to be the case when rupture of the sclerotic, without displacement of the lens, has occurred. Now, as the sclerotic is equally thick and strong at all the points of any circle drawn concentrically to the circumference of the cornea, the rupture, if it were produced by direct violence, would as often occur at one side of the eyeball as on another. But it seems that the sclerotic always gives way under the extreme bending of its fibres, which takes place at the point nearly *opposite* to that which receives the blow; and this is commonly inflicted on the outer, or the lower side of the globe, the inner and upper sides being protected by the prominence of the nose and superciliary ridge.

The prognosis of ruptured globe, with displacement of the lens, and partial or total separation of the iris from its attachments—even if unattended with laceration of the retina, or large extravasations of blood—must of course be unfavourable; and yet the history of recorded cases of this accident shows it to be by no means of so invariably destructive a kind as to deter the surgeon from all hope of doing good. But he must not trust too much to "energetic treatment;" for those cases seem to have done best eventually where there was the least amount of interference with the reparative efforts of Nature, but where the one essential—perfect repose of the injured organ—was secured.

The striking benefit attending the use of mercury in idiopathic inflammations of the eye, has led many persons to believe that it must be as effectual in combating inflammation resulting from violence. But those who in the latter case, employ bleeding and mercurializing, seem to overlook the fact, that when the coats of an eye ball have been divided, Nature's first attempt towards repairing the mischief consists in *increasing not lessening*, the flow of blood to the part. The breach can only be filled up by the organization of material deposited there by the blood: to bring the patient under the influence of mercury diminishes the tendency of such material to become organized, and thus counteracts the very efforts Nature is making to repair the breach. As to "moderating the determination of blood to the part," inasmuch as we have no means of precisely knowing how much blood is necessary to furnish an adequate quantity of reparative material, we may, by bleeding, be depriving Nature of her very material for cure. The blood of one patient is rich in reparative matter, the blood of another is poor. What means have we of appreciating the exact quantity of this matter which, in any given patient, is being carried to the wounded eye?

All that the surgeon can do when called in to cases of ruptured globe, is to inform himself, as well as he is able, of the habit of the patient, and endeavour to keep his powers as near the standard of very good health as possible. As regards local treatment, the duty of the surgeon consists in maintaining the wounded part *in perfect repose*, both in respect of motion and light. For this purpose it is not sufficient to bandage only the eye which has been wounded. Both eyes must be kept covered, or the movements of the sound one, will of course be accompanied by corresponding movements of the other. A week or ten days is not too long a time for keeping the lids *uninterruptedly* closed, without examining the injured eye. Premature motion, and exposure to light, are almost sure to be followed by irritation and pain.

I need hardly add, that in cases of sub-conjunctival displacement of the lens, that body is to be removed. by carefully dividing the conjunctiva covering it. Should the iris have been detached from its connexions, and hang out of the wound, it should be snipped off close to the surface of the globe. These, and all other manipulations which may be found necessary, should of course be performed as much as possible without pressure on the eye-ball; and this evil may be best avoided by letting an assistant hold the lids asunder with specula. By some surgeons it has been recommended to delay the removal of the displaced lens for a few days, to allow time for the breach in the sclerotic to close. This delay would manifestly be improper if the lens were to be the cause of pain. In that case it must be removed at once.

That the state of patient's bowels should be attended to;—that, if restless, he should be soothed with such narcotics as experience may have proved suitable to him, or as the surgeon's judgment may suggest;—that the amount of food should be regulated by the vigour of the patient's circulation; and stimulants either given or withheld on the same grounds: all these are points which must be left to the good sense of the surgeon, since no fixed rules can be laid down as applicable to the treatment of all cases.—*London Lancet, Feb.*, 1853.

ON TEMPORARY ALBUMINURIA: MORE PARTICULARLY OCCURRING IN THE COURSE OF CERTAIN FEBRILE OR OTHER ACUTE DISEASES.

By Dr. J. W. Begbie,—Physician to the New Town Dispensary, Edinburgh,&c.

[Dr. Begbie divides his subject into three parts:—*Desquamative Albuminuria*, under which head he classes the urine in erysipelas, Asiatic cholera, and scarlatina; *Inflammatory Albuminuria*, under which he places the urine in the dropsy following scarlatina; and *Critical Albuminuria*, in which he considers the urine in pneumonia and certain cases of typhus. In speaking of the first head, he states his belief that in every case of scarlatina, a small amount of albumen would be found on careful examination. This is generally found three or four days after the commencement of desquamation. In examining the urine, both the nitric acid and heat tests should be employed. It should be carefully made for a few days before, and until the process of desquamation is fairly completed. Dr. Begbie believes that after it has once disappeared it will not return.]

The Microscopic character of the urine, with which the albumen is invariably associated, is the presence of a considerable amount of epithelial, derived from the different parts of the urinary apparatus. Sometimes the entire epithelial lining of the small tubes of the kidney was present, though certainly not frequently. I do not remember to have seen in the urine of simple scarlatina the albuminous or fibrinous casts of the small tubes of the kidney, the appearance of which is so common in the urine of the dropsical affection. Besides epithelium, the urine generally contained amorophous urate of ammonia, sometimes crystalline uric acid; and occasionally, though very rarely, the urine, though examined very soon after micturition, contained crystals of the ammoniaco-magnesian phosphate. In all such there existed a greater than usual amount of epithelium and mucous sediment. It is not uncommon to find octahedral crystals of oxalate of lime in the urine at the same stage of the disease.

The pathological import which the existence of albumen in the urine denotes, is a point on which difference of opinion must still be expected to exist seeing not only how very different are the facts recorded in regard to the occurrence of albumen, but how varying the estimation of the importance be which is awarded to its presence. While many believe its manifestation to be accidental, and of no importance, there are others who conceive it, if at any time accompanied with dropsy, to be its certain prelude. Both of these opinions I have attempted to show are erroneous, and, at least so far as my own observations go, founded on incorrect data. What then is the cause of albumen in the urine in simple scarlatina, and what its pathological import? I conceive it to be as essential a symptom of the disease as is desquamation of the cuticle—to be associated to a certain extent with that desquamation—to be, in fact, the result of a desquamative process, which the mucous membranes in this disease, equally with the skin, are subject to. Granted then, that this desquamation occurs, when such a change is taking place in the epithelial membrane lining the minute tubes of the kidney, the office of the cells composing which is to eliminate from the blood the matters, solid or fluid, which in the normal exercise of the renal function compose the urine, it surely is not surprising that the albumen from the former should, to a slight amount, enter into the latter. Such I believe to be the cause of its occurrence; nor can I regard its presence as indicating any pathological condition, further than the separation of epithelial cells and their passage in the current of the urine. No symptoms referable to any such condition occur, no febrile reaction, no lumbar pain, no non-elimination of urine, no suppression of its wattery parts not even any diminution in its quantity, and with the exception alone of the presence of albumen, no marked alteration in any of its sensible qualities. I have said that this albuminous condition of the urine in scarlatina is associated with the cuticular desquamation, it is so in the time of its occurrence, and so it is also as regards its amount, for I have noticed the albumen in the urine to be greatest in amount and to continue longest, in those cases in which the desquamation had taken place to the greatest extent. In those cases of the urine of which no coagulability has taken place—for my more recent experience has shown me a few such—there has been no marked desquamation, and no direct evidence of any epithelian separation, as shown by examination of the urine. We know that in many cases of scarlatina, especially in those where the eruption, though well-marked has not been brilliant, extensive, or lasting, it is not uncommon for the desquamatic process not to take place at all, at most to a comparatively very slight extent. Such are the cases in which the coagulability of the urine will perhaps not occur. I say *perhaps*, for in some such I have, notwithstanding, found it. I am still, therefore, disposed to regard the temporary albuminuria of scarlatina as probably as frequent in its occurence, and of the same importance as a symptom, as the desquamation of the cuticle.

[In speaking of the urine in Asiatic Cholera, Dr. Begbie remarks upon the suppression of urine as a marked symptom of a return to the proper quantity of that secretion, marking a favourable change in the disease, in many cases of death from cholera, death seems to have been produced from the poison in the blood, producing coma—such as is ordinarily the case where suppression of urine has occurred.

In Erysipelas, though the urine is frequently found albuminous during convalesence, yet it is not so certainly so, as in scarlatina.

Inflammatory Albuminuria.—Under this head Dr. Begbie refers to one example—the dropsical disease following scarlatina—as follows :]

Every one who has paid attention to the condition of the urine in this most interesting affection, must have noticed the great dissimilarity subsisting between its external and other characters, and those of the urine in simple scarlatina; while in the latter, the amount of urine passed except during the continuance of febrile symptoms, is undiminished, one of the most certain forerunners, as it is always the most invariable accompaniment of dropsy, is the excessive reduction of the quantity of urine. This urine when further examined, is found to contain a large amount of albumen; while under the microscope, frequently blood, not unfrequently exudation corpuscles or compound granular cells, always much epithelium, and the fibrinous casts of the renal tubes are recognised. The symptoms which accompany these changes in the urine are generally well-marked, the most prominent, save the dropsy, being a very uneasy, often severe, lumbar pain, and marked febrile excitement, But independent of these general symptoms, it will I think be admitted, that the characters presented by this urine, while they differ from those of the urine in simple scarlatina, indicate also the existence of a much more serious change in the secreting mucous membrane of the kidney, than a merely desquamative one. In order however, to arrive at a correct opinion in regard to the pathological importance of the change undergone in the kidney during the dropsical disease, it is necessary to bear in mind both the symptoms presented by the patient, and the hints afforded by the characters of the altered urine. These taken together give evidence of general febrile excitement, and of renal congestion, inflammation, and exudation. I have examined the urine in many such cases, and have found the albuminous condition much more lasting than in the simple cases, —indeed observations and experience show now pretty plainly that the long continued albuminuria of dropsical scarlatina, may and often does lead imperceptably—insidously it may be—to orgaaic renal disease. In many instances I have found the inflammatory symptoms alluded to, speedily and entirely disappear. I have not seen many cases of the dropsy following scarlatina, which I had watched from the commencement of the primary disease, but I have seen a few, and in all such the dropsical and aggravated symptoms appeared at the time the temporary albuminuria was going on, and were evidently the result of exposure to cold. This variety of albuminuria, then, which I have called inflammatory, may or may not be *temporary*; it is to be feared that not unfrequently neglected, or even unskilfully treated, the affection it accompanies, lays the foundation of permanent renal disease. In most cases however, it is fortunately otherwise, while in nearly all it may be looked upon as, under judicious management, a curable disorder.

[*Critical Albuminuria.*—Several trustworthy observers have noticed the frequent occurrence of albuminuria in pneumonia, and, says Dr. Begbie,]

To this albuminuria I have given the title of Critical Albuminuria, because my data being correct, and my conclusions justifiable, it is to be regarded as an evidence of a critical action, and commencement of a change undergone by a diseased part, before its return to a healthy state. But I can further illustrate this subject by a reference to the changes which occur in typhus

fever. I have found albuminuria by no means an uncommon attendant on the convalescence from typhus; not however, nearly so invariable in its occurrence as in scarlatina, or even so common as in pneumonia; so frequent however, as to lead me to examine all cases in which it occurred, and that with very great care. The result has been, that no one of any such cases has, either at the time, or during a considerable period of observation afterwards, afforded the evidence of any organic change in the kidneys, to account for the albumen in the urine.

The albuminuria in the case of typhus appears to me of special interest as occurring much more frequently, if not entirely, in certain cases of typhus. It is in those cases in which we know, or have reason to suspect, that the deposits, have taken place in internal organs, and we find albumen in the urine. Two or three observations of a somewhat different nature have led me to this conclusion; for example, I have found the urine albuminous in cases of abdominal typhus,—that is, in those cases in which we generally find severe diarrhœa as a symptom during life, and deposit in the intestinal glands as the most prominent lesion after death. In several cases of this kind, which proved fatal, I have found albumen in the urine for days before death; and in others, which happily recovered, I have as frequently noticed its occurrence. In both those instances the albumen appeared for the most part, at an advanced period of the disease, at least after the particular symptoms had continued for some time; while in the former, the albuminuria continued up to death; in the latter, in some it disappeared as convalescence was fairly established, and in others it lasted for a longer period. The amount of albumen in these cases, and the other characters with which the coagulability was associated, were exactly as I have described them in the example of pneumonia; and finding the albuminuria to bear a relation to the deposits in internal organs in typhus, I have been led to regard the kidneys as the emunctories by which the morbid matter so deposited to a certain extent is at least removed from the system,—and so doing, to regard the temporary albuminuria of typhus as a critical albuminuria. It is I think, no objection to this view that deposits, such as those referred to, remain in organs for a lengthened period; for firstly, I do not think that we can pretend to limit the period of their removal or disappearance; and I am inclined to believe that when they do so disappear, the urine will very probably contain the ingredients I have noticed; and secondly, the calceous masses found in the spleen, and other organs, accepted as the earthy remains of the deposits spoken of, certainly attests the removal by some channel or other, of the animal matter of which, in their original condition, these deposits were partly composed. This is an interesting subject, and invites further inquiry.—*Monthly Journal of Medical Science, October*, 1852, *p*, 321.

ON THE INFLUENCE EXERTED BY CHRONIC DISEASES UPON THE COMPOSITION OF THE BLOOD.

By MM. Becquerel, and Rodier.

A paper recently read at the Académie des Science, details the results of MM. Becquerel and Rodier's latest hœmatological researches.—1. The majority of chronic diseases and various anti-hygenic circumstances induce an increase or diminution in the three principal elements of the blood—the globules, the fibrine,

and the albumen, and this either separately or simultaneously —2. The globules undergo diminution in the course of most chronic diseases of long duration, and especially in organic diseases of the heart, the chronic form of Bright's disease, chlorosis, marsh cachexia, hæmorrhages, hæmorrhoidal flux excessive blood-letting the last stages of tubercular disease, and the cancerous diathesis. The same result is observed in those whose food is not sufficient in quantity or reparative power, or who are exposed to insufficient aëration, humidity darkness &c.—3. The albumen of the serum of the blood is diminished in quantity in the third stage of heart disease, great symptomatic anæmia, the cancerous diathesis, and insufficient alimentation.—4. The fibrine is maintained at its normal proportion, and sometimes increased, in acute scorbutus. It is diminished in chronic scorbutus, as also in the scorbutic condition symptomatic of certain chronic diseases, which is most often and most markedly observed in organic diseases of the heart.—5. In all the above mentioned circumstances, the quantity of water contained in the blood becomes very considerably increased.—6. A diminution of the proportion of globules is especially accompanied by the following phenomena: a colourless state of the skin, palpitations, dyspnœa, a *bruit de soufflet* heard at the base of the heart during its first sound, an intermittent *bruit de soufflet* in the carotids, and a continuous *bruit* in the jugulars.—7. The diminution of the proportion of albumen, even though not very considerable, when it takes place in an acute manner, rapidly gives rise to the production of dropsy, but it requires to be much more considerable when not appearing in the acute form. Considered in a general manner, dropsy is the symptomatic characteristic of a diminished proportion of the albumen of the blood.—8. A diminished proportion of fibrine is manifested by the production of cutaneous or mucous hemorrhages.—9. In anæmia symptomatic of considerable hemorrhage or insufficient alimentation, the charge in the blood is characterised by a diminution of its density, an increase of the water, diminution of globules, a maintenance of the normal proportion, or sometimes a slight diminution of the albumen, and a normal proportion of fibrine.—10. In chlorosis, which is an entirely distinct affection from anæmia there may be no changes in the blood whatever. When such are present, they consist in a diminution of the proportion of globules, an increase of that of the water, and the normal quantity or an increase of the fibrine.—11. In the acute form of Bright's disease, the fibrine continues normal, and the albumen is diminished. In the chronic form there is a diminution of globules and albumen, and sometimes of fibrine.—12 Most of the dropsies regarded as essential depend upon a diminution of the proportion of albumen and usually originates in a material cause, consisting in a degeneration of the solid or fluid parts of the economy.—13. In diseases of the heart, the blood becomes more and more changed, as they approach the fatal termination. The changes consist in the simultaneous diminution of globules, fibrine, and albumen, and an increase of water.—14. In acute scorbutus, the principles of the blood do not undergo any appreciable modification. In the chronic form the fibrine is notably diminished, while the globules are sometimes considerably increased. In both forms, the increase of the proportion of soda of the blood explains all the circumstances; but it has not yet been demonstrated.—15. The above modifications should influence our therapeutical management of these morbid conditions, as each element of the blood is susceptible of special modifications. Thus when the proportion of

albumen is diminished, we prescribe cinchona, and a tonic strengthening diet. A diminution of fibrine and an increase of the soda of the blood are to be met by good diet, vegetable acids, and appropriate hygiène; and by the hygienic measures and the exhibition of iron, we combat the diminution of globules. —*L' Union Médical—Medico-Chirurgical Review July*, 1852, *p.* 256.

ON SOME OF THE PRINCIPAL EFFECTS RESULTING FROM THE DETACHMENT OF FIBRINOUS DEPOSITS FROM THE INTERIOR OF THE HEART, AND THEIR MIXTURE WITH THE CIRCULATING BLOOD.

By Dr. William Senhouse Kirkes.

[The following observations on the above subjects were communicated by Dr. Burrows, to the Royal Medical and Chirurgical Society.]

As an introduction to the Subject, the author observed that it was a clearly established fact, that the fibrinous principle of the blood might under certain circumstances, separate from the circulating fluid, and be deposited within the vascular system, especially on the valves of the heart. The forms of fibrinous concretions to which the following observations especially applied, were, first the masses usually described as Laennec's globular excrescense; and secondly, the granular or warty growths adhering to the valves, and presenting innumerable varieties, from mere granules to large irregular fungous or cauliflower excrescences projecting into the cavities of the heart. These growths, when once formed, whatever might be their origin, were full of peril, and would often remain so, long after the circumstances which give rise to them had passed away. When of large size, or loosely adherent, they might at any time be detached from the valves and conveyed with the circulating blood, until arrested within some arterial canal, which might thus become completely plugged up, and the supply of blood to an important part be suddenly cut off, from which serious if not fatal results would ensue; or smaller masses might be detached, and pass on into arteries of much less size, or even into the capillaries whence congestion, followed by stagnation and coagulation of the blood, and all the consequent changes such coagulated blood is liable to undergo in the living body, would necessarily follow. Many singular morbid appearances observed in internal organs, and not well accounted for, were probably brought about in this manner. Again the masses of fibrine might soften, break up, and discharge the finely granular material resulting from their disintegration into the circulating blood, and, contaminating this fluid, might excite symptoms very similar to those observed in phlebitis, typhus and other analogous blood diseases. Thus the fibrinous material detached from the valves, or any other part of the interior of the heart, might be the cause of serious secondary mischief. The parts of the vascular system in which these transmitted masses of fibrine might be found, would in a great measure, depend whether they were detached from the right or left cavities of the heart. Thus, if from the left, they would pass into the aorta and its subdivisions, and would be arrested in any of the systemic arteries or their ramifications, and especially into those organs which receive large quantities of blood direct from the left side of the heart, as the brain, spleen and kidneys; on the contrary, if escaping from the right cavities, the lungs would necessarily become

the primary, if not the exclusive seat of their ultimate deposition. A division of the subject being thus naturally formed, the author proposes to consider the subject, first, as to the remote effects resulting from the separation of fibrinous deposits from the valves or cavities of the left side, and secondly, as to the corresponding effects produced by the detachment of like deposits from the valves or cavities of the right side of the heart. The author then proceeded to elucidate the first branch of the subject, in which masses of some magnitude were detached from the left side, and arrested in an arterial channel of notable size. This pathological fact was illustrated by three cases, in many respects identical; for in each, death appeared to ensue from softening of the brain consequent on obstruction in one of the principal cerebral arteries, by a mass of fibrinous material, apparently detached from the growths on the left valves. The first case was that of a female, aged thirty-four, of pale and delicate aspect. She had suffered from rheumatic pains, and there was a loud systolic murmur heard over the entire cardiac region. While under treatment for these symptoms, she suddenly fell back as if fainting. She was found speechless, with partial hemiplegia of the left side, but there was no loss of consciousness; the hemiplegia increased, involved the face and limbs, and gradually became complete in regard to motion, but sensation remained unimpaired. These symptoms lasted five days, when she quietly died. The post-mortem examination developed much congestion of the pia mater, amounting in some places to ecchymosis. The right corpus striatum was softened to an extreme degree—being reduced to a dirty greyish-white pulp. In the posterior lobe of the right cerebral hemisphere, was a similar spot of pale softening. The right middle cerebral artery, just at its commencement, was plugged up by a small nodule of firm whitish, fibrinous-looking substance, not adhering to the wall, but rendering the canal almost impervious. The vessels of the brain were generally healthy, except a yellow spot or two in the coats of those at the base of the brain. The heart was enlarged; several broad white patches externally. The right valves were healthy, so also were the aortal; but the mitral valve was much diseased, the auricular surface being beset with large warty excrescences of adherent blood-stained fibrine. The right common iliac artery, about an inch above the origin of its external branch, was blocked up by a firm, pale, laminated coagulum, which extended into the internal iliac. The pleuræ were adherent in places; liver and intestinal canal healthy; spleen large, pale, and soft, and contained a yellowish-white, cheesy substance. The kidneys were pale, rough and granular; within the cortex of the right were several large masses of yellow deposit, surrounded by patches of redness. Death had resulted in this case from the softening of a large portion of the right side of the brain, which the author considered to have arisen from an imperfect supply of blood, consequent on the middle cerebral artery of the same side being obstructed by a plug of fibrine. The author then discussed the sufficiency of such an obstruction to produce the effects ascribed to it, and he brought forward many examples showing that atrophy and disorganization usually resulted from any circumstance which materially impeded, or entirely cut off, the supply of blood to a part. The author then directed attention to the probable source of the fibrinous plug found in the middle cerebral artery. The suddenness of the cerebral symptoms rendered it probable that the blocking up of the artery was equally sudden, and

not the result of gradual coagulation of the blood within the vessel. The absence of all local mischief in the coats of the artery at the point of obstruction, as well as elsewhere, pointed to some other than local origin for the clot; and the author, at the time of the examination, formed the opinion, that a part of the fibrinous deposit on the mitral valve had become detached, and carried by the stream of blood, until arrested at the angle whence the middle cerebral proceeded. This explanation suited equally for the plug found in the common iliac; for it was quite conceivable that portions of the loosely adherent fibrine might be easily detached by the stream of blood washing over the mitral valve, and when once admitted into the circulating current, they would only be arrested by arriving at a vessel too small to allow their transit along its canal. Two other cases were described by the author, possessing many interesting points of resemblance: one, a female, aged twenty-four; the other, a male of the same age. Both were admitted into the hospital with hemiplegia of the left side; each had heart disease, indicated by a long systolic murmur. The post-mortem examinations revealed the following morbid appearances common to both:—

Softening of a limited portion of the brain, producing death by hemiplegia; obliteration of the cerebral artery supplying the softened part; coagula in one of the iliac arteries; fibrinous deposits in the kidneys and spleen; and the presence of fibrinous warty excrescences on the valves of the left side of the heart. So many and such rare features of resemblance could not fail to demonstrate a very close connexion between the several morbid appearances so exactly reproduced in each case. The author believed that these three cases satisfactorily established the two following conclusions—1st, that softening of a portion of the brain, with attendant loss of function, might result from obstruction of a main cerebral artery by the lodgment of a plug of fibrine within its canal; 2ndly, that the foreign substance thus obstructing the vessel was probably not formed there, but was derived directly from warty growths situated in the left valves of the heart. The author thought it not improbable, although on the absence of direct proof it was but supposition, till further investigation confirmed these facts, that many cases of partial and temporary paralysis suddenly ensuing in one or more limbs of young persons, especially if accompanied with signs of cardiac disease, might be due to interruption of a proper supply of nutriment to the brain by the temporary plugging up of a principal cerebral artery by fibrine, detached from a diseased valve on the left side of the heart. Other arterial branches, besides those of the base of the brain, might arrest these fibrinous deposits derived from the valves of the heart. In case 1 and 2, coagula were found in the iliac and femoral arteries; and in case 3, in the renal. The author thought that many specimens found in museums, and supposed to illustrate the spontaneous coagulation of the blood, or the deposition of fibrine within a limited portion of an arterial trunk, were probably to be referred to the same cardiac origin, and he illustrated the point by reference to a preparation in the museum of St. Bartholomew's Hospital. The second subject of inquiry consisted of an examination into the effects produced by smaller portions of fibrine detached in a similar manner, but arrested in the minute arterial branches, or even in the capillaries. The author thought that the singular masses of yellow fibrinous substance found in the spleen and kidneys, and other organs

and hitherto described as "capillary phlebitis," "metastasis," or "fibrinous deposits," were derived from this cause. Out of twenty-one cases in which the author had observed these deposits in the spleen and kidneys, or other parts deriving blood directly from the left side of the heart, in nineteen there was disease of the valves, or of the interior of the left side of the heart. In fourteen of these there were fibrinous growths on the surface of the left valves; in the remaining five there was simple mention of valvular disease. The author thought that the mere fact of so large a number of cases of so-called "capillary phlebitis" being associated with the presence of fibrinous deposit on the valves of the heart, suggested a very close relation between the two morbid states. The author then entered upon the third branch of this part of the subject, concerning the series of effects which might result from the introduction of fibrinous particles into the circulating blood, manifesting phenomena indicative of the existence of a morbid poison in that fluid. A case was related of a youth, aged fourteen, admitted into the hospital with obscure typhoid symptoms, the surface body of the being covered with petechiæ. Delirium, with much febrile prostration, followed; he became subsequently comatose, and died. Upon an examination of the body, the surface was found covered with petechiæ. The pia mater was infiltrated with what seemed recently effused blood. The surface of the brain thus presented a blotchy appearance, amid these spots were yellow-coloured patches of various sizes; some were of a greenish yellow hue, and had the appearance of being smeared over with pus. The brain was unduly congested, and ecchymosis was near the surface; the cerebral arteries and sinuses healthy; several petchial spots on the surface of the heart, as well as in the cavities; and on the auricular surface of the mitral valve some white fibrinous vegetations, very soft and friable; a like deposit on the aortic valves, with evidence of ulceration; several large yellowish blotches extended deep into the substance of the cortex. The intestinal mucous surface was covered with petchial spots, which were apparent also on the mucous membrane of the bladder, pharynx, œsophagus, stomach, larynx, and trachea. The author considered the mystery of this case cleared up by the post-mortem examination. The attack had been ushered in by a severe pain in the right groin, which was rheumatic; the ensued rheumatic inflammation of the mitral and aortic valves, with ulceration of the latter, and deposition of fibrine. From these deposits portions had probably separated during life, and were transmitted with the blood to all parts of the body, and being arrested in the capillary networs and smaller arteries, produced the various petchial and buff-coloured spots above described.

The second part of the paper related to the effects which might result from the detachment of fibrinous deposits from the right valves of the heart. Reference was made by the author to a paper on the Formation of Coagula in the Pulmonary Artery, by Mr. Paget, published in the 'Transactions of the Society,' as well as to a specimen in the museum of St. Bartholomew's Hospital, in which there was deposition of fibrine on each of the pulmonary valves, with old coagula filling many branches of the pulmonary artery. In this case several large solid, fibrinous masses were found in the substance of the lungs, presenting appearances not unlike portions of old pulmonary apoplexy. Lastly, the author recapitulated the principle points which he was desirous of establishing, viz.,—1st. That fibrinous concretions in the valves of the heart admit of being readily detatched during life. 2nd. That if detatched and transmitted in large masses, they

may suddenly block up a large artery, and thus cut off the supply of blood to an important part; if in smaller masses, they might be arrested by vessels of smaller size, and give rise to various morbid appearances in internal organs; or the particular mingled with the blood might be but the *debris* of softened fibrine, yet with power to produce a poisoned state of the blood, and bringing on typhoid or phlebitic symptoms. 3rdly. That the effects produced and the organ affected would be in a great measure determined by the side of the heart from which the fibrinous material had been detached; if from the right sid, the lungs would bear the brunt of the secondary mischief; but if, as was most commonly the case, the left valves were the source, the mischief would be more widely spread, and might fall on any part, but especially on those organs which were largely and directly supplied with blood from the left side of the heart, as the brain, spleen or kidneys.—*Lancet June 5*, 1842, *p.* 542.

CASE OF DEATH FROM THE FORMATION OF FIBRINOUS CONCRETION IN THE HEART.

By Dr. Walter Carslage, Saddleworth.

[The patient in this case was aged 64. He was attack with erysipelas of the head and face, combined with symptoms of congestion of the liver. From these he gradually recovered, though the system did notseem to regain strength, and the heart's action was very feeble. Five weeks after the first attack he caught a slight cold, and a mild repetition of the erysipelas, and hepetic derangement appeared.]

These symptoms, however, were checked as speedily as before; the appetite returned and digestion seemed to be easily accomplished; and yet, with all these favourable appearances, his strength failed the action of the heart and respiratory movements gradually grew feebler, and at length without any sign of pulmonary, cerebral, or abdominal disease, these asthenic symptoms slowly terminated in death, just eleven weeks after the first commencement of the erysipelatous attack.

Post-mortem.—With the exception of a slight enlargement of the liver, we could find no organ exhibiting structural change. The lungs were perfectly healthy. Upon opening the heart, however, we discover what in my opinion, fully accounts for the gradual dissolution; a fibrinous mass filled both its right cavities, and sent up large and long branches into the pulmonary artery and its ramifications. The concretion was firm and white, and had an attatchment to the walls of the heart.

Remarks.—I have but few remarks to make on this interesting case, and I should probably never have thought of publishing it at all, had not my attention been made before the Medical Society of London, by Mr. B. W. Richardson, "On the Fibrinous Element of the Blood," and which have been reported in the columns of 'The Lancet.' I find that in a paper read by Mr. Richardson in November last, he thus observed: "Lastly, in case of asthema, where fibrinous concretions exist in the heart, the very cessation of the act of life may be owing to their presence and gradual increase, the central organ of the circulation becoming literally choked by them." In January, again, the same author briefly alluded to this subject, and produced a pathological specimen which strongly

supported his views; and, lastly in the month of March, on exhibiting another heart in which a fibrinous clot was found, he gave more enlarged views on the matter, and threw out the idea, that during those diseases which are known to be attended with an abnormal quantity of fibrine in the blood, it may be that some of the overplus of fibrine is deposited on the elevated structure of the moving heart; and he concluded by saying that, "In all inflammatory cases marked by great super-fiormation of the blood, and which end by what is called sinking, it would be interesting to learn how far similar concretions in the heart may be concerned in bringing about the sinking state."

Now, without wishing to mention the many theoretical points which Mr. Richardson and other physiologists enter into with reference to the formation of fibrine, &c., &c., I cannot but observe, that the case which I have related above affords striking testimony as to the correctness of the opinions from which I have just quoted. My patient had suffered from erysipelas, a disease in which the blood is always super-fibrinized: he sank in the most gradual manner, and the autopsy revealed no cause for the sinking, except (what was surely sufficient) a large fibrinous concretion in the heart.

Of course the narration of a single case does not go far to establish any new opinion, but perhaps it may excite others to turn their attention to the same subject. I have been puzzled, over and over again, at seeing patients gradually sink into death, after some slight disorder, with no evident disease that could account for such a serious result. Now if any explanation so simple as that given by Mr. Richardson should prove after further research, to account for some of these occurrences, a great step in the practice of medicine will certainly have been made.—*Med Times Gazette, Sep.* 11, 1852, *p.* 259.

ON FIBRINOUS DEPOSITS ON THE LINING MEMBRANE OF VEINS

By Henry Lee, Esq.

Simple inflammation of the veins—that is to say, inflammation commencing in the coast of veins—is regarded by the author as a very rare disease. The lateral lining of veins especially would appear to be as little susceptible of inflammation as any structure in the body. The large number of instances of phlebitis met with in surgical works, occurring in daily practice, are regarded by the author as depending upon and as being excited by, a vitiated condition of the blood. This opinion is principally supported by the two following facts: first, that in every case of so-called inflammation of the veins, the blood will be found to have coagulated in the vessels; and secondly that where such coagulation does not take place, no inflammation will be produced. Continental writers of the highest reputation, have indeed mentioned the concentric layers of lymph which are secreted as the result of inflammation in the interior of veins and English writers, whose names carry with them the greatest authority, have described the adhesions of the opposed sides of the veins by lymph secreted from the capillaries under a state of inflammation. The advocates of this view have particularly referred to an experiment by M. Gendrin in which he mentions that by introducing irritating substances into the arteries and veins, he obtained large deposits of lymph upon their interior. The author on the contrary, having found that inflammation of the coasts of the veins only occurred in cases where the blood has previously coagulated in them, was induced to believe that the deposit found in the veins might be derived directly from the blood. M.

Gendrin's experiment was therefore repeated, precautions being taken to exclude all blood from the vessels; and it was found that under these circumstances no lymph was effused in the vein. The lining membrane of the veins does not contain any blood-vessels of its own nor does it required any, being in direct contact with the blood. It appears reasonable to suppose, that under such circumstances it would not secrete lymph, and the experiments and observations of the author lead him to this conclusion. The lining membrane of a vein, the outer coats of which are inflamed, may undergo various changes, or may be disintegrated, and cast off into the cavity of the vessels. Lymph and pus may then be secreted into the interior of the canal; but this can only occur in the latter stage of the disease. The readiness with which some morbid poisons produce coagulation of the blood, and the constancy with which such coagulation (indicated by the cord-like induration of the vessels) is found to precede the other symptoms of inflammation, lead to the conclusion, that a vitiated condition of the blood is the common cause of phlebitis. Under such circumstances, although the irritation produced is caused by the morbid matter detained in the vein, yet the inflammation is at first manifest in the surrounding parts. The cellular tissue becomes distended with serum; the cellular coat of the vein then becomes thickened, red and inflamed; and finally, the changes which have been noticed extend to the lining membrane. The effects of inflammation thus are shown to extend to, and not from, the internal surface of veins. M. Cruveilhier indeed regards the coagulation of blood in a vessel as the effect of inflammation previously existing. But the author has satisfied himself, that if blood be prevented from stagnating in a vein, no change will be thereby produced in its lining membrane. The inflammation is not therefore propagated by continuity of surface, as has been generally supposed, but by the stagnation, in different parts of the vitiated blood. Coagulation of the blood would therefore appear to be the cause, and not the effect of inflammation of veins. This view is further supported by the fact, that simple adhesive inflammation of a vein will not produce coagulation of its contents. A preparation was exhibited, showing the effects of a ligature upon a vein twenty-four hours before death. No coagulation of the blood, nor deposits of fibrine on the lining membrane, had in this case taken place. The coats of the vein were thrown into folds, and a white band marked the situation of the ligature: but the projecting folds of the lining membrane presented their natural, smooth, polished, and lubricated appearance. Healthy venous blood will remain fluid for days, when confined in a vein by a ligature. In this respect there is a contrast between a vein and an artery. In the latter case, the internal coats are divided, and the blood coming into contact with the divided edges, immediately coagulates. In the vein, on the contrary, the lining membrane is not divided, therefore the blood remains in contact only with the natural lining of the vessel. Cases in which a small quantity of pus has been introduced into a vein affords the strongest contrast to those in which the coats have been mechanically irritated. In the latter case no coagulum will form, or only sufficient to unite any lesion there may be of the lining membrane. In the former on the contrary, extensive fibrinous plugs will occupy the vessels. These will sometimes occupy the whole diameter of the vein, and become firmly attached to its sides; at other times the outer layers only will become firmly coagulated, and the central ones will remain in a semi-fluid condition. It will sometimes happen that the central portions will be removed, leaving the outer layers attached to the walls of the vessel. The circulation may then be continued through an adventitious cylinder of fibrine. Cases occasionally occur, in which a delicate velvety layer only is deposited on the lining membrane, which remains unaltered in appearance in other parts. The coagula which form in veins will, under such circumstances, lose, in different situations, much of their colouring matter; and it will be observed that the lining membrane of the vein is coloured (from

imbibition) in exact proportion to the amount of colouring matter contained in the different parts of the coagula. It will occasionally happen, that portions of the decolorized fibrin will become organized and intimately connected with the sides of the veins, as illustrated in a preparation exhibited to the society. Such layers of fibrin appear constantly to have been mistaken for lymph, the product of inflammation. The extreme readiness with which the blood coagulates from the contact of purulent matter, affords a most important provision for the security of the general system. It appears to depend upon a faculty with which the blood is endowed for its self preservation. This faculty, although hitherto unacknowledged by physiologists, doubtless exists and is comparable to the preservative sensibility with which every other part of a living being is endowed. When purulent fluid is introduced into a vein, if the coagula are firmly formed, a local inflammation will alone ensue; but, if the morbid matter extends along the vessels, a high degree of constitutional irritation will follow, and the symptoms will occasionally bear a striking resemblance to those of typhus fever. In cases as they present themselves in practice, these two sets of symptoms are constantly present at the time; but they may be produced separately by a very simple experiment: if, for instance, purulent fluid be introduced separately into a vein, and allowed to remain undisturbed, local inflammation only will be set up, which will terminate in the formation an abscess around the vein. The contents of the vein will then become softened, and expelled externally, together with the contents of the abscess But, if the morbid matter be forced forward, in the course of the circulation, no local inflammation will occur, but the symptoms will indicate either the presence of secondary inflammation in some internal part, or of a general contamination of the blood. If the view taken of the origin of inflammation of the veins be correct, it will be evident, that any treatment, to be effectual, must have reference to the first periods of the disease; and that those remedies will most effectually guard the system against the contamination (so much dreaded in this class of cases) which will favour the sequestration of vitiated blood, and tend to localize the disease. The remedies which have been employed to subdue the local inflammation, appear but too often to have done so at the expense of the general system; for, although the local symtoms have become less prominent, fatal mischief has appeared in other parts. In severe cases, those remedies only can be safely employed which tend to preserve the power of the blood, and especially those which increase its coagulating power, as to enable it to separate that portion which has become infected from the general circulation. Bark and opium, together with a nutricious diet are the means which appear to favour these actions upon the due performance of which the safety of the patient depends; while bleeding and calomel, however useful they may be in a case of simple inflammation of the coats of a vein, appear inadmissible when the disease, as generally happens, originates in its contents.—*Med. Times, and Gazette May* 15, 1852 *p.* 503.

ON THE TOPICAL TREATMENT OF ACUTE INFLAMMATION OF THE LARYNX AND TRACHEA.

By Dr. Ebden Watson.

Professor of the Institute of Medicines in Anderson University, &c., Glasgow.

[Dr. Watson first endeavours to explain the modus operandi of a solution of nitrate of silver on an inflamed mucous membrane. He says:]

There is a little experiment, simple, and easily repeated, which is familiar to all who have paid attention to the recent advances made in our knowledge of the inflammatory process, and which presents us with an excellent occasion for observing the action of the solution of nitrate of silver, in the different stages and degrees of that morbid state. I refer to the excitement of inflammation in

the web of a frog's foot stretched out under a microscope. When, for example, a red-hot needle is passed through the web, the following are the phenomena observed:—A spot in the centre of the inflamed part is sphacelated, destroyed by the passage of the needle through it; a circle round the spot is usually found in a state of complete congestion, the vessels being dilated, and the corpuscles almost perfectly stationery within them, and in the part beyond this circle the vessels are not so much dilated, and the stasis of their contents is not so complete. The stream is seen passing slowly away in the collateral circulation of the unaffected parts of the web.

Now these two circles represent two degrees of inflammation, which it is important to distinguish wherever they occur, and, perhaps, especially when the seat of morbid action is the mucous membrane of the larynx or trachea. That part of the web of the frog's foot in which the stasis was complete represents the most intense, or sthenic degree; the other, in which the stasis was not so complete, represents what is usually called the sub-acute, and perhaps chronic, varieties. And the effects of the solution of caustic on each of these parts are markedly and importantly different. In the part which is most intensely inflamed, the solution, in the direct ratio of its strength, increases the stasis of the blood within the vessels. The latter seem to be unable to dilate further, and are, therefore, little changed, but the nitrate of silver acts through the coats upon the blood which they contain by causing its partial coagulation, and, likewise, by withdrawing water from the serum for the crystals of the nitrate which begins partly to form if the solution is strong. In that part of the web, on the other hand, which had been less intensely inflamed, the stimulant solution causes renewed and increased dilatation of the blood-vessels, and the retarded current moves on in them more freely than before; a cure being thus speedily affected if the exciting cause of the inflammation has ceased to act.

That precisely similar degrees of inflammation occur in the mucous membrane of the larynx and trachea with those just described as artificially produced in the frog's foot, I need hardly attempt to prove, for it will be at once admitted that there are three kinds of acute laryngitis; one, in which no false membrane is formed; a second, in which a false membrane is formed, but in which the pharynx, as well as the larynx, is affected, viz., the diptherite of French writers; and, lastly, that intense local inflammation of the larynx and trachea, accompanied by exudation, to which in this country we generally restrict the term "croup."

1. The first kind varies in its intensity from the most trivial catarrhal irritation, to a severe inflammation of the whole thickness of the mucous membrane lining of the windpipe. It very often commences about the fauces and passes downwards, causing cough more or less croupy in its character, difficulty of breathing, and horseness of voice. It is accompanied by fever of greater or less intensity, and its chief physical signs are, in increased loudness of the respiratory sounds in the larynx and trachea, with occasionally a whistling sound, from the absence of the natural mucus of the part, and from partial spasm of the glottis. This kind of laryngitis is more frequently complicated with bronchial inflammation than with pneumonia, and as the patient sinks from non-aeration of the blood, the most prominent symptoms of the laryngeal affection gradually give way, and it is often difficult after death to detect more than a slight redness of the bronchial mucous membrane. The inflammation sometimes, however, terminates in œdema glottidis, and but rarely in the exudiation of a false membrane.

I have mentioned the chief features of this kind of laryngitis, first because it is very common in its slightest degree, and is by no means rare, even in its utmost severity; and secondly, because it is not usually a very sthenic disease or, to speak more correctly, it does not present so very high a type of inflammation as that which is characterized by lymphatic effusion, and is, therefore,

an example of that degree of this morbid process comparable to the outside circle in the inflamed portion of the frog's foot. The proof of this lies in the absence of exudation.—the usual complication being bronchitis, not pneumonia,—the longer duration of the disease, and its frequent termination in serous effusion into the glottis, or by passing into the chronic form of laryngitis.

It will be expected, then, that the solution of caustic should act well in such cases, and so it does; but pehaps, in none is a greater amount of discrimination necessary in the adaptation of the strength of the solution to the severity of the inflammation which may be present, as well as in choice of the proper time for commencing the topical measures. For it must not be thought that I advocate restriction to any one remedy, local or general, in the treatment of this disease. In the severer forms of the affection especially, depletion of some kind will at first be necessary to check the violence of the inflammation, and an emetic will be useful in restoring the moisture of the mucous surface. It is after the use of both these remedies that the topical application is alone admissible, nor can it even then be employed to the exclusion of other means. In children it will be especially necessary to repeat the emetic several times during the progress of the case, and in very few will the judicious practitioner refrain from the use of counter-irritation to the outside of the throat while he is proceeding with the topical treatment internally. It is, however, with the latter that I have specially to do at present, and therefore to the management of it I shall, in a great measure, confine my remarks.

Contrary to what might, *a priori*, be expected, the result of those experiments I before alluded to is, that the more intense the degree of inflammation of the laryngeal lining, the weaker ought to be the solution of silver applied to it. In those cases in which the intensity of the inflammation has never been great, or in which, as is more likely to happen, the primary violence of the disease has been subdued by other treatment, a stronger solution may be used with advantage. Its first effect, when thus judiciously applied, will be to coagulate the albuminous film upon the surface of the membrane which has been stripped of its epithelum and thus to cover and protect it. Another, and almost simultaneous effect, is to stimulate the basement membrane to form new epthelium, and to secrete new mucus, and thus the artificial film of coagulated albumen is by-and-by replaced by a more natural covering, and the surface is lubricated by its appropriate moisture. If, then, a renewal of the morbid process could be prevented, a cure would already have been accomplished, but this is seldom or never the case. The good effects of the topical application wear off in a few hours, and the former abnormal phenomena may even in that time have reappeared in nearly equal severity. The treatment must therefore be continued —the touching of the larynx must be repeated frequently for some days, and indeed until all the symptoms of laryngitis have completely disappeared.

In some severe cases of this kind, especially in adults, there is great danger of a chronic thickening of the laryngeal mucous membrane being left behind, and of the voice being thus permanently impaired. It may, therefore, in such cases be a very good precaution to give a little mercury towards the tend of the acute attack; but whenever the mouth begins to be affected, the topical application of solution of caustic must be stopped; for the larynges lining, especially at its upper part, is then far too vascular and irritable to bear the touching, and its only effect would be to cause still greater excitement of the part. It is for this reason chiefly, and because I put considerable faith in the topical remedy that I should recommend the mercurialization to be postponed until the termination of the acute stage. After the mercury has had its required effect, it is often a good plan to repeat the topical treatment for a time, when its efficiency will be found very great in restoring the tone of the voice, and fitting the patient to bear a change of his apartment in the first place, and, ere long, removal to the country.

tubes. The exudation, which in these cases occurs after a short stage of intense erysipelatous redness, sometimes in considerable thickness. It is, therefore, in many respects different from the more local, firmly adhering, lymphatic exudation of true croup.

This seems to be a common affection on the Continent, especially in Paris, and also in New York according to Dr. Horace Green, who tells us that a solution of caustic acts admirably as a topical application in such cases. It seldom, however, occurs in this country, at all events in this city, except as an occasional, and happily rare, form of epidemic.

3. True exudative croup is altogether a different disease. It appears in very sthenic conditions of the general system; the blood is invariably rich in fibrine and corpuscles, and the exudation which forms in the larynx and trachea always contains, and is sometimes chiefly composed of, fibrine. It is, moreover, a purely local affection, seizing at once on the larynx or trachea, and confining its chief violence to one or both of these organs. It is, besides, more frequently complicated with pneumonia than with bronchitis—another proof that the degree of inflammation present is very intense, and to be compared with that which exists in the web of the frog's foot around the puncture of the red-hot needle. And my experience of the effects of the solution of caustic in cases of croup, justifies this comparison, and confirms the important inference that such treatment is unsuitable for the degree of inflammation present in them; for I have always found the symptoms of congestion in the laryngeal lining, such as pain and difficulty of breathing, increased by the application of even a weak solution of nitrate of silver, and the very act of applying the solution is hurtful in these cases; the sponge generally brings away part of the false membrane upon it, and leaves the delicate and highly vascular tissue beneath exposed, and often bleeding. Pain, anxiety, and fear, increased exudation, and sometimes ulceration, are thus produced, the original disease being thereby aggravated.

To show that I have not been too easily led to these conclusions, and to give force and point to what I believe to be a very important statement, I shall relate the two following cases, which I venture to think interesting in many points of view, and which I have, therefore, selected from my case-book. One of them illustrates the action of the topical application in an adult case, and the other in that of a child.

Case 1.—The subject of this case was a gentleman past the middle period of life, and before the illness which I am going to describe, particularly strong and healthy.

One evening of the winter before last, he was suddenly seized with difficult respiration, tightness in the throat, harsh, dry, whistling cough, and high fever. All the symptoms of croup, indeed, became very soon but too well marked; and, a few hours after the apparent commencement of the attack, the following were the physical signs which presented themselves:—The number of respirations in the minute was much increased, and yet the feeling of oppression on the chest remained unabated, so incomplete was the inflation of the lungs; indeed the respiratory murmur was but feebly heard in the upper parts of the chest, while the bronchial sounds were dry and snoring in their character. In the trachea the inspiration was long, and accompanied by the harsh sound of the air passing along the dry and narrowed tube. A little higher up, and chiefly at the commencement of inspiration, the glottis was heard vibrating so as to occasion a stridulous sound.

When the patient spoke he suffered great pain, and increased feeling of anxiety. His voice was feeble and broken, being at times deeper, and then suddenly slighter, than his ordinary tone.

There could, therefore, be no doubt that this was an instance of acute tracheal croup, accompanied by exudation. It was treated as such, by emetics,

purgatives, hot baths, bleeding, antimony, and calomel, with a blister on the trachea, and in the evening I commenced to apply a solution of twenty grains of nitrate of silver in an ounce of water to the interior of the affected organ; but each application gave great pain and uneasiness, and increased the sense of suffocation. The violent fits of coughing which were thus produced undoubtedly occasioned the separation of small portions of the false membrane, but that was no improvement, since the surface thus exposed was tender, unprotected, and often bleeding. I next followed Dr. Horace Green's example, and increased the strength of the solution twofold. This, however, only made matters worse, and indeed the patient himself began to dread the repetition of the proceeding. Still my faith in the remedy was not completely exhausted; I determined, before abandoning the topical treatment altogether, to use a weak solution of the nitrate of silver; I therefore diluted it to ten grains, and ultimately to five grains, in one ounce of water, and yet I was unsuccessful. It was, indeed, too apparent to me that the larynx was not in a state to bear either the stimulant solution, or the presence, for however short a time, of the sponge by which it was applied; I therefore gave up the topical treatment entirely at this time, and used more ordinary measures. The patient was still further depleted, and more decidedly mercurialized. He was likewise frequently blistered during the next month, by the end of which time he was much improved, but still had a good deal of hard, whistling cough, dyspnœa when he moved about, and great pain when he spoke, referred to the glottidean region. The tone of voice was weak, but not unusually hoarse.

The most careful examination of the chest still showed that the lungs were free from disease. The respiratory sounds in the trachea were loud, hash, and dry, and were accompanied by a pretty constant rale, as if there were one or more valvules of exudation matter still adhering to the walls of the trachea. The vibration of the glottis in breathing and coughing was not so free as formerly, indicating a degree of œdema of the organ.

On opening the mouth, the fauces were seen to be red and swollen, and the epiglottis was felt by the finger covered with soft and doughy mucous membrane.

After careful consideration of all these circumstances, it was determined, in consultation with my father, that two caustic issues should be opened, one on each side of the thyroid cartilage; that the iodide of potassium should be administered in decoction of sarsaparilla; and that I should again apply the caustic solution to the interior of the larynx and trachea, now that the inflammation had passed the acute stage. Under this plan of treatment the patient made daily advances towards health, and was soon able to take exercise out of doors wearing a respirator.

The effects of topical treatment during this latter period were as manifestly beneficial as formerly they had been hurtful. The strength of the solution was at first only ten grains to the ounce of water, but was gradually increased to a scruple in the same quantity. After each application the patient found that in a short time his breathing was freer, his cough less frequent, and his voice stronger; but this improvement at first lasted only about forty-eight hours, at the end of which period the application was always renewed with the good effect of sustaining the improvement. By-and-by, however, the intervals were lengthened with impunity; the gentleman spent the summer at the coast, and is now perfectly well.

I think it worth mentioning, in conclusion, that I still see this gentleman occasionally for a feeling of dryness in the throat, which nothing but the stimulent application seems to relieve. This appears to be a very common state of matters after the caustic solution has been applied to the throat for any length of time. Hence I now and again see a number of my old laryngeal patients who have been cured of all their symptoms with the exception of this feeling

of dryness, and it is often both intense and annoying. I believe the most of them would disregard it, were it not for the fear of a relapse into their former state, and I therefore encourage them to forget it if possible, and to use such means as rubefacient liniments externally, or some simple gargle. Still it must be confessed that no remedy for this disagreeable feeling is so effectual as the solution of caustic, and if it be not too strong, and if the intervals of its application be not injudiciously short, I do not think its continued use in the cautious manner just indicated will do any injury to the mucous membrane.

But to return from this digression; the case which I have narrated proves, as clearly as any single case can prove, that the topical treatment is unsuitable during the acute stage of exudative croup: and, were it not for the inconvenient length to which it would protract my paper, I could relate many others, the subjects of which were children, and which all go to corroborate the above conclusion. I have in these cases invariably seen good reason to stop the topical treatment if I had begun to use it early in the disease, because I found that it retarded, if it did not prevent, then favourable progress. One of the most marked of the cases to which I allude was the following, and I relate its chief features here, because it is the last case of acute exudative croup in which I have used, or intend to use, the solution of caustic.

Case 2.—The patient was a girl, four or five years old, attended by my friend, Dr. Peter Stewart, of Eglington-street.

This little patient was suddenly seized with symptoms of acute croup, about the middle of last winter. Dr. Stewart was immediately called in, and at once instituted the most judicious measures to check, if possible, the untoward progress of the malady. Among other things he applied a solution of caustic to the pharynx and upper part of the larynx.

Unfortunately, however, as sometimes will and must happen, under the best treatment, the patient's state rapidly became worse, and Dr. Stewart requested a consultation with my father; and a doubt occuring to the former that possibly he might not have passed the probang fair into the larynx, he likewise asked me to see his patient and apply the solution of caustic for him.

The child had been about forty hours ill when I saw her, and was evidently in a most dangerous condition. The exudation was very abundant in the trachea, as evinced by the sharpness of the respiratory sounds heard over it, and by the faintness of the vesicular murmur in the lungs; the glottis, however, vibrated during coughing and speaking, and was therefore, free of œdematous swelling. The pulse was quick but not feeble; the surface of the body was hot and moist, and the face was of a dingy hue, the lips being almost livid.

I introduced the probang, the sponge of which had been moistened with a solution of twenty grains of caustic in an ounce of water, so easily through the rima glottidis, that I feel quite convinced that Dr. Stewart, who is in the daily habit of using this plan of treatment in many other cases, especially in hooping-cough, had likewise reached the seat of disease, and that there had been a fair trial of the topical treatment in this case from the commencement. I repeated the application thrice during my first visit, and Dr. Stewart renewed it again in the evening. At my second visit, next morning, I used a stronger solution, viz., one of forty grains to the ounce of water. After each application the child seemed a little easier, perhaps from the passage being partially cleared by the sponge and by the child's own efforts, but she always became worse in a very short time; and although all the ordinary means had been used during the whole progress of the case, besides the topical measure, still the child's state was evidently becoming very hopeless. The pulse was more rapid, but not as feeble as might have been expected, and the colour of the skin was much more dingy—indeed it was almost lived. The child died that evening, and I regret to add, that no inspection of the body was permitted.

In this case the failure of the topical treatment was far too marked to occur in any one's practice without exciting very serious reflections regarding its employment in the disease of which it was an example, and it led me to look back to my notes of other similar cases, as well as to institute some such experiments as that which I formerly related. The results of these observations and reflections have been to convince me of the total unsuitableness of the treatment in question to acute cases of exudative croup.

But I may here be met with the objection, that it, in my cases of croup, the topical treatment was unsuccessful, a very different result ensued in Dr. Horace Green's cases. This impression, however, will not, on examination, be found to be so correct as many may be inclined to think. Nor is it, in my opinion, detracting from Dr. Green's merit to hold that it consists in having effectually directed attention to the general subject of topical applications to the interior of the larynx rather than in recommending that treatment in cases of croup.

Dr. Horace Green illustrates his little work on croup by thirteen cases. He may possibly refer to others throughout the work, but these are the only examples fully related, so that they can be judged of independently by the reader; hence they are carefully numbered so as to permit of easy reference. Of these thirteen cases, two are quoted from Mr. Ryland's work on the larynx, chiefly for the sake of the account given by that author of the morbid appearances after death. In these, of course, the topical treatment was not used, so that the cases which illustrate this treatment given by Dr. Green are reduced to eleven. Nor am I convinced that these were all cases of true exudative croup; nay, I think it is certain they are not; for No. V. was a mere hoarseness, and No. VIII. was a spasmodic affection of the glottis which came and went without any symptom of croup at all. Nos. II., VII., and X., were apparently cases of acute œdema glottidis, leaving only six cases, the symptoms of which resemble those of croup. Even some of these six have more the characters of diphtheritis than of croup; and in one of them (No. XIII.) the affection followed measles. In only four of the six cases was the disease fully developed, and of them one-half died. But, supposing that all the eleven cases related in this book were really cases of croup, more or less severe, I do not think the mortality among them, viz., three deaths in eleven cases, was less than it generally is in the ordinary run of croupy cases occurring in the better ranks of life, and treated in the usual way; and therefore it follows that Dr. Green's experience, so far as we have it in his work on croup, does not show that his success in the treatment of cases of that disease was increased by his using the topical applications to the interior of the larynx; for he very properly used other measures as well, and the result has been a mortality not at all less than it he had neglected the topical treatment altogether. I consider it no small corroboration of my opinion, in regard of this point, that M. Trousseau states in the 'Union Medicale' for 1851, No. 100, as one reason for his superior success of late years in the treatment of severe cases of croup, that he has discontinued the application of a strong solution of caustic to the larynx and trachea, which he used formerly to insist upon.

The termination of acute inflammation of the laryngeal mucous membrane, whether that inflammation had been of the simple or of the exudative type, in œdema of its loose subjacent tissue, is an event so remarkable and important that I have reserved until now the few remarks which I wish to make on its topical treatment. I believe that the occurrence of the lesion referred to is by means infrequent, and that it is always attended with imminent danger to the patient's life. The rapidity with which the inflammatory stage sometimes terminates in this manner is sufficiently remarkable to have struck every one who has observed cases of the kind. In some of these it is the result of constitutional debility, however that may have been produced; while in others it

seems referrible to a peculiarity in the nature of the morbid process itself. In the former class of cases it generally arises during the progress of some exhausting disease, such as typhus fever, or towards the end of exudative croup itself, when it is always a formidable and often a fatal complication. And even when it occurs as a more primary disease, the inflammation of the mucous membrane appears to be reduced in intensity by the very occurrence of the serous effusion, although it had previously been even of the exudative type. From what has been formerly stated, then, regarding the action of a solution of caustic applied to a subacutely inflamed mucous membrane, it might, *a priori*, be expected to produce a beneficial effect on the œdematous glottis; and this expectation has been remarkably fulfilled in my experience, as the following instance will sufficiently exemplify.

Case 3.—A young child, of eight months old, had severe hemorrhage from the gums after division of them over the incisor teeth, and in the exhausted state which followed, he caught cold, and became affected with the ordinary symptoms of croup, which were chiefly combatted by an emetic, counter-irritation over the throat and chest, and by repeated small doses of calomel. But very soon the chief, nay, only symptom became that of impeded respiration. The child's efforts during inspiration, the dry, whistling sound which accompanied it in the trachea, the nearly total absence of vesicular murmur in the lungs, and the short expiratory sounds, taken along with the previous state of the little patient, rendered it evident that œdema glottidis had occurred; and if to this it be added that the pulse was feeble, the patient pale and exhausted, and that he could hardly be made to receive nourishment,—his extreme danger will not be questioned.

I introduced the probang down to the glottis, but not through the rima, owing to the swelling of its margins. The strength of the solution used was thirty grains to the ounce of water, and it was applied three or four times at short intervals. The effect was soon apparent. Some coughing, and the expulsion of tough muco-albuminous matter first followed, and then the child became quiet; the breathing was freer, although, of course, there was still considerable obstruction at the glottis. In a few hours, this obstruction seemed to be increasing, and the application of the caustic solution was again renewed in the same way, and with equally favourable results. The calomel was continued, and a warm water enema was administered, after the action of which the child took the breast, and slept for a short time. The future progress of the case was marked by a gradual but steady improvement. The calomel was soon stopped, the bowels were duly regulated, and the topical applicants were persevered in daily for two or three weeks, by the end of which time all obstruction to the breathing, as well as the cough, and even a degree of hoarseness which had latterly been observed, had completely disappeared; and the child's general health rapidly improved.

[In another case of an infant, only two months old, to which Dr. W. was called, the symptoms at first appeared only those of a slight cold, but which gradually assumed a frightful degree of intensity. Dr. Watson touched the glottis with a strong solution of caustic, which assisted in expelling a quantity of ropy mucous and relieved the respiration. After a purgative enema, and a tepid bath, the touching was repeated with manifest improvement; only four or five repetitions were necessary, and the child was well in a few days. Dr. Watson then continues:]

On reviewing the whole subject, then, the following are the principal conclusions to which my observations, experimental and clinical, have conducted me:—

1st. The solution of the nitrate of silver, when applied to an inflamed mucous membrane, acts differently, according to the intensity of the inflammation that may be present; in the asthenic varieties it operates as a stimulant of

the capillaries of the part, and likewise of its secreting apparatus, while in the sthenic variety it increases the congestion of the membrane, chiefly by diminishing the fluidity of the blood in its vessels.

2nd. In acute laryngitis, in which there is no false membrane, and probably in diphtheritis, in which there is an albuminous exudation, the local application of solution of caustic, varying in strength inversely in proportion to the intensity of the inflammation, may be employed with more or less speedy benefit.

3rd. During the violence of true exudative croup, the stimulant application to the part affected is injurious, but when the disease begins to yield to antiphlogistic and other treatment, it may assist in the cure.

4th. Œdema glottidis, whether occurring as a primary disease, or as a complication of other morbid states, is always speedily relieved, and in some cases effectually cured, by the application of strong solutions of the nitrate of silver to the œdematous organ.

And 5th. It follows as a corollary, derived partly from the forgoing conclusions, and partly from the results of my experience of the topical treatment communicated to 'The Dublin Quarterly Journal' in November, 1850, that the solution of caustic acts beneficially in only one, viz., in the asthenic variety of laryngeal inflammation: for it matters not whether such has been the original character of the affection (acute but not asthenic cases), or whether it has become so under the combined influence of time and general treatment (chronic cases).—*Dublin Quarterly Journal, August,* 1852. *p.* 48.

ON FATTY ENLARGEMENT OF THE LIVER.

By Prof. Bennett.

Fatty liver is now well known to depend on the secretion of a large quantity of oil, which is stored up in the hepatic cells. These cells are under such circumstances frequently enlarged, and contain oil varying in amount from a few granules to a large mass, which occupies the whole of their cavities. Not unfrequently livers, which to the naked eye appear healthy enough, may still be demonstrated with the microscope to contain an unusual number of fat granules, and there can be little doubt that considerable variations may exist in this respect quite compatible with a state of health. Almost all stall-fed animals that do not labour, possess a large amount of fat in their hepatic cells. Is is only where the organ is much enlarged, altered in colour, and presses upon neighbouring viscera, that its fatty degeneration can be said to interfere with the vital processes.

In man, fatty degeneration of the liver has been observed to be very common in two kinds of cases—1st, in drunkards; 2nd, in persons labouring under phthisis pulmonalis. Drunkards are continually taking alcholic liquids, which abound in carbon, and which being too large in amount to be excreted from the lungs as carbonic acid, and from the liver as bile, is stored up in the latter organ in the form of fat. In phthisis pulmonalis, the excretory power of the lungs is diminished, and the excess of carbon in the tissues and food is thrown upon the liver to be excreted. Under these circumstances, it is converted into fat and stored up in that organ.

The manner in which the livers of geese are prepared in Strasburg, is by following a process somewhat similar. They are confined in close cages, in a heated atmosphere, and largely supplied with food. Want of excercise and heat

diminish the respiratory functions, and cause that of the liver to be disordered, and the result is enlargement of the organ from accumulation of fat. In the case before us, such exactly seems to be the cause of the disease. A man is kept stationary watching a steam-engine, in an elevated temperature, whilst he is consuming his usual food, and exceeding in alcholic drinks.

This view, however, has been objected to on the following grounds:—1st, That the connection between fatty liver and disease of the lungs is not general; 2d, That there is no evidence that a fatty liver does not excrete bile as usual; and 3d, That as a considerable portion of bile is absorbed into the blood to be excreted from the lungs, the liver must be considered as preparing material for these organs. Hence it is argued that it would be a strange compensation if the functions of the liver were to be increased, while that of the lung is diminished by disease (Budd.) But if fatty liver be not always conjoined with diseased lung, it will be found associated with some circumstance which diminishes the function of that organ, in relation to the work it is called upon to perform; for instance, the separation of carbon from the alcoholic fluids taken by the drunkard. Again, want of exercise from various diseases, and especially phthisis, whilst, in order to support the strength, wine and nutritious diet are given liberally, may frequently be seen to be the cause of fatty liver. Further, although it be granted that the liver may in health prepare carbonaceous matters for pulmonary excretion, it must be clear that if the lungs cannot accomplish this function, such matters must be thrown back or retained in the liver, and constitute a powerful cause of fatty degeneration of that organ. On the whole, therefore, we must regard excess of carbonaceous matters in the system, and the diminution of pulmonary action, as the chief cause of derangement in the functions of the liver; a view which has the merit of pointing out to us as remedies a diminished diet, a temperate climate, appropriate exercise, and an endeavour to promote the functions of the lungs and skin.

There is another structural alteration of the liver, which, from the colour and general appearance so like bees' wax it assumes, has been called "waxy," and sometimes "brawny," liver. This disease has been confounded with fatty liver, although an examination of its minute structure will show that the hepatic cells present a very different character. Instead of being enlarged and filled more or less with oil globules, they are colourless, shrunken, and for the most part destitute of contents, while the nucleus has disappeared. The lesion seems to me to be a further stage of the fatty degeneration, in which the oily matter is absorbed, and the cell-walls are left behind and aggregated together; but further researches are required to determine this point.—*Monthly Journal of Medical Science, August*, 1852, *p.* 164.

ON CIRRHOSIS OF THE LIVER.

By Professor Bennett.

This morbid change in the liver consists of hypertrophy of the fibrous element between the lobules of the organ and its subsequent contraction, whereby its volume is diminished, and the secreting cells compressed and atrophied. As a further result the large nous trunks are also compressed, and their com-

mencing ramifications so congested that effusion into, or dropsy of, the peritoneal cavity is induced. The nutmeg liver is an incipient condition of cirrhosis, in which the portal system of vessels in the organ is congested. In both conditions, the hepatic cells are more or less fatty and atrophied. The fatty degeneration in nutmeg liver may be seen to commence at the circumference of the lobules, whereas in the advanced stage of cirrhosis, all the cells are more or less diseased, some loaded with fat, and others with yellow pigment. Notwithstanding the great organic changes which are frequently observed in this disease, danger is not so much to be apprehended from interruptions in the functions of the liver, as from the ascites induced by the constriction of the large abdominal veins, which, by distending the abdomen and compressing the lungs and liver, so interferes with those important organs, that death is occasioned.

The treatment in cirrhosis must be purely palliative, and directed to diminishing the ascites, by means of diuretics and diaphoretics. The question of drawing off the fluid by paracentesis is one which may arise, in case the swelling is very great, and the embarrassment to the pulmonary and renal organs, extreme. Even then, although temporary relief may be obtained by the operation, there is every reason to believe that, in the majority of cases, life is in no way prolonged.—*Monthly Journal of Med. Science Aug.*, 1852, *p.* 166.

NEW METHOD OF PERFORMING TRACHEOTOMY.

By Dr. C. Gerson.

For the performance of Tracheotomy, Dr. Gerson has contrived an instrument consisting of three moveable branches, which join at the end, so as to form a sharp point, and can be separated by means of a vice at the other extremity of the cone. By turning the vice from left to right, the branches diverge and form a cone, of which the base is turned towards the wound, and which thus resists the tendency of the cartilages to expel it from the aperture.

In operating, an incision of two or three *centrimètres* (four-fifths of an inch to one and one-fifth) is made through the skin, the veins are put aside, and the incision, gradually diminishing in length, is continued until the space between two of the cartilaginous rings can be distinctly felt with the nail of the fore-finger. The trachea is then fixed; and the instrument is glided along the nail of the fore-finger, and is made to penetrate into the space between the rings for about three or four *millimètres* (about one-seventh or one-ninth of an inch). An expansion about a quarter of an inch from the point, prevents the instrument from penetrating too deeply. The instrument being held steadily, the handle of the vice is now turned, and the branches of the instrument caused to diverge. When the opening is sufficiently wide to allow the canula to pass between the branches of the instrument, it is introduced into the trachea. The loss of blood is inconsiderable; and the air escapes with so much force, that it would expel every drop which might be inclined to enter the bronchi.—*London Journal of Medicine, October*, 1852, *p.* 982.