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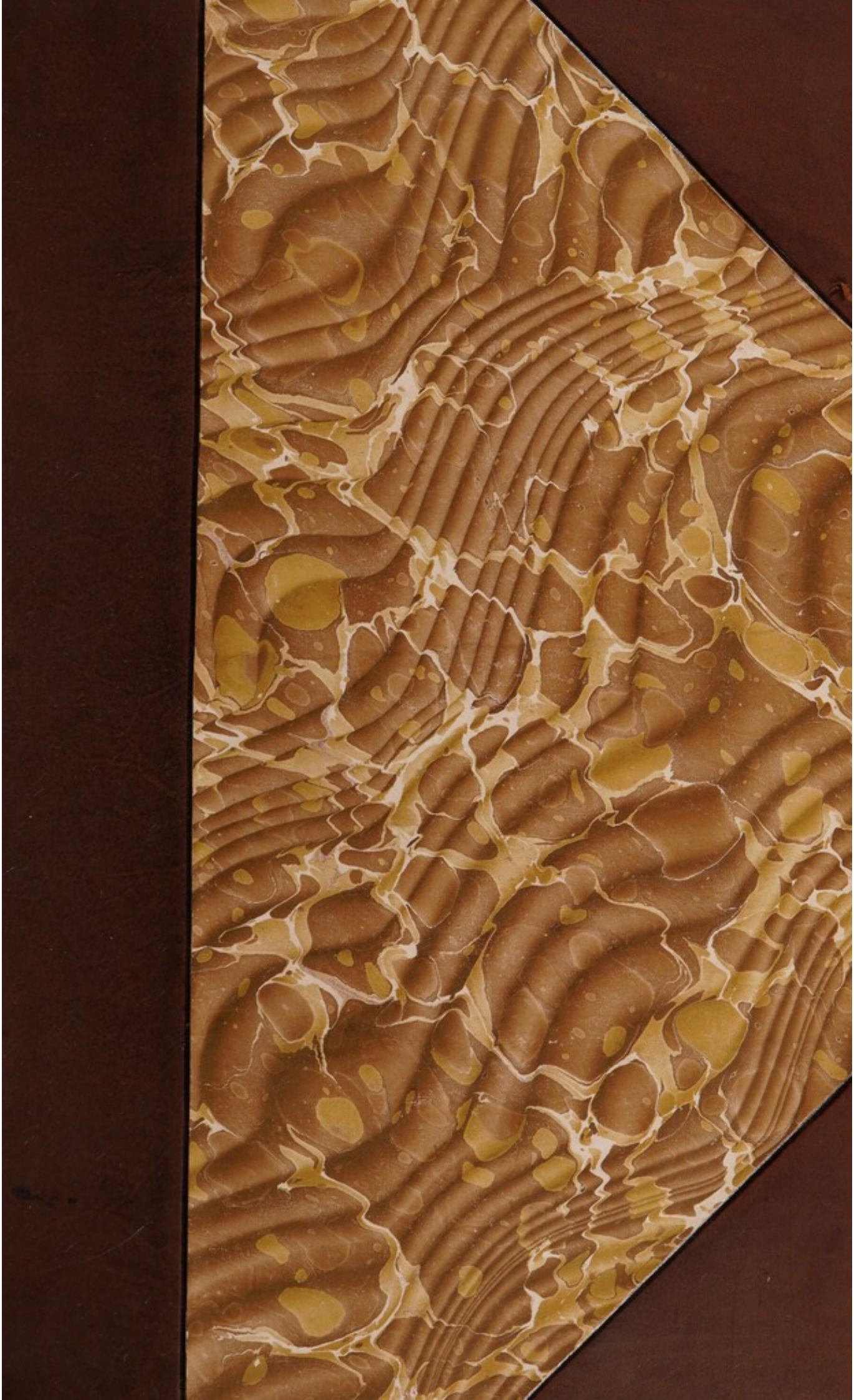
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LECTURES

ON

ANATOMY, SURGERY,

AND

PATHOLOGY;

INCLUDING

OBSERVATIONS ON THE NATURE AND TREATMENT

OF

LOCAL DISEASES.

DELIVERED AT ST. BARTHOLOMEW'S HOSPITAL.

BY JOHN ABERNETHY, F.R.S.

SURGEON TO ST. BARTHOLOMEW'S AND CHRIST'S HOSPITALS,

ETC. ETC. ETC.

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The dissemination of Lectures delivered by public Teachers having been proved to be beneficial rather than inimical to the interests of the professors themselves (the value to the student never having been disputed), has induced the printing of the present volume, with a view to the accomplishment of the latter object, namely, the interests of the medical student, and the propagation of knowledge generally throughout the profession.

The estimation in which the opinions of the celebrated Teacher in the School of St. Bartholomew's is held, must render the present volume invaluable to the student, while it will equally serve as a work of reference to the elder branches of the profession, containing, as it does, the views and opinions of one whose life has been spent in instruction, and whose talents and acquirements are no less admired than respected.

Nor is the value of the volume confined either to the medical *tyro* or the more experienced practitioner; the popular reader, from the plain, lucid, and colloquial style of the Lectures, will derive both pleasure and instruction from the perusal; while the general contents yield a body of information not to be met with in works of this description, and which will render it, in families, a book of familiar consultation and reference.

LECTURES

ON

SURGERY, &c.

SURGERY and Medicine are essentially, what the French Republic was declared to be, " one and indivisible." Custom, however, has separated them into two parts, and I have now to speak of those diseases of which surgeons undertake the management. These are, for the most part, external injuries, such as are the result of accident, or such as require operations; but you will find these external diseases often arise from constitutional causes, and where a disease arises from the state of the general health, you must remove the cause before you can cure it. Again, when they arise from accident, you will find that local disease will disturb the whole constitution; consequently, if I were to begin to speak of local disease, in the first instance, I should be perpetually adverting to the general system, and it will be better therefore that I should first direct your attention to the nature of the constitutional affections which are produced, or which may be produced by local diseases. These constitutional affections having been previously considered, the subject of local diseases will be rendered simple, and a great deal of repetition will thus be saved. I may truly affirm, that this view of the subject was never clearly set forth until the time of Mr. HUNTER; he was the first surgeon who treated of the constitutional symptoms produced by local disease, and no one who pretends to study surgery should omit reading, and endeavouring to understand, his work on this

subject; I allude to the Treatise on the Blood, Inflammation, and Gun-shot Wounds.

I begin with this proposition.

Local disease, injury, or irritation may affect the whole system, and induce general disorder—characterized, however, by more especial disturbance of some parts of the system.

Thus, local disease, injury or irritation, may induce, as its immediate consequence, pain, sickness, swooning, rigors, shivering, convulsions, delirium, or disturbed state of mind, tetanus; or it may occasion various febrile affections. Now, though I think we must consider the first set of symptoms, namely the nervous ones, as the more immediate effects of local irritation on the system, still it will be most convenient to consider the febrile affections in the first instance. These febrile affections are some of an inflammatory character, some of a more chronic kind, and some attended with great debility; they are, in short, fevers exactly like those which Dr. Cullen has described under the heads of Synocha, Synochus, and Typhus; and no physician can distinguish them from those which occur without any external local injury. I remember the time when physicians were rather wroth that a surgeon should attempt to describe any fever at all: they were inclined to say, "What do you know of fever? this is general inflammation which you meet with?" But I will venture to assert, that the fevers produced by local disease are the very identical fevers which physicians meet with where there is no external injury. This is a very curious circumstance, and very illustrative of medical science, as you will find hereafter; but I will not at present dwell upon it, but proceed to the description of these febrile affections. Mr. Hunter, to distinguish and appropriate them as objects of surgery, calls them symptomatic fevers, and he first treats of the symptomatic inflammatory fever.

Now, I always wish you to examine the subject for yourselves; do not pin your faith on any thing which I, or any body else, shall say: you should be, *Nullius addicti jurare in verba magistri.* Examine all things for yourselves, and I will lead you to do so, by stating such facts and circumstances, as may furnish you with data for the exercise of your own reason and judgment. I will suppose, that a healthy subject—which I do not think is to be found in London—has met with a compound fracture of the leg. When the limb has been set, and the patient is tolerably quiet, if you examine him,

you will find that he is unwell, and that he has, in short, the disease of which I am about to give a description. There will be an unusual excitement of the whole system, characterized by the following circumstances: First, with regard to the sanguiferous system, you will find that the pulse is increased in all its attributes; it is more strong, more full, more firm, and more frequent than usual. With respect to the secerning system, which is so closely connected with the sanguiferous system, you will find that the secretions are diminished; the urine is scanty and high coloured, the skin is dry and rather hot, the tongue is dry and white, the bowels have a tendency to costiveness; in short, it seems, that all the secretions are diminished. With respect to the nervous system, it is evidently excited; the man is vigilant; he does not sleep, or if he forgets himself, and dozes for a short time, he wakes suddenly in agitation and alarm; he has a sort of consciousness of strength, the pupils of his eyes are contracted, and there are, in short, all the symptoms indicating an excited state of the nervous system. In taking a physiological view of disease, it is necessary to consider the principal organs of the human body, and to show the effect of disease upon them severally and conjointly. With respect to the absorbent system, I have nothing to observe as to its being affected under these circumstances; but there is a system appertaining to the human body, which may be termed the respiratory system, which partakes of the general constitutional disorder. The disorder of the stomach and bowels is indicated by a white and dry tongue, defective appetite, thirst; if there be any disposition to take food, it is of the vegetable kind, and acids.

With respect, however, to this disordered state of the digestive organs, I shall treat it as a separate and distinct subject; and I shall now proceed to advert to the varieties of the

SYMPATHETIC INFLAMMATORY FEVER.

I should have observed, however, that if blood be taken from the patient in this disorder, it will not be in its ordinary state; it coagulates more slowly than usual, the crassamentum is pale on the surface, buff-coloured and cupped; it has the character, in short, of inflammatory blood. If vital parts are injured, you will not find the same bold action in the sanguiferous system, the pulse is not so full and strong, but it

is more frequent, and seems, as it were, oppressed. The nervous system is more disturbed when vital organs are affected; when the patient is bled, you will find the blood strongly inflammatory, and the pulse seems to rise upon bleeding. This is the only variety of sympathetic inflammatory fever which I need particularly notice: and having given you the description of the symptoms, I shall proceed to what, in the language of the schools, is termed the *ratio symptomatum*, or the explanation of the nature of what they are pleased to call the symptoms. But, hang it, I need not trouble you with this; the symptoms explain themselves; they are the natural consequences of the excitement of the heart and arteries, and the disturbed state of the digestive organs. I may go at once then to the treatment. Treatment! there is no treatment. The disease is the necessary and natural consequence of the injury; it must inevitably take place: you may mitigate, but you cannot cure it. What must you do then to mitigate the affection of the sanguiferous system which is called fever? It is evident that there is too much action; you must try to lessen it then: how? Why, by taking away blood. Blood is the natural excitement of the heart and arteries, and if you diminish the blood, you diminish the excitement. But in taking away blood you rob the patient of a vital fluid; consider what you are about; recollect that you cannot cure the disease, and take care that the course you adopt may be calculated to mitigate it. I will still put the case of a man with a compound fracture, where considerable constitutional vigour is necessary for the reparation of the injury. Do not take away his blood which is his life, for you may find, after a certain time, that he will stand in need of every degree of vital energy to recover from the injury. I have seen a patient bled, and bled, and two or three days after, the medical man has been glad to throw in the bark, and try every means, when it was too late, to give strength and vigor to the constitution of the patient. Your only warrant for bleeding is, that the action of the fever may perchance induce greater debility than the loss of blood. Action wears out strength, and if by lessening action you can save strength in a greater degree than it will be impaired by the loss of blood, then you are warranted in bleeding. The fever so uniformly subsides on a discharge taking place from the injured part, (that is, in three or four days, when suppuration is established,) that it was long known by the name of

the suppurative fever. That the sympathetic fever, however, arising from local injury is not essential to suppuration has been shewn by Mr. HUNTER; and indeed it is so self-evident, that I need not dwell on the point. If I were to say every thing which might be said on every subject which arises, I should weary my audience extremely, and I therefore confine myself to those points which appear to me to be of moment.

As the fever subsides spontaneously on the third or fourth day, when suppuration takes place, avoid bleeding the patient, if possible. Give him diluent drinks and acids, such as lemonade, &c., for which he has a desire. It is quite right that a person who has met with a bad accident should have his bowels cleared out in the first place, for the accumulation of fæces in a state of confinement would of itself be enough to bring on a sort of fever. The bowels should be kept gently lax; nothing is more tranquillizing to the system, and it is itself a mode of depletion; for by promoting the secretions, you lessen the plenitude of the blood-vessels. This is a fact which those who have not studied anatomy and physiology cannot be so thoroughly acquainted with as those who have; purging is depletion, and lessens the plenitude of the blood-vessels without inducing the same degree of weakness as bleeding. Inducing general perspiration has the same effect, by promoting secretion from the superficial vessels of the body. I hold it to be an axiom in the treatment of all diseases, that we should endeavour to put to rights what is wrong. Deficiency or paucity of the secretions is wrong, and we put that to rights by bringing the secretions up to the ordinary standard. With this view, antimony may be given in small doses to restore the general perspiration, and in some degree to deplete at the same time the sanguiferous system. In short, give your patient vegetable food, with toast and water, and other diluent drinks; keep his bowels gently lax; and administer saline medicines, with small doses of antimony. I will add a word or two to prevent any surprise which may be felt at the observations I have made with respect to bleeding. It may be said that patients are sometimes bled, and bled largely, and that this profuse bleeding appears to do them good. Undoubtedly there are cases in which we are obliged to bleed most profusely, but this is not on account of the fever, but the cause which produces it. If a man has been shot through the body—if an internal and vital organ is injured and inflammation comes on in

that organ—the pulse will not be so full and strong, but it will be very frequent, and on bleeding the patient, the blood will be found cupped and highly inflammatory. The pulse will rise on bleeding, and the patient will be in some degree relieved; the inflammatory symptoms, however, will return, and you bleed him again and again, until you diminish the inflammation of the vital organ. In such cases, the patient can only be saved by the most resolute conduct on the part of the surgeon, in pursuing a mode of treatment, which would otherwise appear most outrageous. You must either let the inflammation of a vital organ kill the patient, or run the hazard of killing him yourself with the lancet. In such a case, a surgeon is justified in adopting the alternative; it is, in fact, his duty to say, "I will rather be the executioner myself than suffer the disease to kill the patient." It is only by the most determined conduct, that the inflammation of a vital organ can be subdued. We do not, however, bleed here for fever, but for the inflammation, which if suffered to continue, will injure irrevocably, an organ essential to life. It is really very curious to know how a patient may be bled under such circumstances. I have lived in London all my life, and am very chary of taking patients' blood, but still if some were to see how I would bleed a person having inflammation of a vital organ, they would wonder how the same person could act so differently from his ordinary practice. In such cases, you should bleed the patient upright, till he faint: put a stop to all action of the heart and arteries for a time, and when this is renewed, bleed him again till he faint; open veins in both arms, nay, open more vessels if necessary, for it is by such resolute conduct alone that you can save the patient. Taking a little blood morning, noon, and night, in the horizontal position, will never do in inflammation of a vital organ; you must subdue all vascular action, by the determined course which I have described to you. The inflammatory state of the blood will be your warrant for bleeding, and when you have taken as much blood as you dare from the general system, you should still apply leeches and counter-irritants. If I were to pursue this subject, however, I should deviate from the main object of this Lecture, which is to treat of sympathetic fever. Let us keep to the same case of the patient with the compound fracture:—at the end of three or four days, when the wound begins to discharge, how different is the state of this man. When suppuration is fairly established

what is the state of his pulse? It is indeed more frequent than natural, but it is neither hard, nor strong, nor full. What is the state of his secretions? his skin is moist, and is apt to break out in sweats from slight causes, such as taking food; after sleeping also, he wakes in a perspiration. What is the state of his urine? it is more copious, and sometimes even pale. What is the state of his tongue? it is moist and clean. What is the state of his bowels? there is no longer any tendency to costiveness. What is the state of his nervous system? he is languid, disposed to sleep, and appears conscious of weakness. What shall we say of this man? It is evident that he is not well, but we cannot presume to define the nature of his illness: it is the natural result of local irritation on the system. All these symptoms may be increased in degree, and they will then assume the character of

HECTIC FEVER.

There will be a frequent pulse without strength or hardness; morning and evening sweats, such as are described in the Nosology of Cullen, who has, I believe, taken his description from pulmonary hectic. The lax state of the bowels will be increased to copious purging, and as the patient gets weaker and weaker, you will observe that hard, odd state of the skin, which, to use the expression of Mr. Hunter, looks as if the blood had forsaken it. There is an increased degree of languor, disposition to sleep, and consciousness of weakness. The digestion is imperfectly carried on, and the appetite of the patient declines. Medical men differ as to the nature of this disease; some say that it is hectic fever, others that it is not; and distinctions have been taken between rapid and slow hectic, and I know not what. For my own part, I think it is one and the same fever with that which physicians call hectic, and that it is the natural consequence of the irritation of local disease on the system. I state to you what appears to me to be the simple and real nature of the disease; and you must judge from your own observation whether I am right or wrong. Mr Hunter has given some reasons (and all Mr. Hunter's reasons deserve attention), in support of the opinion that the quantity of hectic fever is proportionate to the importance of the organ in the animal economy, and the want of power, for the reparation of the injury. Thus there is more fever in dis-

ease of a vital organ, than in disease of parts which are of less consequence. So also it will be proportionate to the want of power in the part for repairing the injury; joints, for instance, which have little restorative power, are more likely to produce hectic fever.

I have an additional observation to make: it may be well conceived that there is a very exact correspondence between the importance of the local affection, and the disturbance of the constitution induced by it; but if you look at surgery extensively, it would seem that either the state of the general health will determine the peculiarities of the local disease, or the peculiarities of the local disease will induce corresponding peculiarities in the constitutional disorder. Both of these propositions are probably true. Why local disease produces inflammatory fever; why, during the inflammatory stage, the secretions are locked up, but the moment a discharge takes place from the wound they are renewed; and though irritation is kept up, it is of a very different kind, you will see as you go on, and I shall not therefore dwell upon these points at present. Mr. Hunter says there is a greater feeling of weakness than the actual weakness warrants. Parts, for instance, which seem extremely weak, are nevertheless capable of repairing wounds made by the amputation of a limb. Many a man, labouring under a great degree of hectic, and appearing excessively weak, has been completely relieved by the removal of a limb, and the large wound made by the amputation has healed kindly. There is a greater consciousness of weakness, therefore, than the actual deficiency of strength warrants.

I will go on to speak of what is called the treatment of this fever. It is the necessary consequence of the excitement of the local malady, and if you change that, you will change the fever. What are the indications set down? It is said that you should support the strength of the patient. Aye, unquestionably, you must support his strength, for his constitution is getting weaker and weaker, in consequence of the undue action kept up by the irritation of the local disease. And how are you to support his strength? by giving him bark? This used to be the opinion, but I believe bark is less relied upon now; and for my own part I know of no mode of increasing the strength of the patient, but by increasing his powers of digestion. To me it appears that all strength arises from power of digestion, and if bark increases the power of digestion, then

give it. You are not, however, to *throw in* the bark, as the phrase has been: there is a paper published in the Medical Transactions, by Dr. HEBERDEN and Sir EVERARD HOME, in which they state that they have never seen the bark do any good in hectic fever, unless it has been accompanied by a wound. There are wounds in which the bark is known to be an effectual remedy for a certain morbid condition of them. The bark should be given with a view of increasing the tone of the stomach, and ought not in general to be relied upon as a remedy. Tonics and cordials cannot be given in inflammatory fever without augmenting all the symptoms; but in the fever of which I am now speaking, they may be given in moderation with advantage. They are, indeed, necessary on account of the state of the stomach, and may be given without producing any aggravation of the febrile action. The bowels should be kept regular; the management of the stomach and bowels, however, I shall reserve as a separate subject.

With respect to the disturbed state of the nervous system which obtains in this fever, a question arises whether you may give opium to mitigate it. The answer is, Certainly; it will not do in inflammatory fever; but in hectic, where excited action is connected with irritation, opium may be given to lull that irritation, and diminish the susceptibility of the nervous system. If ever you give opium for this purpose, pay attention to the mode of administering it; nothing is more absurd than to give a night dose of opium, which stupifies the patient and makes him sleep at night, but renders him more miserable and agitated in the day. If opium be given to diminish the susceptibility of the nervous system, in the constitutional affections arising from local disease, it should be administered regularly, in adequate doses, every six or every four hours, so that its effect may be kept up, and no intermission be suffered to take place in its sedative influence. It is necessary, at all times, in giving opium to attend to the state of the bowels, so that costiveness may not be induced.

SYMPATHETIC IRRITATIVE FEVER.

Still keeping to the subject of compound fracture, I shall now suppose that it has gone on badly; that the wound has discharged very profusely, so that in dressing it in the morning you are obliged to sponge up the matter; that it appears flac-

cid and flabby; that there are no granulations; and that every thing, in short, indicates a want of energy in the constitution, to repair the mischief. The patient gets weaker and weaker, but still the surgeon cherishes a hope that a favourable change may take place; when on a sudden, the discharge from the wound, which has been so profuse, ceases altogether, or at least very little discharge remains. The wound becomes dry on the surface, and an inflammation is set up about it, which is called erysipelas. I do not think that it should be so called, but for want of a better name, we will call it erysipelatous inflammation. Corresponding with this change in the local affection, is an equally important change in the state of the constitutional malady. The pulse becomes strong and firm again, there is a burning sensation in the skin, and occasionally considerable perspiration; the urine is again scanty and high coloured; the tongue dry and covered with a brown fur; and there is great excitement of the nervous system, indicated by wandering of the mind, agitation, delirium, picking of the bed-clothes, and subsultus tendinum. This class of symptoms is one of the general effects of local injury, and Mr. HUNTER calls them symptoms of dissolution. I do not exactly understand his meaning. If he means that these are symptoms indicating that the patient is likely to die, this may be granted; they cannot be said to be certain symptoms of approaching dissolution, for patients sometimes recover from them. The disease is as like typhus as possible, and I will undertake to say that if the most skilful physician were taken to the bed-side of a patient labouring under this affliction from a compound fracture, and were asked, after he had felt his pulse, what was the matter with him, he would be unable to distinguish it from typhus fever. You may call it, therefore, if you please, the sympathetic typhoid fever. I am inclined to call it the sympathetic irritative fever, for great irritation, with diminished power of the constitution is its essential characteristic. You will see this kind of fever sometimes occur in the first instance; thus in the opening of lumbar abscesses there will be restlessness, delirium, brown tongue, and all the symptoms of typhus, preceding the hectic fever.

With respect to the treatment of this fever, I must again observe that I know of none; it is the necessary consequence of the state of the local disease, and if you alter that, you will alter the constitutional symptoms; without this you can

do nothing. You cannot cure, you can only mitigate the disease. With this view, attention should of course be paid to the regulation of the bowels of the patient. Cordials, and any medicines that will impart strength to the patient, should be given ; opium should also be administered, to lull the general irritation, and mitigate the severity of the excitement which the local malady imparts to the system. In inflammatory fever, if the cause were removed on a sudden, it is not likely, though there is no proof of this, that the disease would cease. In hectic or habitual fever, which is a teasing excitement of the general system, from the operation of the local affection, if you remove the cause the fever will generally cease. I have seen a patient far gone in hectic, who had no appetite, no sleep, from the pain of local disease, and who was exceedingly reduced ; yet when his limb has been amputated, and he has been laid in bed, egad ! he would sleep like a person who had long been without rest ; when he awoke, it would be only to eat ; and when he had eaten, he would go to sleep again.

The sympathetic irritative fever more resembles the inflammatory fever ; for though you remove the cause, the effect will not cease ; and when the limb has been amputated, the irritation is known to continue. I have seen this happen when surgeons have ventured to amputate in cases where, unfortunately, the operation has been delayed too long. I will mention to you a case which occurred in this hospital. A man had an inch of his tibia fairly knocked out, and the bone was exposed, so that you might put your finger into it. The limb was laid straight, and kept steady. The wound inflamed, and at length granulations began to shoot up from the ends of the bone. There appeared to be every probability that in a few days these granulations would touch and inosculate with each other, when, unfortunately, just at this juncture the man was seized with a violent fit of purging. The consequence was, that in the course of a night the whole of the granulations disappeared. Still hopes were entertained ; the bark was given, and the surgeon who attended him forbore to remove the limb, though there were decided symptoms of the incompetence of the constitution to repair the injury. The third state of the wound took place ; it became dry ; there was a tendency to slough on the surface ; nay, it did slough, and an erysipelatous inflammation was set up around it. The man was delirious ; day after day elapsed, and the sloughing continued, when, as

I was going through the square of the hospital one afternoon, in the Summer time, a dresser came to me and said, "for God's sake come up, there is a man bleeding to death." The sloughing had caused hæmorrhage; a vast quantity of blood surrounded the limb; the man lay like a corpse on the bed, and no pulsation could be felt. Hartshorn had been applied to his nostrils without effect; but I said, "fetch me the amputating instruments; if the man is dead there will be no harm in taking off his leg; and if not, there is no hartshorn like the cut of a knife." I took off the man's limb; the warming pan was then passed over him, burnt feathers and hartshorn were applied to his nostrils, and he began to breathe a little. He was totally unconscious of any operation having been performed; a circumstance which I have witnessed three times in the course of my life. The symptoms of fever continued after the operation, but gradually subsided. Three or four days after the amputation the stump was examined; it was of a greenish aspect, and looked exactly as if it had been made from a dead body in the dissecting room. This man did well, and I remember him a sturdy beggar in the outskirts of London many years after. I mention this case to illustrate the principle I have laid down, that the symptoms of sympathetic irritative fever, differing in this respect from those of hectic, do not immediately subside, though the cause of them is removed.

LECTURE II.

In the last Lecture, I directed your attention to the febrile affections produced by local disease, injuries, or irritation; and I particularly described to you the sympathetic inflammatory, the hectic, and the sympathetic irritative fever. These febrile affections are sometimes of an intermittent character. A regular paroxysm of intermittent fever is not unfrequently produced by local disease, injury, or irritation; this most generally occurs when the irritation exists in the urinary organs. Passing a bougie for the first time, will occasionally lay a

person up with a paroxysm of intermittent fever; in these cases the paroxysm does not in general recur, but the cold fit is often extremely severe, the hot fit so excessive, that the patient is frequently delirious, and the subsequent sweating very profuse. I have known a fit of rheumatic fever brought on by irritation in the urethra, produced by the application of caustic; and the muscles have been so severely affected, that, egad! the patient has not been able to turn himself in his bed for a fortnight. These and other irregular affections you will occasionally meet with in practice; every thing cannot be plain and simple as it is laid down in lectures, where it is necessary to state things simply, in order to render them intelligible, before we consider them in a state of combination.

The effect of local irritation in the production of febrile and other affections is subject to considerable variety, as it may bring on those disorders to which there is a propensity in the general system. A person may be, as it were, on the brink of a disease, which may be brought into activity by his catching cold, or any other trivial occurrence. In proof of this, I will mention a case which I have noticed in the "Surgical Observations:" it is that of a woman who was the subject of cancer, and who thought herself a remarkably healthy woman, though that could not be the case, or she would not have had this disease. She was at least a strong woman, and possessed of great strength of mind as well as of body. Thus she arranged every thing herself for the operation; and after it was performed, she got up, walked to her bed, and laid herself comfortably in it. Soon after she felt an uneasy sense of distention and tenderness in the stomach, a sense of strangulation, globus hystericus, in the throat; and as I was pretty sure that this state of the stomach could only be relieved by producing secretions from that organ, I began to give her small doses of purgative medicine, which were repeated every fourth hour. Three days and nights elapsed before any discharge could be obtained from the bowels; and at the end of that time the bowels began to secrete. During the whole of that time there was no secretion of urine; but as soon as an evacuation was obtained from the bowels, the kidneys began to secrete also, and not before. The evacuation resmebled the stuff that is taken out of a muddy ditch. At the end of a week she had a violent fit of gout in the hands, and afterwards in the feet. Here was an instance of local irritation bringing on a

disturbed state of health, and exciting, as it were, all the morbid propensities of the patient's constitution.

I have stated that as local disease may produce, or be produced by constitutional causes, it was essentially necessary that a surgeon should understand the morbid conditions of the general health, which may be concomitant to the local malady. I stated also, that it may be convenient to treat in the first instance of the febrile affections, although the nervous affections accompanying local disease have the natural priority. I began by saying that local disease, injuries, or irritation, may occasion pain, sickness, fainting, rigors, convulsions, delirium, and other affections of the brain, and tetanus. Many of these affections are sometimes the immediate consequences of local disease, injury, or irritation. Irritation, though attended with an excitement, the nature of which we do not understand, is not necessarily accompanied with pain. The passing of a bougie, though not productive of pain, may occasion a patient to faint away. It does not follow that the state of excitement which I shall call irritation, is productive of absolute pain ; but local disease, injury or irritation, may produce pain. And what is pain ? Oh ! pain is the result of inordinate action of the nerves of the part. I am aware that I weary you by dwelling on these points, but I do not mind that : I persevere, because I think it necessary that these points should be attended to. Pain is the excessive action of the nerves of the part, and it may take place independently of any vascular action. The severest pain does not necessarily imply any thing violently wrong in the part affected. Tic doloureux, a disease of rare occurrence in my younger days, is a nervous pain of a most tormenting kind, where the part affected is perfectly pale, and there is no appearance whatever of inflammation. Pain does not necessarily imply any great or important degree of disease existing in the part, though it is often the precursor of very serious disease. I do not want to persuade you that pain is no evil, but I wish you to see the subject in its true point of view. Excessive pain may be felt without any material disturbance of a man's constitution ; as, for instance, in tooth-ache ; so in gout, a person may suffer the most racking tortures, without having any considerable degree of cerebral disturbance, though the action of the nerves, propagated to the cerebrum, produces the pain.

Sudden sickness is often occasioned by accidents. Mr.

HUNTER thought that the stomach had a direct sympathy with the remote parts of the body. How much it sympathizes with the head, how much disorder of the stomach is capable of affecting the head, every fool knows. Mr. HUNTER, on the supposition that the stomach was directly and immediately affected from the remote parts of the body, called it the centre of sympathies; and if the hypothesis be true, this designation would be peculiarly apt and appropriate. Whether it be true or not, I cannot say; I have not made up my mind on this point, and it would be extremely difficult to do so.

Faintness, which is the consequence of an affection of the stomach producing its effect on the brain, is often occasioned by local injury. There is a curious circumstance with which you will be familiarly acquainted, and which shows the concatenation of causes producing the same general result. When you introduce a bougie into the urethra of a patient for the first time, having placed his back against a wall, as we usually do, you had better look up into his face, and see how he bears it; for though the bougie does not give any actual pain, a patient very often faints on its first introduction. I am always on the alert on these occasions, for I have had several patients drop down upon me; and whenever I see a person getting pale, and about to faint, I lay him gently down on the carpet. He will speedily recover when he is placed in the horizontal position; and his face, which was before of an ashy paleness, will soon become as red as a turkey-cock. The first effect of the introduction of the bougie is the production of sickness; the second effect is a sympathetic affection of the heart, which failing to propel the blood in due quantity to the brain, occasions the cerebral affection: the patient gets light-headed, his face becomes pallid, his muscles are no longer capable of supporting him, and down he drops. I remember, when I was an apprentice, whenever this happened, my master used to say, "Oh, Sir, I am afraid you are faint; pray take a glass of cold water." The glass of water was given him, and what was the common effect of the cold water on a sick stomach? why, the patient would very often puke it up in his face.

With respect to shivering, Mr. HUNTER imputes it in every instance, to an affection of the stomach; it is a symptom of suppuration, which is invariably preceded by inflammation. It also ushers in other changes in local disease; in some instances it precedes ulceration.

Local injury or irritation frequently produces a state of delirium, in which a man is utterly unconscious of his situation; he goes on imagining things, as in a dream, and acting in consequence of such imaginations. Delirium often takes place in consequence of an accident of no very momentous kind; it may occur without fever, or it may be accompanied with that irritative sympathetic which I described to you in the last Lecture, and which is often the "last stage of all, that closes the sad eventful history" of a compound fracture. Delirium seems to be a very curious affection; in this state, a man is quite unconscious of his disease; he will give rational answers to any question you put to him, when you rouse him; but as I said before, he relapses into a state of wandering and his actions correspond with his dreaming. People who are delirious and suffer pain, have generally uneasy dreams; but delirious patients seem often to have undisturbed, and even pleasant dreams. I remember a man with compound fracture in this hospital, whose leg was in a horrible state of sloughing, and who had delirium in this state. I have roused him, and said, "Thomas, what is the matter with you? how do you do?" He would reply, "Pretty hearty, thank ye; nothing is the matter with me; how do you do?" He would then go on dreaming of one thing or another; I have listened at his bedside, and I am sure his dreams were often of a pleasant kind. He met old acquaintances in his dreams; people whom he remembered *lang syne;* his former companions, his kindred and relations, and he expressed his delight at seeing them. He would exclaim every now and then, "That's a good one," "Well, I never heard a better joke," and so on. It is a curious circumstance, that all consciousness of suffering is thus cut off, as it were, from the body; and it cannot but be regarded as a very benevolent effect of Nature's operations, that extremity of suffering should thus bring with it its antidote. I have seen fits of direct mania produced by local disease; only last year a case of this kind occurred in this hospital. I remember particularly inquiring into the history of one man who was seized with a fit of insanity in consequence of a fracture of the leg, with a view of ascertaining whether any of his family had ever been affected with insanity, and I could not find that there were any.

OF TETANUS.

Tetanus is often the result of local disease, injury, or irritation, and it is to this subject that I shall more particularly direct your attention in this evening's lecture. The word tetanus, as most of you are aware, means tension, and is applied to what may be called a muscular affection. It begins generally in the muscles of the jaw, and when it is confined to this part of the body, it is called *trismus*. When the muscles at the back of the neck stretch the head backwards, it is called *opisthotonos*. When the body is bent forward, it is called *emprosthotonos*, and when the tension is confined to the muscles of one side of the body, it is called *pleurosthotonos*. A distinction has also been taken, according to its greater or less intensity, between acute and chronic tetanus. Tetanus sometimes comes on very shortly after the receipt of an injury, but this happens more frequently in hot than in cold climates. It is apt to come on after amputation has been performed on board ship. Sometimes it does not come on until the wound has nearly or entirely healed. Tetanus seems to be chiefly a muscular affection, but you will be naturally curious to know what is the state of the other parts of the body. In the sanguiferous system, we find no particular alteration; when the patient is labouring under the spasms, and shaken by convulsions, we naturally expect to find a considerable acceleration of pulse, and we do find it; but in the intervals the pulse is regular enough, and of the common standard. With respect to the secretions, De Haen has remarked the high colour of the urine, but I have not found the urine of tetanic patients either high-coloured or deficient in quantity. Patients generally sweat when they are agitated by tetanic convulsions; at other times nothing particular is observable, either in the secerning or sanguiferous system. As to the state of the bowels, they are in general acted upon with great difficulty in tetanus, and what passes from the patient is generally very bad stuff indeed. With regard to the state of the nervous system, the patient is tranquil in his mind, and has his senses about him. I have known a man under tetanus, while his case was going on worse and worse, declare that he felt himself heart-whole towards the termination of the disease. The patient labours under no particular agitation of mind, but there is undoubtedly

great insensibility in the nervous system with respect to the operation of medicines. Alcohol will not fuddle a tetanic patient; opium will not make him sleep; mercury will not salivate him. He is very unsusceptible of the excitement of the most powerful medicines; what will produce a considerable effect on others, will not in the slightest degree affect him.

OF THE TREATMENT OF TETANUS.

I generally speak a little historically of the treatment of tetanus, and I do so because HIPPOCRATES has told us almost as much about this disease as we have known ever since. He says, that in certain cases of tetanus, where the fits increase, and there are exacerbations of the symptoms, the patient generally dies on a particular day. I do not exactly remember on which, but not far distant from the commencement of the disease; but where the fits do not become more frequent, nor increase in severity, the patient generally gets well. HIPPOCRATES states, that the most effectual remedy in tetanus is the affusion of cold water, but he limits it to particular cases, excepting traumatic tetanus, or that which is produced by wounds. It is curious enough that the observations of HIPPOCRATES seem to have been confirmed during the progress of medical science; and that, in truth, they comprehend all that is known with regard to the treatment of this disease. After the time of HIPPOCRATES, it was treated as a disease of tension; and strong action, and warm water, warm oil, bleeding, &c. were employed, with a view of producing relaxation. DE HAEN was the first among the moderns who tried to investigate the nature of tetanus. He tried all the various remedies which have been recommended; in one case, he took 130 ounces of blood from the patient in twenty-four hours, and the patient died. DE HAEN did not discriminate this disease, for some of the cases which he supposes to be tetanic are not absolutely cases of tetanus. Dr. WRIGHT, of Jamaica, sent over some papers to this country, which were published in the London Medical Observations and Inquiries, in which the cold affusion was declared to be a cure for tetanus. He mentions the case of one poor negro, who was so convinced of its efficacy, that he used to lie in the neighbourhood of a spring; and when he found the convulsions coming on, he would roll himself into the water. Dr. CURRIE, of Liverpool, tried the

cold affusion, and it certainly did good; he relates one case, indeed, in which every body conversant with the history of tetanus must be convinced that the patient would inevitably have died but for the cold affusion. He was plunged into the water, and kept there till he was nearly exhausted: he was, however, got out alive; a perspiration subsequently came on, the symptoms were mitigated, and eventually he did well.

Dr. Rush, of Philadelphia, treated tetanus as a disease of debility, and recommended tonics and stimulants. If you read the periodical Journals, you will find all these things eulogised, and no doubt many patients have got well, in whose cases tonics and stimulants have been employed. Alcohol, employed in quantities which would fuddle six healthy men, did not fuddle tetanic patients, but it produced a most corrosive acidity in the stomach. I will tell you honestly what I think of this matter:—I believe that the patients would have got well, if no treatment at all had been interposed. There are cases of protracted tetanus, in which the disease gradually wears itself out and spontaneously terminates. We very often impute too much to the remedies we employ. With respect to this cold affusion—I maintain it will not answer in a great number of cases! I remember, very lately, some cases published at Dublin, where a trial had been given to the cold affusion; the patients were first put into a warm bath, the temperature of which was gradually reduced, and at last they were soused into cold water, but nevertheless they died. I put no faith in this empirical mode of treatment, for empirical I must call it. I know of no specific remedy for tetanus. The application of the cold affusion seems to be only administering to a symptom; it lessens muscular action, at least for a time, because whatever lessens the temperature of the body lessens muscular action. The cold affusion has been tried at the Veterinary College; for the horse is liable to be affected with tetanus, and when under this disease really presents a very curious spectacle. His limbs are stiff, and his legs stand out; and his ears are cocked, and so is his tail also. A horse so affected was slung, and taken out of the stable at a time when there was much snow on the ground. He was rolled in the snow, and in a short time his ears became uncocked; his tail grew lank, and he stretched it; his limbs grew pliant, and he moved them; and so they led him back to the stable, where his limbs soon stiffened as before, and eventually he died. Now, I think

the nervous and muscular system so intimately connected, that tetanus may be considered a nervous affection; and I can only say, that we ought to pay especial attention to the removal of that irritation of the nervous system, which so constantly arises from a disordered state of the digestive organs. It is difficult to get the bowels to act in tetanus, but it is a most essential point to attend to their secretion. The abdominal muscles act with so much difficulty in this disease, that there is little fear of over-dosing your patient. Give calomel and jalap with treacle, which will answer better than saline medicines. If a patient is delirious, or is so violent that he cannot be induced to take a dose of medicine, I watch an opportunity when he opens his mouth, put a gag into it, whip the medicine with a spatula over the tip of his tongue, and then take out the gag. I have purged patients in this way, when I knew not how I should have accomplished it in any other manner. When the calomel and jalap have been administered till they act on the bowels, they will at last generally bring away such stuff as people who have never seen it, can have little idea of. I remember, on one occasion, asking an old nurse what sort of evacuations had come from a tetanic patient, who had passed a week without having any, "Lord, Sir, they are not stools—they are sloughs!"

With respect to opium, though it has no remedial efficacy in tetanus, it is impossible to witness a patient's sufferings, and not try to mitigate them by administering it. When you give opium with that view, you should administer it in such doses, and at such intervals, as to keep up a permanent effect on the system. I have known, and been educated under, surgeons who religiously believed that opium in large doses was a specific in this disease. I recollect a patient who had tetanus, from a wound which he received at the time of the riots in the year 1780, to whom a scruple of opium was given every day, besides a dose of a drachm at night, and when his body was afterwards opened, thirty drachms of opium were found undissolved in his stomach. Give it repeatedly in small doses, so that it may liquefy, and produce the desired effect, but do not suffer it to accumulate in the stomach. At the time opium was believed to be a specific in tetanus, I recollect a case in which it was thought desirable to produce a discharge from the bowels, and the patient was manifestly relieved by the diminution of the dose of opium, and the exhibition of pur-

gative medicines. Calomel and jalap were given at regular intervals, but thirty-six hours elapsed before any evacuations could be procured, and when they were, they were so intolerably offensive, that all the patients who were capable of quitting the ward, fled from it in dismay. I am convinced, that in tetanus, and all nervous affections, it is a most material point to operate on the brain through the medium of the digestive organs; producing secretions from the alimentary canal has a more beneficial effect than any other means we can employ for the alleviation and cure of tetanus. So much for the general treatment of tetanus. With respect to local treatment, suppose a man has his fingers crushed, and tetanus suddenly comes on, what is to be done? Should the finger be amputated? I see no objection to this, if the injury is such that the finger is not likely to be of any use, but will the amputation stop the tetanus? No; I think I have seen cases where it has been mitigated by amputation, but notwithstanding this mitigation the patients have died of tetanus. I would not therefore take off any material part of a man's body with a view of relieving tetanus. It has been said, that traumatic tetanus arises from a particular irritation in the wounded part, and that if you induce a change in the wounded part, you will effect a corresponding alteration in the symptoms of tetanus. On this supposition, caustic has been applied to the wound, and there is, undoubtedly, authority for the application of caustic. But there is authority for doing many absurd things, and I would not recommend you, because a man has been wounded to make a worse wound, and aggravate the injury with a view to the cure of tetanus. It will be sounder surgery to endeavour to soothe the local malady by tranquillizing the state of the general health. But there is another remarkable circumstance which deserves attention. When the irritable and sloughing state of the wound shall have gone off, when healthy granulations have formed, and the cicatrix is rapidly advancing, egad! in this state tetanus *may* occur. How are we to account for this? I am inclined to think that it is to be accounted for in the following manner: We know that the disposition to disease occurs long before the action of the disease takes place. In the painful and bad state of the wound, the disturbed state of the nervous system has produced disease of the digestive organs, which re-acted on the nervous organs; so that the disorders have reciprocally aggravated one ano-

ther; so that, at last, even though the wound is drawing to cicatrization, or has even healed, and there is no longer any local irritation, the disposition to tetanus, from the established irritation of the cerebral and visceral functions, comes into activity. Perhaps those who know nothing of the opinions I have formed on subjects of this kind may not thoroughly understand what I mean. I propose this as a question to surgeons, whether the disordered state of the digestive organs, established during the irritative state of the wound, may not be the occasion of tetanus, when that irritative state has ceased? If this proposition be established, the very important conclusion follows, that by preventing this disordered state of the digestive organs you would prevent tetanus. I have hitherto heard no opinion on this subject; it is a subject, however, of great importance, and one which deserves your most serious attention. It is to this subject, namely, the reciprocal operation of disorder of the digestive organs on the nervous system, and of the nervous system on the digestive organs, to which I shall in the next lecture direct your attention.

LECTURE III.

As local diseases become the source of constitutional derangement, so, on the other hand, *local disease* is often the *result* of a general *disturbance of the system*; and these reciprocal actions are produced through the medium of what has been called an universal sympathy. If the local disease which acts as an irritation to the general system be extensive, or be seated in an important part, the sanguiferous and secerning functions are disordered, and this disorder is characterized by the term Fever: and we find peculiarities in the nature of this fever, according to the particular state of the constitution at the time. Hence we find that the glands are affected in inflammatory fever, that there is languor in the hectic, and sensorial derangement in the irritative fever. We call it a state of fever, because

it is somewhat marked by the fulness of the pulse, and the variation of the temperature of the body.

It is not the sanguiferous system which is alone affected; but we find that the *nervous system* is thrown into a like disorder. There is great agitation, restlessness, irritability, and so on; the nervous system is in a state of excitement, and this very much influences the functions of all the organs of the body; and the secernent system becomes very much disturbed. There is, in fact, an immediate connexion of all the parts of the body through the medium of the nervous system, and therefore we need not wonder why the whole should more or less suffer from the injury of a part. But whether the brain first influences the stomach through the nervous system, or whether the stomach being first disturbed irritates, through the same medium, the brain, is a question not so easily determined. There is, however, a reciprocal action established, both of disorder of the digestive organs on the nervous system, and of the nervous system on the digestive organs, as I shall proceed to show you.

It is by this univeral sympathy we find that the digestive organs are affected, as well as other parts of the body; and this was noticed by Mr. Hunter: he said, that violent local irritation will, through the re-action which is excited, produce violent disorder of the digestive organs; and I will relate some cases which will show you how very great the force of this sympathetic action is.

An operation is performed, and the patient is put to bed; but, in the night, he is restless and uneasy; there is flatulency and pain in the stomach; there is nausea, and perhaps vomiting; and his skin becomes hot and dry; the bowels are constipated. What is to be done? What! but to relieve the turbulence of the system, by restoring secretions from the inflamed organs; and medicines given with such a view should not be given in irritating doses, but in moderate quantities, and at regular intervals. But these medicines will not act; what are you to do then? Why go on giving them, and you will find the patient will resist their influence, even until the second or third day, when he at length begins to feel some gurgling in his bowels, and about twenty or thirty fetid evacuations perhaps take place. The patient becomes relieved almost directly; yet, he has probably taken nothing but a little toast and water during the whole time; and this large quantity of fluid, therefore,

which is discharged, is the morbid secretion of the alimentary canal, and the other abdominal viscera.

I could tell you a diversity of cases in support of the proposition with which I set out. I shall separate one for this purpose: but they are all interesting, if I were to tell you the whole. The one which I have separated is interesting, because it shows, in a very remarkable degree, the influence of the nervous system on local disease. A man broke his leg; he got tipsy, caught his leg between the bars of a grating, and snapped his tibia, and grazed the skin; he was brought home, and the limb was carefully set by some surgeon; for the first three days after the accident I visited him, with the surgeon, and then I saw him no more. He went on very well; his general system was comfortable; and the functions of the body regularly performed. He went on in this way for about three weeks; when some friends came to see him, he played at backgammon with them, enjoyed himself very much, and the evening was spent very happily; in the night, he was seized with delirium; and the surgeon who attended him told me, that when he took off the splints, he found that the part of the skin which had been merely grazed had degenerated into an ulcer, and this ulcer continued to become deeper and deeper; it ulcerated through the fascia, and went on until it established a communication between the fractured bones and the atmosphere, and thus converted what was before a simple fracture into a compound one. Mr. Hunter says, that nature takes the alarm, and, in proportion to the effect of the mischief a disordered action takes place to attempt to repair it, and thus she struggles to effect the rescue of the patient. He was treated by cordials and laudanum, and he was relieved from the delirium; but his bowels were blown up enormously; he was completely tympanitic, and he died. On examination, the tibia was found broken into two or three pieces; and one of the fractures extended into the cavity of the knee joint. The brain and lungs were also examined, and the bowels, but nothing peculiar could be traced to account for the symptoms; the bowels, as I before said, were found immensely distended; and this arose, probably, from a state of atony of these organs.

Now, if a severe source of irritation will produce such violent effects on the digestive organs, then a milder form of it will produce the same kind of disorder, but in a less degree.

Every old woman knows that the progress of dentition in children causes a great constitutional disorder, and what is the consequence? Is not the child restless and uneasy? Is he not sick, and cannot take food? Aye, to be sure he is: he is purged, too, and the stools are white, or they are green; the bile is not secreted properly. In the nursery all this can be observed, but not so when persons are grown up to manhood, a state in which they are frequently only children of a greater form. Here, then, you see is a local malady which imparts irritation to the constitution, and this again disturbs the digestive organs.

To show you that a disorder of these organs may affect the system at large, I shall quote other cases. A man may have a blow on the stomach from the kick of a horse—nay, hang it, I always like to keep to cases:—The keeper of a lunatic asylum was struck a violent blow on the abdomen by one of the patients, who came very slily behind him without being seen; he reeled about, stood up, faced the patient, and attempted to secure him, but was struck again, and he fell down deprived of all power, and there he was soon afterwards found: he was taken up, and put to bed, but he lay in a state of stupor for some time, and his recovery was very doubtful. Purgative medicines were given him, but he had no evacuations for three days and three nights, and then they were of the most offensive kind. We understand these things better now than formerly; we know that the *secretions* are *dependent* on the state of the *nervous energy*. Experiments have shown, that if arsenic be given to an animal, there is abundant secretion thrown out of the stomach to defend it from the effects of the poison; you would be surprised to see what a quantity of fluid you would find in the stomach of such an animal. Mr. Brodie divided the eighth pair of nerves, and he then gave the animal arsenic, but there was no secretion to be found. Oh, no! The beautiful electrical combinations which had before produced their effects on the living structure were destroyed; and therefore the various compositions and decompositions necessary for the performance of the functions of the organ were at an end.

We cannot, by our purgatives, let them be of what kind they may, succeed in procuring the wished-for result by main force: but we must solicit, and solicit, and yet there will be no answer to our solicitations; go on for two or three days

with our medicines, and then there is a copious secretion poured forth. I have some doubt in my mind whether there is any benefit derived from them at all, and whether this return of secretion is not owing to some change which has taken place in the state of the nervous system.

I remember, when speaking of tetanus, I said, that there was the greatest difficulty in opening the bowels, and I forgot to mention to you that there is a remedy which has been found useful in this affection, it is the croton oil, and it is certainly a more powerful medicine, as a purgative, than any other. Yet when we consider the state of the system, and that constipation is the mere effect of irritation of the nervous system, we need not wonder why no reliance should be placed on it. The secretions should be solicited then, and not enforced. The medicine should be given in doses of moderate quantity, so as to act mildly, it should be also mild in its nature, and should be given at regular periods, say every four hours; and this is what I call soliciting the secretions.

Another case I will tell you: A man goes a shooting, the gun goes off, perhaps as he is getting over a hedge, it rebounds and hits him on the bowels; he feels rather alarmed for a minute, and has an unpleasant feeling in his abdomen, yet it presently becomes better, and he is determined to go on with his day's shooting, and he thinks little about the blow; but in the evening he becomes feverish, has a restless night, his bowels become constipated, and he has flatulence at his stomach and nausea. He at last recovers, after several days of rest, by restoring the secretions, which had been disturbed. We see here evidently that the digestive organs are disturbed in consequence of the local injury. It may be imagined that these are cases in which no man could expect any other result; but you will find, if you investigate the matter, that a compound fracture, or any other local violence, or nervous irritation of a less degree, will also produce the same derangement of the functions of these organs. We know that there is a direct communication between the stomach and cerebrum, and that the disordered state of one will produce a like state in the other. This disturbance of the cerebrum may take place in consequence of local injury, or more immediately from the operation of the mind. You may daily see that the various affections of the mind, as perplexity, fear, anxiety and grief, never fail to disorder the person's stomach and bowels. To conclude this

part of the subject, you must have noticed how much the cerebrum influences the stomach, and the stomach the cerebrum. You may consider this subject as one of the greatest importance in the practice of your profession. The disorder of the stomach and bowels is the fruitful parent of a very numerous and dissimilar progeny of local diseases.

Suppose a man is a glutton, and overcharges his stomach with all the cursed mixtures which a vitiated appetite can invent, what can he expect but the constant production of an irritable material, from the fermentation of the vegetable matter, and from the animal matter becoming rancid? The product is quite sufficient to account for all the train of evils which are sure to happen to the nervous system. In reference to this subject, I may say that it is of no use for a man to understand merely the words employed to communicate the knowledge of a thing, but he must understand the thing itself; and the understanding it must be the result of an exertion of the man's mind, whereby he receives and compares the import of what has been said. On all occasions satisfy yourselves as to the correctness of what you hear, by observation and the exercise of your own judgment. Now, if the stomach cannot digest what it receives, what is retained there must be creating compounds of a most irritating kind, and prove a source of contamination rather than nourishment to the system.

The secretions are generally disordered when the sanguiferous system is in a disturbed state. There is a very close relation between the sanguiferous and secerning functions; and as a proof of it, I shall instance to you the function of the kidney in particular. It acts as a powerful drain to the mass of circulating fluid, and gets rid of a large quantity of matter which would otherwise become a source of inconvenience, and perhaps mischief to the constitution. You must frequently have observed the state of the urine when the digestive organs have had their functions disturbed; and I think that this immediate sympathy may be explained by attending to the source from which they derive their nervous influence, and they do so from the splanchnic nerves, or from the ganglia, composed principally of these nerves. Now this is what I call Mr. HUNTER'S physiology; it is the science which looks to the state of organs associated in their functions, and to their liability to become disordered by certain causes. What I have said to you on this subject has been deduced from facts, and not from mere spe-

culations; the knowledge of the circumstance, that the disordered state of the digestive organs will produce a degree of cerebral disturbance, and thus, through this medium, re-act on local disease, is one of great importance; and I, therefore, shall not mind troubling you by a repetition of it. That this disordered state of the digestive organs may secondarily depend on the excited state of the cerebral functions, produced by the severity of local disease, is a proposition which I hope I have made equally clear. All this it is necessary to understand; but with regard to the absolute state of the nervous or sanguiferous systems, on which the performance of the healthy or disordered functions may depend, is a subject on which I have said nothing, nor do I wish to say any thing. Another point, of which we know just as little, is the manner in which particular medicines act on particular organs, to produce the consequences which we are in the habit of observing. You probably know more of medicine than I do, and I shall leave the subject to you who may be devoting yourselves to the study of medicine, to exert yourselves in investigating it, and then to tell us what the modifying causes are, and the experiments which you may make to ascertain them.

But the ordinary causes of these complicated evils are as plain as A, B, C. It is evident that they are to be traced to the very irregular and unnatural habits which men practise. When patients apply to me, and I see that their complaints are chiefly of the nature I have been describing, I tell them that I am no physician, and I offend them stoutly when I tell them that they have their health in their own keeping. If a man were to do as CORNARO did, he would be rewarded for it by a long and happy life. CORNARO was given over by his physicians at the age of thirty-five; he saw that there was not the least chance of recovery, if he continued to swallow the trash they were in the habit of giving him, and that there was no good in putting food into his stomach, if his stomach could not digest it. What did it do there? Why, it played the very devil with CORNARO's guts. "So," said he, "I dropped the plan pursued by my physicians, and adopted a regimen of my own." The principal beauty of CORNARO's life was the happy state of mind in which his continued temperance preserved him. He limited himself to twelve ounces of food for each day, which was of a nutritious kind, and no inducement could prevail on him to exceed it. He enjoyed the simplest food with

the greatest relish, for CORNARO's appetite was rather keen; so that he used to say, when eating a dry crust of bread, "Oh! how delicious it is; it is so delicious, that I am almost tempted to exceed my allowance;" yet he never did. He writes, between eighty and ninety, "the society of my friends is delightful, and even the company of children is amusing to me; and when not otherwise engaged, I read godly books. But the infirmities of age increasing upon me, and becoming more feeble, my friends advised me to increase my diet, which I did to fourteen ounces. But from the time I began to increase it I was dissatisfied with myself, and felt that it was producing mischief in my stomach. I had not continued it long before I was obliged to return to my former allowance." CORNARO, however, could not live for ever; and we find that, to the time of his death, he might be said to have enjoyed perfect health. He went down to the grave at the advanced age of one hundred and five; the account is given by his niece, who was a nun at Padua.

Now, what I propose as a diet, is Cornaro's diet, and it is no fanciful system. The diet should always be of a moderate quantity, it should not be either wholly vegetable or animal, but it should be of a nutritious kind. The diet I have taken the liberty to recommend to the public is Cornaro's, with a few conundrums of my own, as Dr. Franklin says. I do not pretend to have adhered to such a diet as Cornaro did. Oh, no! I acknowledge myself to have been a sinner; and I remember once having been living irregularly, and having been taking butter and sauces and sweetmeats, and indulging a pampered appetite, things that turned acid and rancid on my stomach; I was seized with pain in my bowels and headache, and had a sore throat; and I had a friend of mine, a physician, to look into my throat, and there was a long discussion as to what sort of cynanche it was to be; one said one thing and the second another; but I smiled and said, if you do not know what it is I know what will cure it; so I took a dose of calomel and jalap, and lived upon toast and water for about ten days, and I got rid of my sore throat and fever together. It is of great importance that the functions of the alimentary canal should be regularly performed, and the quality of the secretions and excretions attended to as well as the quantity. Every old woman knows how necessary it is to attend to this if she wishes to keep herself in a comfortable state of health, and therefore she mixes

up some gentle laxative compound, such as a little senna tea with some manna, or perhaps with a little tincture of rhubarb, and she takes sufficient of this to act at a given time, and if it should fail of its usual effect, why, she adds a little more to the dose, or takes a smaller quantity of it in four hours, and thus the end is answered perfectly well. All men should particularly attend to this subject, especially those who have sedentary occupations, or who are advancing in years.

The poetical Doctor, as I call him, Dr. Darwin, has said, that as there are moral errors to be corrected, so there are also physical errors to be corrected, as those of diet, habits, and so on. Now, I do say, that the public are indebted to me for what knowledge they possess as to the importance of attending to the digestive organs in the treatment of various local diseases, and of the advantages to be derived from alterative doses of mercury in these affections.

Of what importance is the liver in the animal economy? Surely, it is of great importance; it is the largest organ in the body, and if its function is disturbed, the result you may expect to find in observing the functions of other organs, associated with it in office, impeded. Yet a blue pill, in cases of this kind, will sometimes do wonders, if the regulation of diet be at the same time attended to. Mercury, when exhibited in small doses, and influencing the secretion of such an important organ, without disturbing the function of any, has been called, when thus employed, alterative. Medical men, in common with others, have committed a very natural error; they imagined that, as small doses of mercury did good, that larger doses of mercury must do more good, and therefore they found themselves mistaken.

The public have said that I never recommend medicine, and nothing has been more wrongly said. But if it be supposed that I place reliance on medicine merely, without attention to other circumstances, they may be right. For example, how comfortable, when the stomach feels weak and the appetite flags, is the exhibition of a tonic, such as an infusion of calumba or cascarilla; then again there are the mineral acids, they are very valuable, especially in combination with the former; then there is the decoction of sarsaparilla, it exerts a very beneficial influence on an irritable state of the system, and it appears to me to do so by acting on the nerves of the stomach, and thus produces a tranquillizing effect. There is a variety of

medicines which have, from their influence on the liver, and increasing its secretion, been called anti-bilious, but I should call them pro-bilious. When a person has palpitations of the heart, and uneasiness, with flatulency and distention, a dose of a medicine called carminative will be of great use and afford much relief, as the *Mist. Galbani*, for example, and you know there are many others; but all I could say to you of these medicines has been said by others, and perhaps better said.

LECTURE IV.

As in general diseases we name the disease from its peculiar character, so in local diseases we also name them from certain peculiarities which they may exhibit. We find the disturbance of one part influencing the sanguiferous, nervous and absorbent systems generally; but we may have local disease without such phenomena taking place extensively, but confined only to the neighbourhood of the part. Diseases have been classed according as they have exhibited the characters of a disordered state of either of the systems I have just mentioned: therefore, adopting the same plan, I shall begin with disorders of the vascular system, and shall speak of

INFLAMMATION.

And first, of the most violent kind of inflammation, namely, *phlegmon*, which is the most common form of inflammation, and generally takes place in a good constitution. But I always like to keep to cases, and I shall give you the following: Supposing some very powerful stimulus to produce a violent local irritation, as a piece of glass or iron sticking into a man's arm, and by sticking there, should be attended with heat, redness, throbbing, pain, and swelling, the part is said to be inflamed, and the inflammation is said to be phlegmonous. But what does common sense tell us to do, but to pluck out the foreign body, as the glass or the iron, and to apply a wet cloth

to the part? The irritation being removed, the increased action subsides, and the inflammation of that part is said to terminate by resolution. I have described a case, being very simple, and in which nothing more need be done. But the question is, how are all these circumstances produced? Here I have been pleased to enter into the theory of inflammation, not that I invariably do so, but to afford you a specimen of medical theory, and to fix your minds attentively upon the character of phlegmon. The question is, can we account for all the phenomena upon a plausible or verifiable theory? Let us try, if you please On a sudden, there is a great *swelling:* from whence does this arise? There is, it may be, some effusion into the interstitial cellular substance, which augments the bulk of the part; but an inflammatory swelling subsides upon putting on the tourniquet: the effusion then is not adequate to account for the very considerable bulk, though it may account for it in some degree. Well, we suppose the bulk to arise partly from the effusion, and partly from the turgescence of vessels. But the vessels are overcharged and distended with blood, and the part which is inflamed looks *red;* it was naturally pale: this, it may be said, is owing to an increase of the heart's action, forcing the red blood into vessels naturally carrying a colourless fluid; but there is something more than this; and that there is something more is rendered plain by experiment. Mr. HUNTER created inflammation in the ear of a rabbit; he had the animal killed, and immediately injected the rabbit from the aorta, but the vessels of the inflamed ear appeared larger and more numerous than usual, they were, therefore, preternaturally injected; now, if this increase in the size of the vessels had depended upon the force of the heart's action, the vessels of both ears would have been filled equally. Now we believe that the arteries of the inflamed part are not only more distended than natural, but there is also an absolute increase in their actions beyond those of the surrounding parts.

The more you consider the phenomena of inflammation, the more you will be inclined to receive this opinion, as I believe. But let us proceed. This supposition explains most of the other circumstances; it explains the redness, heat, and the throbbing of the part. If we take the commonest example, for instance, some stimulant is applied to the skin, it excites the actions of the part; you see that there is a swelling produced, and the skin is red. Now the redness is the effect of the pre-

ternatural injection of the red blood into vessels which did not naturally receive it, and the heat of the part also arises from the same cause. The surface of the body is much colder than the interior; but as the vessels of the skin become larger, the heat becomes greater. Mr. HUNTER tried to discover if there was any increase of temperature in inflamed parts internally, and for this purpose he injected some stimulating fluids into the rectum and vagina of an ass, and then introduced a thermometer, but he found there was no absolute increase in the temperature. But in the more superficial parts of the body, there is a comparative increase, because the heat is greater than it before was in that part, but not greater than it before was in the blood of the interior. Then there is the throbbing of the part; ah! what is it that causes the throbbing in a part in which there was none before? Why the pulsation of arteries carrying the blood, certainly it is, which causes this feeling.

One fact being granted, explains all the phenomena, and to show you that the throbbing does depend on the flow and injection of the blood from the heart, you will find it perfectly synchronous and simultaneous with the pulse. But there is another circumstance which I must lead you to notice; it is that which I have before hinted at, namely, that there is a disposition in the arteries of the inflamed part, not only to receive a greater quantity of blood than natural, but also to act on that blood with greater frequency than natural. In whitlows, for example, the arteries of the part inflamed, and the digital arteries, will throb powerfully, and the radial and ulnar arteries too, if the inflammation be very severe, and I have even known the excitement so great as to cause the brachial artery of that side to pulsate strongly. Now if you put your finger on these arteries, and on the same arteries of the opposite side, you will find there is an absolute increase in their pulsations. It is curious to see how far this disposition to pulsate does extend from the inflamed part. There are some arteries which pulsate, others which do not. The abdominal aorta always throbs, but more sometimes than at others, so much so, as to induce many persons to suppose that they have aneurism, and many medical men have I seen who have believed this; but it is dependent principally on the state of the stomach, for that organ very much influences the heart's action, but by taking a little bitters and a few drops of one of the mineral acids the aneurism has disappeared.

There is then a disposition in the vessels of an inflamed part to receive a greater quantity of blood than natural, and there is in severe cases also, an increase of the heart's action. We have been able to account for the swelling, heat, redness and throbbing of a part, and we have now to consider the cause of them. Some people talk a great deal about pain, and I could tell you a great deal about what has been said, but it would only be a waste of time. The whole part is inflamed, the vessels, nerves, and absorbents, and therefore we need not be at a loss for pain, But, say some people, the inflammation of the arteries is not the cause of pain; they are elastic, they admit of distention, like an inflated bladder; but JOHN HUNTER said that the pain was as great when they are contracted as when distended. You see that we cannot gain any thing by this supposition. Oh! there is no doubt but that the nerves are inflamed in common with the other structures: their fibres are put upon the stretch, and their sensibility is increased by the excitement into which the part is thrown; we may in this way easily account for the pain.

Well, I have accounted for all the phenomena, by the supposition that the arteries have a propensity to receive an undue quantity of blood. But what gives them this propensity? This is more than I can tell, or, I believe, any one else can tell. Arteries, when inflamed, are supposed to have their coats thickened as well as their calibre increased. But I wish you to fix your minds upon a phenomenon in inflammation which has attracted the attention of all observing men. It is the resistance given to the touch by an inflammatory swelling. You press it, and you can make but little impression on it, and it seems to furnish some considerable resistance. Now how can this happen? The arteries are continuous with the veins, and if there were no resistance, the blood must pass from the one set of vessels into the other. But there is an inflammation of the skin, which is scarlet in appearance, but then it yields readily to pressure, and the part becomes pale. But not so in phlegmonous inflammation; and that led BOERHAAVE to believe that there was an obstruction to the circulation through the smaller sets of vessels, from the extreme lentor of the blood, and led CULLEN to believe that there was a spasm in the extreme vessels; and later investigators to believe that there was an obstruction in the capillary vessels; other experimentalists believe that there is a surcharge merely in the quantity of

blood sent to the part, and that it cannot be returned in the same quantity by the veins, and thus attempt to account for the accumulation in the arterial vessels. They say, which I cannot believe, that this is sufficient to account for the resistance. Now, supposing I were to speculate as to the cause of resistance, I should say it was owing to the obstruction of the extreme arteries; and I should be led rationally to suppose a cause, namely, the irritability of these minute vessels. There are, as I before said, some arteries which pulsate, others which do not pulsate; and if you look at a frog's foot through a microscope, you will see the stream of blood passing steadily on through the minute vessels; if you irritate the part by pricking it with a pin, you will see a globule of blood try to force itself through a vessel, but it cannot, and it then passes off by another anastomosing branch; but supposing the irritation to be continued, the action is increased, the pulsating arteries increase, and the arteries which before carried a colourless fluid now carry red blood and pulsate, and thus create a resistance. I own I am a Cullenian as far as resistance in the extreme vessels goes.

The principal phenomenon of inflammation is, the preternatural distention of the pulsatory arteries. This action will fatigue and weary them, and they might be so fatigued as not to be able to contract again to their former size, some people say; but unless you apply the powers of common muscular action to the blood-vessels, I do not see why this must happen. The office of some muscles requires that they should be fatigued, and should be followed by rest, but the blood-vessels are never fatigued, they act 72 times in a minute for perhaps 80 years. Supposing a man possessed certain muscular powers, but that he was obliged to increase the ordinary effort a little, he might continue to do this for some time without getting weak, although there might be some subsequent debility. If a man, for example, be in the habit of walking twenty miles in the day, and should from some circumstances be required to walk twenty-five, surely this would be no proof of debility. Yet there are some who think that inflammation is weakness, but it seems to me to be preposterous. What! stick a piece of glass into a man's arm and have debility? No: there is an excited and increased action, and this accounts for the phenomena of inflammation. Who can doubt it?

Here I shall call upon you to attend to a circumstance which

you will frequently have to consider in practice, the hæmorrhage from diseased arteries. A patient has a diseased knee; the surgeon finds that nothing more can be done for the disease, that it has become a source of irritation to the constitution, and he advises the patient to have it removed: he is, perhaps, at the time a little easier than usual, and he puts the operation off; he has a fresh attack of pain, and he asks the surgeon to remove it. It is done, and instead of having to secure two or three vessels, you find about twenty little vessels which bleed profusely, and perhaps requiring as many ligatures; the patient is put to bed, but the surgeon is called up in the night to stop the hæmorrhage, which has again returned, and this may happen repeatedly. This was the practice formerly, but not now, because we always amputate beyond the inflamed parts.

Inflamed arteries, when divided, spout out the blood to a great distance, much farther than would have been the case in a healthy state of the vessels. Now, is this the action of the heart or of the vessels? The action of the heart cannot be partial; it sends the blood with equal force to all the parts of the body. Those who speculate upon the power of the heart's action seem never to have taken into account the following phenomenon: It is known to you, and it will be to all who study anatomy, that there are four little arteries which send their blood into the interior of the penis, and they distend it as suddenly as if it were injected by a twelve ounce syringe. Is this the heart's action? or what is it? Certainly it is done by the power belonging to the vessels of the part. Granting then that there is a degree of resistance, we may explain very readily the other circumstances. Some have said there is no obstruction in the extreme vessels; but I say there is an obstruction, because the blood is not driven into the veins in the same proportion as the arteries receive it; and I am disposed to believe that there is a spasm in the extreme vessels, from noticing that the secretions of inflamed parts are always interrupted. In nephritis you cannot get a drop of urine, or if you do it is mixed with blood, and the return of the secretion affords you a good proof that the inflammatory action is subsiding. I once had a dreadful attack of ear-ache, and I dare say most of you know what that is: well, it went on, became worse and worse, such a noise in my head as if twenty hammers were at work, and the pain dreadful; all at once there was a terrible crack, out something went with a very buzzing noise, and I was easier

directly. Why, it was an inflammation of the lining membrane of the ear, and it had terminated by the secretion of pus. The effects of phlegmonous inflammation on surrounding parts are worthy of notice; and you will find a different treatment necessary for the different kinds of inflammation.

Well, as to the *treatment of phlegmonous inflammation*, why, as I told you before, all that is necessary is to remove the irritation; apply wet rags to the part, and then it will terminate in resolution. Wet rags! what good can they do? Why, one of the most powerful modes of checking increased action locally, is to diminish the temperature of the part. Here I would observe, that when noticing any point of surgical practice, I do so once for all, and do not return to it. Heat, to a certain extent, is necessary for all vegetable and animal action: increase the temperature, and these actions are increased; diminish the temperature, and these actions are diminished. This is the proposition; but whether it is true is the point. That it is the case, I think, is apparent from the influence of the returning Spring upon the vegetable kingdom, and hyburnating animals; they leave their abode, and commence a new series of actions: on the return of Winter we find both again shut up in a state of indolence and inactivity. Now, although we are not dormice, we feel the change from cold to heat very agreeable, and we know that severe degrees of cold very much diminish our muscular efforts. When Dr. Solander was with Captain Cook, in his voyage round the world, a party landed on some part of North America, and amongst them was the Doctor himself. He had just been cautioning them against the benumbing and stupifying effects of cold, and of the danger there was of giving way to such feelings. He had just finished what he had to say on the subject, when he was taken in the very way himself. He said, "I am very weary, let me lie down; do let me lie down only a few minutes;" but if they had, he would never have raised himself again: the other officers insisted on his exerting himself, and absolutely drove him before them, and it was with the greatest difficulty that he could reach the ship. Soldiers and seamen have, in very cold climates, frequently perished by yielding to this desire to rest. They have stepped aside from their companions, and laid down, as they supposed, to sleep; but it was with them the long sleep of death.

Heat, on the other hand, augments natural action. There

have been certain *savants*, who have put animals into a room hot enough to broil beef-steaks in twelve minutes; and what was the consequence? Why, to increase the pulse most astonishingly. But were the animals' actions increased by it? Certainly not. The poor things could not skip about if they would, for one part of the room was just as hot as the other. Now, if you wish to diminish the temperature of a part, you do so by evaporation. You may convert water into ice in this country, even in the Summer, by this process. Ice has been made at Benares, in the East Indies, by evaporation. Now, to produce the proper effect of evaporation on the animal body, it is not necessary you should freeze the part, you only wish to diminish the excess of heat; well, then, you apply wet rags, wetted either with vinegar, or vinegar and water. There are various modes of producing evaporation, and you would have no such effect produced if you were to wet rags, and then put them and the part they covered under the bed-clothes. It is necessary it should have access to the air; the heat of the part makes the water gaseous, and the air carries off the heat from the part. Sponging the part, and allowing it to evaporate, is a very good plan; but then it should be done regularly and constantly; the heat of the part will rapidly diminish, and you should continue it until the temperature of the part is below that of the other parts. This is the most potential way of producing evaporation. Some men kill parts of the body very quickly by evaporation; but I do not think we have a right to kill parts. A man has an inflammation of his knee, you give him an evaporating wash, and tell him to keep his joint always covered with a piece of wetted linen; the rags smoke as if they were laid on a hot iron, and he feels the greatest comfort from applying them. But yet I do not like the words "cooling applications;" I would call this part of surgery the *regulation of temperature;* but not take the law into our own hands, and so diminish the temperature of parts as to kill them. Poultices made without lard evaporate; if there be a surplus of heat they will evaporate; but I call a poultice a local tepid bath, and I would not, therefore, put them on cold.

I shall speak of the *bread and water poultice.* The way in which I direct it to be made is the following: Put half a pint of hot water into a pint basin, add to this as much of the crumb of bread as the water will cover, then place a plate over the basin, and let it remain about ten minutes, stir the bread about

in the water, or, if necessary, chop it a little with the edge of a knife, and drain off the water, by holding the knife on the top of the basin, but do not press the bread, as is usually done; then take it out lightly, and spread it about one-third of an inch thick on some soft linen, and lay it upon the part; if the part to which it is to be applied should be a wound, you may place a bit of lint dipped in oil beneath the poultice. It is very comfortable and soothing to the part; it is like a bath of tepid warm water, it produces a perspiration on the part to which it is applied. I do not know a better application. If there should be a surplus of heat in the part, you may expose the poultice, and allow the evaporation to go on a little. The poultice may be made with poppy water, if you think sedatives are necessary. It may be made also with the hemlock juice, if recently expressed, and it is a very good application to irritable sores. So also you may make a carrot poultice, but it should not be made with the great coarse substance of that vegetable, you should use the recent juice. This poultice admits of medication, but there is nothing better that I know of than the bread poultice for broken surfaces.

There is an erroneous notion prevalent, that cold is bracing, and heat relaxing; but you need only consider this to see its absurdity. Heat excites action, how can it relax? Now I grant there is a difference between heat and moisture, and mere heat. But cold is bracing, what is meant by that? They say a cold bath is bracing; ah! a man jumps into a cold bath, and he feels chilled; he jumps out again, and rubs himself all over with a coarse cloth, he is invigorated, refreshed, and cheery; he feels as if he could jump over the moon. The heat and vigour that he feels is not from the cold water, it is the result of the re-action which follows; it has roused the action of the heart and arteries, and produced a temporary vigour and hilarity. If a man takes a glass of brandy he feels vigorous enough afterwards, but you cannot say that the brandy is bracing. To try the experiment fairly, you should keep him in the cold water a length of time, and see what a poor shivering wretch he would be; why, you might almost knock him down with a feather.

I do not now speak of leeches, or any thing of that kind: I have spoken to you of blood-letting, when on the subject of

inflammatory fever, that is, when there is inflammation of some vital organs.

But bleeding a man for phlegmon, and leeching him, would be all nonsense; the inflammation will either subside spontaneously, on the removal of the irritating body, or will proceed to another termination, of which I shall speak in my next lecture.

LECTURE V.

If the inflammation, of which I was speaking in my last lecture, do not subside on the removal of the irritation which produced it, or by the employment of the methods which I have pointed out, it hurries on, increasing in violence; the symptoms all become aggravated, and perhaps some constitutional disturbance is produced; it hurries on, I say, to a *second termination*. It is often a very violent disease, and having reached a certain height, it appears incapable of being counteracted, and terminates in a manner which I am now about to describe. When all the symptoms by which the disease is characterized, namely, the irritation, throbbing, and pain; when these become so severe that the patient can hardly bear the suffering any longer, all of a sudden there is a peculiar circumstance takes place; something seems to crack in the interior of the swelling; it is attended with a sensation of rupture, and a fluid is found on the examination of such cases—that is the fact: what is the consequence? Why, an immediate abatement of all the pain takes place; the quantity of fluid increases, the parts become less hard, there is a sort of pyramidical protection from the centre of the tumour, the fluid approaches the face, and a fluctuation becomes palpable to the touch; the superincumbent parts become thinner and thinner. The skin is taken away in one or more points, one generally when the inflammation is healthy, and we are supposing phlegmonous inflammation as it occurs in a healthy person: something escapes beneath the scarf skin; it becomes distended, then

bursts, and a great discharge of matter takes place, and there is a complete abatement of all uneasiness, and the disease may be said to be well. Very frequently shivering has been observed to take place when this change occurs, and on this, subsidence of pain, and the feeling of fluctuation; and it is a circumstance which has always engaged the attention of medical men; so much so, that when inflammation is going on in internal parts, and the changes that these parts undergo cannot be seen, they think shivering a symptom, as they call it, indicating the change which has taken place in the local actions of disease, as Mr. HUNTER has observed; he says that he has seen it precede ulceration, and he attributes it in every case to an affection of the stomach. Now it does not always occur when the fluid, which we denominate matter or pus, is forming, as I shall afterwards have to show you, when I am considering that low, languid kind of inflammation, which is denominated chronic.

I wish now to engage your attention to this particular process, and to the nature of the circumstances which I have described. An effusion of fluid takes place, this must happen by secretion; there is no rupture of vessels, and therefore it must be a secretion. The secerning arteries pour out something, and we have reason to suppose that it is a serous fluid. Examine a part which has been the seat of inflammation, that is, of recent inflammation, and what will you find? Why, you will find in it a fluid, colourless, transparent, and coagulable by heat, and thus far, in all probability, serum. And again, when you apply a blister to the skin, remove the cuticle and dress the sore with a piece of lint, take it away again in a short time, and you will find that it will be covered with a serous fluid, and afterwards pus will be secreted from the same surface. The secretion from the surface of a recent wound is first serous, and then the secretion becomes good matter, as we phrase it, after a time. But when pus is secreted, it is not effused into the cellular membrane merely, because, if it were, it would be diffused wherever that texture is found; but it is poured out into a cavity, a cup-like cavity prepared to contain it. Now this contrivance is very curious; however, granting that it was effused originally into the cellular substance, still it could not pervade this tissue, because there is a limitation set to its extent. It may be accounted for in this way; the inflammation, which was most severe in the central part of the swelling, was less severe in the circumference, and whilst the

effusion of a serous fluid is taking place in the centre, there is in the surrounding part, where the inflammation is going on to a less degree, an effusion of a gelatinous material, which agglutinates and consolidates the surrounding structures, and thus sets up a barrier to the diffusion of the central fluid. It is called a gelatinous matter, because it is like animal jelly, and has the property of uniting the surfaces of a recently incised wound, and it was first described by Mr. HUNTER as the adhesive matter. He describes the process by which the walls of an abscess are formed, in the following manner: "That inflammation which is most violent in the centre, and tends to suppuration, is less violent in the circumference, and is disposed to throw out adhesive matter, which, by gluing the parts together, produces a mound to prevent the extension of the pus into the cellular substance." This cavity, which you may call the cyst of the abscess, is walled in by a combination of the surrounding cellular texture, by the means which I just described.

We suppose that matter is poured out in the centre of the tumour, and as the quantity of matter increases, it enlarges the space by the pressure which it occasions; it continues to increase, and approaches to the surface of the body; and as the collection of matter forming the abscess extends itself, the parts situated between it and the surface of the body become removed, until all the superincumbent parts are entrely taken away by a process of absorption; the matter reaches the surface, and then it is that it issues from the cavity of the abscess below the scarf skin; it becomes distended, and gets thinner and thinner, and at the last it bursts, and the matter escapes. Now, if you speculate as to the cause of the increased action of the absorbent vessels, you may say that it is owing to pressure; but pressure operates equally in all directions, and this absorption is continued only towards the surface of the body. There seems to be a general law of the animal economy, that all parts are to become absorbed, which are situated between an abscess and the surface of the body, as if to mark out a way for the escape of the matter. If an aneurism form, which is the giving way of the coats of a large artery, and the subsequent effusion and coagulation of blood, if, I say, it form in the interior of the body, why, it does not burst into the chest or abdomen; oh, no; but all those structures which are placed between the aneurismal tumour and the

surface of the body become absorbed. If the aneurism happen in the chest, then the ribs and sternum are taken away; the muscles covering these parts are also removed; the skin ulcerates, and the discharge of blood takes place. Yet you cannot say that this is all done by mere pressure; for that which presses must press in all directions, and the absorption here takes place only toward the surface. Again, if the aneurism should happen in the abdominal aorta, where it passes down close on the back, the whole of that part of the vertebral column situated near the tumour has been taken away. All I can say is, that it is a very curious process, and appears to be owing to some very wise provision of nature for the safety of the animal body, with which we are at present unacquainted.

As I described to you the process of absorption, I said that the matter got beneath the scarf skin; now there is no evidence of the cuticle ever having become absorbed; and if an inflammation takes place in the finger, and goes on to suppuration, the matter may get beneath the cuticle, and detach it all round the finger, but the cuticle keeps it there, and acts as a bandage, and prevents the matter from escaping for some time; but at length it breaks from the distention, and this generally happens where the cuticle is weakest, which is where it joins with the nail. It is the accumulation of the matter, then, around the finger, that is termed a whitlow. All these points are interesting and important, and deserve, therefore, your most serious meditation. There is a point which I wish you to consider, it is this; the whole of the inflammatory appearances are increasing, and before the matter is discharged, there is a considerable projection of the abscess, and it becomes conical or pyramidical. How can you account for this mounting up of the abscess above the natural height of the skin, and this pyramidical appearance, unless you suppose that there is a relaxation of the surrounding parts of the skin favouring this protrusion, and no doubt materially conducting to the more rapid absorption of these parts?

THE NATURE OF PUS.

Who noticed these interesting changes in diseased actions before Mr. Hunter? No one. He it was who attended correctly to the progress of disease, and he examined also the nature of the fluid which was discharged. But what comes

out? what is the state of the part from whence it comes? Open an abscess and look at its contents; you see that the matter is discharged from a neat little cavity, having a villous surface, that an anatomist would say, Oh! this is a surface destined for a peculiar office, it is capable of secreting and absorbing. JOHN HUNTER, with a view of ascertaining this point still more decidedly, opened the tunica vaginalis of a rat, and introduced a piece of tow into the opening; he took it out several times, and replaced it again, and he found that there was nothing but serum at first upon the tow, but he afterwards saw that there was pus upon it; now, said he, that will do; he killed the rat and examined the surface of the tunic, and he found that it had acquired a villous character. You will see how apparent this change in the surfaces of mucous membranes is, if you notice the eye. When there is a disposition to secrete pus in the polished surface of the conjunctiva, see how rough it becomes; it is covered with little prominences, more or less distinct, but sometimes they acquire a large size, and prove a source of irritation to that organ. It is clear, then, that pus is a secretion. Now if the secretion of pus should go on with greater speed than its absorption can take place, then there is an increase in the size of the abscess; but if the absorption goes on quicker than the secretion, then the abscess diminishes; but if the absorption and secretion go on equally, then the size of the abscess remains stationary. It does not necessarily follow, that, because suppuration has taken place in the interior, that the abscess must go on and burst; but this I shall attend to more particularly when I come to the consideration of that inflammation which is denominated chronic. But phlegmon is a violent disease; it must either be arrested *in limine* by the removal of the cause of irritation, or it will hurry on to suppuration. All abscesses are not of this violent character, and therefore it does not follow that, because suppuration has taken place, it must go on and break.

What is the matter that is discharged?

It is a subject for which prizes without number have been given, and prize dissertations have been written, all having for their object the mode by which you might distinguish pus from other fluids. I could tell you a great deal of what these prize dissertations have furnished, and I could tell you what experiments I have made on the same subject; but all these are already before the public; and I should only be detaining you

here longer than you or I should like, if I were to enumerate the whole of them. All that I can say on the subject is, that you ought to read what has been written, and judge for yourselves. It is my duty, however, to furnish you with the information which I have been able to collect on this point. Now what I consider to be the most certain *distinction* between *pus* and *other fluids*, the consequence of disease, is, that pus contains globules. If you take the trouble to examine the matter recently discharged from an abscess, and examine it with a microscope, you will find this to be the case, and these globules will be of various sizes. I have read lately that these are the globules of the blood, but I do not believe that they are, because the globules of the blood are of a uniform size, and the globules of pus are not. This was the point Mr. HUNTER attended to when he examined the circumstances attending the *secretion of pus*. He said, that which was serous in the first instance did by some change in the secretion become globular. Whether these are the facts or not I cannot tell, they are what Mr. HUNTER said, namely, that the secretion was first serous and afterwards puriform. I might have before told you that pus was at one time considered to be a solution of the animal fibre; that it was melted down by a sort of decomposition, into a white fluid; that it was a breaking down and concoction of the solids. But this was clearly set aside by showing that pus was often found in parts where no breach of texture could be detected. It was a long time before the doctrine of pus being a secretion was brought to perfection, and it was astonishing what difficulty Mr. HUNTER experienced in making the profession believe that it was so. The opposition and obstinacy which that man had to contend with no person can tell.

Pus is a liquor which is just the colour and consistence of cream; one might say that it was a very good dressing for a sore. You know the definition of what the old surgeons called laudable pus, viz. that it was *album, liquidum, et equale*.

We have supposed that the cavity of the abscess is opened, and the matter which it contained discharged. What is the mode of procedure which nature adopts to restore the loss of the part, and what takes place I shall now describe to you. After the vessels have continued to secrete pus for a few days, the inflammation in the part, if properly treated, becomes less, and there is a gelatinous fluid poured out; into this jelly new vessels shoot, and cause little projections on the side of the abscess;

from these little projections other vessels shoot, for they soon become very vascular, and are covered with a puriform fluid. These *granulations* continue to increase, and may be seen occasionally even protruding beyond the level of the aperture. The disease has subsided, the cavity of the abscess is completely filled up, and this appears to be Nature's cure, and her mode of cure.

By attention to these facts, it follows, that any interference on the part of a surgeon is injudicious and unnecessary. The superincumbent parts become thin, and an opening is gradually made by which the matter escapes, and they are not at all inclined to heal till the cavity is quite filled. But some persons, inattentive to the process of Nature, think that if a man have a phlegmon and some shivering, that he must set about letting the matter out, and he sticks a bistoury into the swelling, and perhaps lets out a little, but the parts covering the abscess are not so thin but that they are inclined to unite, and the edges of the incision become closed. Nature's processes are all carried on in the best possible way, and as she has ordained them. There is such a diseased state of skin induced, that it has not the power of getting well until the cavity of the abscess is filled up.

TREATMENT OF PHLEGMON.

What are we to do? What is the surgical treatment of phlegmon? Why, scarcely any; we put a poultice over the inflamed part, after having first tried to reduce its temperature, and this is to be done, as I before told you, by putting on cloths wetted with water, or vinegar and water, and allowing the fluid to evaporate. The patient will say, "Oh! how comfortable this is," and you may go on applying the cloths until the excess of heat is diminished. But after this has been done some time, he will say, "this wash does not feel so pleasant as it did, it chills me very much, and I am very uneasy; cannot you do something else for me?" Well, what you do then is to lay aside the cold application, for we have no right to take the law into our hands and freeze the part, but put on a *greasy poultice*, when the patient will say, "this is very pleasant and comfortable; thank you, Sir; thank you, Sir." Now I remember that when I was appointed Professor of Surgery to the Royal College, I entertained the persons assembled with the

manner of making a greasy poultice, and I said to them what I shall presently say to you on this subject. But I thought to myself, that supposing I should have met a person on my return from the College, and he had said, What! could you find nothing better to talk about before such a *learned body*, than the method of making a greasy poultice? I should have said in reply, I thought that the greasy poultice was not properly made, and I considered it my duty to tell them so; and I hope they will forgive my presumption.

Well, now, as I have told you how to make a bread aud water poultice, I shall now describe the manner of making the greasy poultice. Scald your basin, by pouring a little hot water into it, then put a small quantity of finely ground linseed meal into the basin, pour a little hot water on it, and stir it round briskly until you have well incorporated them, add a little more meal and a little more water, then stir it again. Do not let any lumps remain in the basin, but stir the poultice well, and do not be sparing of your trouble. If properly made, it is so well worked together that you might throw it up to the ceiling, and it would come down again without falling in pieces; it is in fact, like a pancake. What you do next, is to take as much of it out of the basin as you may require, lay it on a piece of soft linen, let it be about a quarter of an inch thick, and so wide that it may cover the whole of the inflamed part. Then take a bit of hog's lard on the top of your knife, and put it in the centre of your poultice, and when it begins to melt, draw the edge of the knife lightly over the surface of the poultice, and you will spread the grease over it. It is now ready to be applied, and it is what I call a *greasy* poultice. Oh! it is one of the best applications which can be made to an inflamed and irritable part, provided the linseed meal with which it is made is sufficiently fine; but I never see any such thing now as good fine linseed meal, it is all pressed into oil cake and given to cattle to fatten them.

It is, when made in this way, oh! it is beautifully smooth; it is delightfully soft; it is warm and comfortable to the feelings of the patient, and it is the best application that can be made to an inflamed part, as I have before said. The reason why I have lately preferred bread in the composition of the poultice is, because I cannot get the linseed meal in a sufficiently fine powder. So much for the greasy poultice. You may laugh if you please, but it is a very good poultice, and the manner of making it cannot be too generally known.

I shall suppose that this violent inflammation may terminate in *mortification*, but it rarely does so; phlegmon is a disease which occurs generally in a healthy constitution, and therefore it is very seldom that the parts mortify. But I admit that the action of this inflammation may be so severe, that it may exhaust the vital principle of the part, and terminate in what we call mortification. I shall consider this more fully hereafter, as a separate subject; but I will tell you what Mr. HUNTER said of it, and you can have no better authority. The blood congeals in the vessels, the circulation goes on no longer, the part becomes cold, changes its colour, and there is some effusion of serum beneath the cuticle. The blood does not circulate in the vessels immediately surrounding the mortified part, but coagulates in the vessels to a considerable distance. This is a very curious circumstance, and serves to shew that there is some peculiar sympathy existing between the vessels and their contents. To such a distance does this coagulation of the blood take place, in the vessels leading to a mortified part, that I have known it to happen, that when an operation has been performed there has been no hæmorrhage. I recollect having seen a man who had a diseased leg, and mortification took place in it; it was considered necessary to amputate, and the limb was consequently amputated above the knee; there was no hæmorrhage; the tourniquet was loosened, but no blood flowed. On examination, it was found that the popliteal artery was plugged up with blood, even to a considerable distance above the place where it was divided; it was a *bloodless* operation. I have seen such more than once, but I never wish to see it again.

I say that phlegmonous inflammation, it must be granted, will sometimes terminate in mortification. What are we to do when it does so terminate, and what applications are we to make to the parts? Oh! they must be soothing, soothing, nothing but soothing. It is excess of action which causes the parts to perish, and therefore you must soothe them. You do not make your applications with the view of acting on the dead parts, but on the living. You must remember, that whilst the inflammatory action is so severe in the centre of an inflamed part, as to cause its vitality to be destroyed, that the neighbouring parts are still very much inflamed, and perhaps going into a suppurative process, and further from these parts the inflammation is still less severe; so that you see there are va-

rious degrees of inflammation in the parts, as they recede from the central part of the swelling. The best thing you can do, is to apply a poultice over such a part, and attend to the regulation of the temperature of the surrounding parts, in the manner which I have before pointed out. You will find much written on this subject in surgical and medical books, and it is useful and necessary that you should know what has been said. You will find various methods recommended:—they say the part is dead; other parts are becoming so; they are in a weak state; and we must support the life of the part and of the system. But always exert your own understanding in cases of so much importance; look at the state of the parts, anatomically and physiologically; and then ask yourselves which treatment you ought to adopt.

There is an inflammation which takes place in the animal body, which is very languid in its character, is very insidious in its approach, and is very slow in its progress; it is called

CHRONIC INFLAMMATION.

It is of an indolent description; it does not attract much attention, because it is not attended with pain. But in speaking of this subject I am speaking of a disease, which, with its various modifications, is of the utmost importance in the practice of surgery. There is no part of the practice of surgery that requires so much ingenuity or skill. Any man knows how to treat an active inflammation; but this slow, disorganizing action will often baffle our best attempts, and defy our most efficient remedies. Our notions respecting its treatment will very much depend on the views which we have drawn, in our own minds, of the changes which inflamed parts undergo; but if they are contradicted by an appeal to facts, then they are opinions that we are not justified in holding.

We generally see in all parts of the body, that when the action of any part is increased, its secretion is increased; but then this secretion is not of its ordinary character. We have daily examples of this in disorders of the liver. Again, supposing a membranous surface to be slightly inflamed; instead of its secreting a thin transparent fluid, you will find that the fluid will become more gelatinous in appearance, and capable of gluing together the surfaces which are brought into contact. We have numerous proofs of it in the adhesions which we find

taking place between the folds of the peritoneum and the surfaces of the pleura, membranes which have been termed serous. We see it again occurring in the cellular membrane; the interstitial structures are connected together by a material, which is elaborated by this slow and indolent form of inflammation. There is not only a thickening of substance, but there is an increase of structure which follows. New vessels shoot into the newly-combined parts; these, again, produce new materials for a further growth; and thus the morbid structure continues to increase; and I cannot help thinking that there is some morbid propensity in the vessels of such a part to take on such a diseased action. These are no fanciful notions; they are such as have been confirmed by facts.

Did you ever examine an inflamed part? If you did, you found, in the early state of the disease, gelatinous matter effused; and into this matter you saw little vessels shooting, thus connecting the unorganized with the organized structure. Vessels cannot grow into nothing, or "into thin air." There must be some medium for this purpose: jelly seems to be the matrix, and is very well adapted for the growth of vessels, and they have a great propensity to shoot into it, to grow, and become a living mass. The structure is thus rendered, as it were, granular; but the process is accomplished in the deposition of the adhesive matter, without the effusion of a puriform fluid. Chronic inflammation is an inflammation which very much disorders the structure of a part, before it engages much of the attention of the surgeon or the patient. But the functions of the parts are also altered with their alteration of structure. We see also, very frequently, that those parts which are chronically enlarged undergo certain changes, either from a disturbance of the general health, or some irritation excited in the local diseased action. Other forms of inflammation, I say, may supervene; a more active inflammation may take place: then the chronic inflammation will suppurate; perhaps ulceration will take place, and the part in this way becomes disposed of; or inflammation and sloughing may come on, and in this way the destruction of the diseased structure may ensue. But these changes in the actions of the vascular system, by which the functions of organs become vitiated, and the structures of parts altered, do not take place in the healthy constitution: or admitting that some result of chronic inflammation had been produced, if the general health had been restored and

preserved, none of the consequences which I have just described could supervene. There is no doubt in my mind, that the state of the bodily health does very much influence the local diseased action; and I could give you many cases to prove this, in addition to those I have already told you, but it is not now worth while to do so.

I am never afraid of inflammation in an active form. Let me see inflammation attacking a part, and I know what to do with it; but, oh! hang it! I am horribly afraid of this low, disguised, chronic form of inflammation, which alters the natural structures, and destroys the proper offices of parts. The treatment of such cases will form a part of absolute surgery, and therefore, in my next lecture, I shall begin what I call *absolute surgery.*

LECTURE VI.

In my last lecture I attempted to describe the nature of a peculiar kind of inflammation which has been called Chronic, and the disorganizing effects it produced in structures which it attacked. I wished you to understand from what I said, that the *morbid actions* of vessels led to a *morbid deposition* of matter, which would not take place in a healthy state of the constitution; that this morbid deposition of matter becomes the foundation for a new growth of vessels; or, as it has been said, becomes the *nidus* for their formation. I said also, that it was by the continuance of these actions that *morbid structures* were built up, and the natural structures destroyed; now there are also *morbid secretions* produced by these actions which tend to keep up the irritation produced by the disease.

There are, it is evident, also certain *morbid propensities* to disease, and there is only one way in which this can be explained. A certain harmony in the office of all the structures of the body is necessary to constitute the state of health; disturb the functions of one part, and you will, to a greater or less degree, disturb the whole. Continue the cause of disturbance, and you will have disordered actions produced some-

where, but if any part, by formation or the influence of habit, be weaker than another, we shall, in all probability, find that part most disturbed. It is to prevent this tendency to diseased action, by an attention to the general health, that constitutes the principal point in the practice of our profession. Inflammation and sloughing, as I told you, may supervene in the newly formed structures, and thus the character of the disease be altogether changed; and to what can you attribute this, but to some derangement which is produced in the general health? I say, that this tendency to diseased structure is what we have principally to guard against in the practice of surgery. And that there is something unhealthy in the constitution which peculiarly modifies local disease has been abundantly proved; it is what we have to counteract by all the means in our power.

There is, however, a *morbid increased action* in the part; and I know of nothing so calculated to diminish it, as the *diminution of the heat* of the part, not to that degree which would kill the part, but to such an extent as I mentioned when speaking of the regulation of temperature. But there is another mode, it is this; you may lessen increased action *locally*, by lessening the quantity of blood circulating in the vessels of the part. Blood is the natural stimulus to the vessels, and by taking away blood, you diminish the action of these vessels; therefore it is that you put on leeches. Now as many or all of these diseases are connected with a bad state of health, take care that you do not weaken too much. Put on a few leeches once or twice in the week, never more than six or eight at the most at one time; but this number once in the week will generally be sufficient. Do not put on so many, or put them on so frequently as to induce weakness. At the same time, I do not say that the application of leeches does that good in cases of purely chronic inflammation, which in more active inflammation they do. They do some good, but not to that degree which would warrant me in recommending them as a surgical means likely to prove beneficial. But if a more active state of inflammation should supervene on the chronic, then their application would be not only useful, but necessary.

It is very wrong frequently to touch local diseases, and it is no unusual thing to see some surgeons handle a part very roughly, in order, as they say, to examine it; and you will often see the patients themselves examining the parts in the

same way, by pinching them about. A touch is a bruise to a local disease, and many times have I seen persons keeping up their disease by repeated examination of the lumps which they may have had in their bodies. I can tell you a case in which you may have a very good proof of the impropriety of the practice.

A lady had a small tumour in her breast, she consulted a surgeon, and he ordered leeches to be applied; the leech-bites produced an extreme degree of erysipelatous inflammation, which extended over the surrounding parts, and had an appearance that the surgeon and the patient did not like. The lady became alarmed, and I was sent for, and I met the gentleman at the lady's house: he happened in the hearing of the patient to say, "Dont you consider it a very unusual occurrence, to follow the application of leeches?" "No Sir," said I, "certainly, not; it is a thing which frequently happens in irritable constitutions; let me say, in persons who have much disorder of the digestive organs." "Lord, Sir!" said the patient, "that is my case, that is exactly my case--what must I do?" I ordered a bread and water poultice to be applied to the part, made in the way I told you a short time since; advised her to try to keep her bowels regular, and to tranquillize the disorder of the digestive organs. The lady then said, "Will you be good enough to feel this lump: did you ever know such a lump as mine is?" "Yes," said I, "many;" and if you attend to what I have told you the lump will soon disappear." So it did; it very soon got well, and she went into the country. Four or five years afterwards she returned with a similar lump; I told her that she must attend to her diet, and adopt the use of some mild aperients to regulate the bowels, and that this lump would go away also. But, to my astonishment, it did not go away, the lump did not subside. I called several times, I believe for many weeks, and could not account for the obstinacy with which this lump remained. One day I observed her fingering and pressing about the tumour, and I said, "Pray, madam, do you finger it in this way frequently?" "O! yes, continually." "I insist on it, then, that you promise me immediately that you will not touch it again for a week, or until I next see you." She did so, and the swelling soon subsided.

By soothing applications chronic swellings may remain stationary for many months, or even years; but still there is swelling; and the object is to get rid of it by causing the ab-

sorbents to take up what has been effused. For this purpose surgeons have adopted a stimulating plan of treatment; they have employed such substances as will irritate the skin and produce pimples, and they have therefore called them *rubefacients;* such as the volatile liniment, liniments made with oil of amber, and other stimulating oils. They have employed *frictions* for the same purpose, either made simply with the hand, with brushes, or with some ointment, as the camphorated mercurial ointment. You may electrify such swellings, and if properly managed, with very great advantage. You may also bandage them, to make pressure on the swelling, and so on. If I were to discuss each of these methods to their full extent, it would lead me very much out of my way indeed. But the cases which I shall have hereafter to mention to you will exhibit the advantage to be obtained from each in the most plain and manifest manner. But what I have to say as a *general rule* for the application of all these methods is this, *all increase of morbid actions should* have completely *subsided before* you adopt a *stimulating plan* of treatment. If you do not attend to this circumstance, and begin your frictions and your rubefacients before the inflammatory action has subsided, you can only expect that the disease will become worse. But first diminish this action by the application of leeches, and there gulation of temperature; then apply your stimulants to the surface, and commence your frictions, but take care that you do not by these induce any inflammation in the diseased part, and your frictions and stimulants will prove beneficial. The effects of friction are so strikingly useful that I shall leave what I have to say on this subject, until by a recital of cases I shall convince you in the best possible manner. Stimulating the surface, producing what is called *counter irritation,* is a very good mode of removing some indolent swellings, such as by blisters, and the tartar emetic ointment. Then there is *bandaging*. Oh! bandaging is made of very great utility in surgical practice; but, as I have already said, I shall leave it until we come to that part where its effects will be more declaratory.

Electricity.—This is a part of surgical practice which may be considered as an *unique*. All the other means which I have before mentioned operate on the surface, but electricity will pervade the very centre of the body. It may be so managed as to be made to pervade a tumour even in the abdomen.

It is a species of stimulation, and may be applied in various degrees of force. Much disputation has been raised as to the manner in which electricity operates on the nervous energy; but I shall speak of it here as a stimulant operating on a local disease. Now with regard to its application, you must modify it according to the tenderness of the part; if you stimulate the part too much you will produce inflammation, and the tumour will increase. Now, electrify one swelling, and the swelling subsides; electrify another, and it will increase. What can we think of it? and think we must; why we must consider that it acts as a stimulant, and increases the action of a part; that if the swelling is disposed to disperse, it disperses; and if disposed to suppurate, it goes on to suppuration. You should begin with very small charges of electricity. You would like the patient to feel it a little; let it be managed so as to produce a feeling of warmth and tingling in the part, and let that feeling subside, if you saw that it did not increase the action too much; then touch it up again, and you might increase the force of the charge. I have frequently heard people say, if you speak to them on the subject of electricity, "Oh! I do not think it ever does any good, I have given it a very fair trial, I was electrified every other day for two months." All this is very possible, and they might have been electrified for two years, without any benefit, unless there be some precise mode adopted for its employment, and the effect which it produces be regarded. Now this is the worst thing that can ever be done; it is this practising without intellect that constitutes empiricism; it is blindly adopting, in this way, a power without a principle to guide it, that I abhor above all things.

Now comes *counter irritation*, a part of surgery which is always liable to be abused. The way in which it is frequently done is like what has been said of the tinkers, that they mend one hole and make two, and so to mend one disease they often make another.

"Where the greater malady is fixed, the lesser is not felt." But surgeons, by their mismanagement, often produce a greater disease, and the original "hides its diminished head." Well, but properly managed it is productive of great benefit. To speak first of *blistering:* you need not apply a blister a foot square in order to derive advantage from it. Oh! no. But blister a small part of the surface of a large swelling at a time. Quiz it as they like, it is a very useful procedure. Yet there is

sometimes more harm than good done by it, for it should be always recollected that you are making a second disease for the system; there is one which nature has produced, and there is another which you have produced for her, and therefore both may very much irritate the constitution. For all local diseases are very much connected with the state of the general health; disturb the general health, and you will do more harm than your counter irritation will do good.

There are various modes of producing counter irritation; you may do it by a vinegar poultice, a poultice made of the grounds of stale beer; by the tartar emetic ointment; by blistering, as I before mentioned; by making rowels, as I call them, that is, by putting in setons, and by making issues. Blistering may be considered one of the mildest of them, yet a blister, oh! a blister is a horrible thing in an irritable system, and disturbs the general health more than you would suppose. There is only one lenient mode of blistering that I know of, and that is, to put a blister on the part of the surface of a chronic swelling, and let it abide on for twelve hours, then snip the cuticle, let out the serum, and dress it with the spermaceti cerate; when this has healed, apply another blister on the adjoining surface, and so on, in succession, over the whole swelling. But to dress a blister in the way it is usually done, is dreadful; to take away the cuticle, and apply savine cerate, it is tormenting; it disturbs an irritable constitution to a degree that no one who has not witnessed its effects could suppose. Here, again, is a subject on which I shall have to say more hereafter. You can never learn surgery from one of my lectures, for one part of surgical practice so explains another, that it may be said to be contained in a circle, and one part is imperfect without the other part.

Setons.—Now then we come to the *setons,* rowels, as I call them; you pinch up the skin, thrust a skewer through it, and then let a large piece of silk remain in the opening you have made; you pull this backwards and forwards every day, to keep up the irritation, and if this be not sufficient, you put some stimulating ointment on the silk, and pull it through again; but then, it is very tormenting indeed. I cannot say that I admire these setons, I never did, and I do not think I ever shall; I should never think of using them, except where I could not put in issues.

Issues.—I hold to be the most lenient, and the most effective counter irritant; and when I have to keep up this counter irritation for a long time, I generally make an issue; what I say is, let me have a chronic remedy for a chronic disease. You may make a sore upon the skin with a hot iron if you like, and they say that it is the most lenient way, if it be hot enough, and a good deal has been said in its commendation; but there is something so formidable in this mode of producing an ulcer, that it would completely terrify some persons; I do not like it, and I never did like it. Now, if I had to make a sore upon the skin, with the intention of keeping that sore open as an issue, I should make it with a caustic, but not such a caustic as would spread all round the place on which it was originally applied, and produce a sore twice as large as I intended; this would be just as bad as the hot iron, in my opinion. Now I like the old caustic best, that is, the caustic made with the potash and quick lime, for with it you may destroy just as much of the skin as you like, and no more. This caustic might be applied with so much precision, that I would engage to sketch out upon the skin any fantastical figure that you would tell me, aye, even if it were such a one as George and the Dragon. The way in which this is to be applied is the following: You take a piece of adhesive plaster, and cut a hole in it of the size that you wish the caustic to be applied; you put this on the skin, and over it put another piece of plaster of the same shape; you go on putting one piece over the other, until you have formed a cavity in the centre to hold the caustic; you then take a piece of the caustic I have last mentioned, wet it a little, put it into the hollow on the skin, and over it put another piece of plaster. The patient says, "Oh, dear, how nice and cold it feels!" You let it remain there about four hours and a half, or five hours, and before that time he finds out that it is not quite so comfortable; you take it off, and you find that there is a good eschar produced. The skin is not a black eschar, like what it is after applying the other caustic, but nearly of the natural appearance; this comes away in a very few days, and you have a wound produced of exactly the dimensions you intended. If you wish it to discharge much, you may dress it with some stimulating application. Peas are commonly used for this purpose, but I considered that there was great inconvenience attending the daily changing of the peas, and was induced to substitute some glass beads. The wound can only

heal from the edges, and if you place any foreign body there, you prevent the process of cicatrization as entirely as if you were to cover the whole surface of the wound. What I have recommended, is a string of small glass beads to be placed round the margin of the circular sore, which become in a few days buried in the granulations, and give no further trouble. Mr. BRODIE has said, that the best plan is to touch the sore over, when necessary, with a little pure potash.

This disease, of which I have been speaking, is one which builds up unnatural structures, and destroys the natural form of such parts as it may attack; it is a disease which requires the greatest skill to subdue of any which comes within the practice of surgery; but the principal way in which relief is to be obtained, is by first attempting to lessen the increased action; and that I think is best to be done in the way which I have already pointed out. Next to this in efficacy, is the employment of counter irritation by the use of issues, and you will see in the practice of this hospital numerous cases in which this treatment will succeed; it generally succeeds, unless it is counteracted by the morbid state of the constitution. A chronic remedy, I say, for a chronic disease.

The consideration of chronic inflammation has brought me to a subject of some importance in surgical practice, which is the description and classification of tumours. Here, perhaps, I shall trespass as much against the usual import of the word, as my predecessors have done in point of nosological arrangement, although I think the classification which I have formed will be found the most useful which I could adopt. I shall exclude from the *meaning* of *tumour* any enlargement of a part which had an original existence in the human body, and confine the term to the *swellings* of such parts as had *no existence* in the *original compages of the body*. Some tumours are built up which have *no investing capsule;* these are composed principally of extension of the natural structures, by some faulty action being induced in the vessels of the part, and by this faulty action a peculiar structure is raised, altogether differing from the structures of the surrounding parts. Other tumours have a *distinct investing member*, and are principally composed of a material which is secreted or elaborated from that capsule; and this will form a very important *distinction* in the subject of *tumours*. In my next lecture I shall consider the various kinds of tumours which I have attempted to reduce to a methodical arrangement.

LECTURE VII.

TUMOURS.

I SHALL begin with such tumours as admit of the readiest illustration, as to the manner in which they form, and such as appear to possess the most simple structure.

Pendulous Tumours.—There are some tumours which hang *pendulous* from the surfaces of membranes, being attached by very narrow bases to the structures from which they hang, by what in fact is a mere peduncle. The manner in which such tumours form was first described by JOHN HUNTER, that great man to whom we all owe so much. He found, on opening the abdomen, a spot of extravasated blood lying upon the surface of the peritoneum: the blood appeared recently coagulated, and was attached by a very narrow neck, of about half an inch in length, to the surface on which it had been deposited; he attempted to account for the length of this attachment, by supposing that the coagulum of blood had adhered to the surface of the peritoneum, and that by the movements of the abdominal viscera, it had been elongated in the way before described. Now there is no doubt but that vessels would have shot through this narrow neck and have organized the clot of blood, which would, in this way, have continued to grow to an indefinite size. We have a tumour in the Museum, which hung pendulous from the front of the peritoneum; produced without doubt in the way which Mr. HUNTER described. The organization, and the peculiar actions, had been so far perfected, that a lump of fat was deposited in the centre of the tumour, whilst the neck remained fibrous and vascular.

I believe that tumours form in all parts of the body in the same way. The jelly, or the coagulable part of the blood, becomes effused either by disease or accident: vessels shoot into it; it becomes completely organized, and what was before an inorganic concrete, becomes a part of the living system. The attachment of a tumour to a part is sometimes by a slender point, and then all the vessels supplying the tumour must pass through that point; but in other cases, the vessels shoot into the tumour irregularly at various parts. The tumour

thus once organized seems to live and grow by its own powers; the future structure which it may acquire seems to depend on the operation of its own vessels.

Sometimes the structure of a tumour is like that of the parts near which it grows: fatty tumours frequently, aye, generally, are found in the adipose structure of the cellular membrane, whilst those which form in the joints are frequently of an osseous or cartilaginous consistence. This, however, is not always the case, for you may find tumours composed of very different materials from the parts by which they are surrounded. It appears that a tumour being once formed is a sufficient cause for its own continuance and increase; it irritates the contiguous parts, and keeps up that increased action of vessels which is necessary to its supply. A tumour having been once formed, increases, condenses the surrounding cellular substance, and forms for itself a sort of capsule. It is connected to the surrounding parts more or less firmly, according to the degree of irritation which it may excite.

I shall consider *tumours* as constituting an *order* in the *class of local diseases*. This order may be subdivided into genera; and I shall first speak of the

GENUS SARCOMA, OR SARCOMATOUS TUMOUR.

It has been termed sarcoma from its distinguishing characteristic, having a firm and fleshy feel. There are several species of this kind of tumour. There is one species of this tumour which appears principally composed of the coagulable part of the blood, rendered very vascular by the growth of vessels through it, without having any remarkable peculiarity in their distribution, and I have therefore called it

The common Vascular, or Organized Sarcoma.—This tumour is of a firm and fleshy feel; it is one of the most simple in its nature, and it is probable that most tumours are at first of this kind of structure. It is met with in different parts of the body; in the testis, mamma, and absorbent glands. After it has acquired a considerable size, the veins on its surface are remarkably large, and have rather a flattened appearance. Such tumours generally grow till the skin ulcerates from the distention, and exposes the newly-formed substance, which frequently sloughs and falls out. However, I always like cases, and I like them because I consider that they impress the mind more

forcibly than mere words. It is said, that "History is a mode of teaching philosophy by a recital of facts." To explain to you what I know of this sort of tumour, I will repeat the following case:—A woman was admitted into this hospital with a large tumour on the inner side of her knee, which had so spread over the tibia that it could not be felt. It continued to increase; the veins were large, and had a net-like appearance on the surface; it distended the skin very much; it ulcerated; considerable inflammation and sloughing of the tumour followed, so as to leave a cavity in it about the size of a pint basin. A copious fetid discharge followed and frequent hæmorrhage. The patient was hectic from the irritation which it produced. What was to be done? Here was a large swelling ulcerated; whether it had any connexion with the bone or not could not be decided. The patient's health was sinking, and it was considered necessary to amputate. I took the limb home, injected it, and carefully dissected the swelling. I found that it did not communicate with the bone or with the joint; there was a part of a thin capsule remaining at the lower part of the swelling, but the principal bulk appeared to be made up of the coagulable part of the blood, rendered highly vascular. Now there was no fault to be attached to the surgeon in this case; the disease was not understood, and you know that to err is human. The next species is the

Adipose Sarcoma.—This tumour is also formed, in the first instance, by coagulable lymph, rendered vascular; and the secretion of the fatty matter is the result of the peculiar arrangements and actions of the vessels. It is generally found in the cellular and adipose substance on the trunk, but sometimes on the extremities. These tumours are generally contained within their cysts, which appear to be formed by a slight condensation of the surrounding cellular structure, unaffected by inflammation. They sometimes arrive to a very great size. Sir Astley Cooper has removed one which weighed 40lbs., others have been removed which have weighed 22 and 16lbs. They appear to increase in a given ratio: they will go on increasing for many years, but they are not noticed much in the early part of their career; for supposing a tumour of this kind to be of the size of a hazel nut, why if it should increase one-third in the course of a year, it might not attract much attention; but if it should be the size of my head, and increase one-third in a year, egad! you would see the alteration plain

enough. The vessels of adipose tumours are neither large nor numerous; they are readily torn, and the hæmorrhage is very slight. There is no tumour that can be removed with so much dexterity and safety as this; it is such as young men who wish to distinguish themselves as operators should always be on the look out for. You have a patient apply to you with swelling, you make an incision upon it, open the sac, put in your finger, turn it round between the capsule and the tumour, and out it comes. A man came to this hospital with a large swelling on the thigh, which appeared to be beneath the fascia; the man gave a very clear account of the case: he said that it had been forming nearly four years; that he remembered it when no larger than an egg; that it had gradually increased, but without pain. The surgeons here would not undertake the operation; they were uncertain as to the nature and connexions of the tumour; it might arise from the bone, and so on. They all agreed that if the tumour burst the poor man must lose his life. I recommended him to call upon different surgeons in London before he returned home, and gave him a list of persons on whom he might call. Mr. Cline gave him more encouragement than any other person, and the man went into St. Thomas's hospital. Mr. Cline divided the skin aud fascia of the thigh, put in his finger, and the tumour was easily turned out; there was very little difficulty attending the operation; no hæmorrhage followed; the wound healed, and the patient did very well.

This kind of tumour has sometimes a *lobulated* appearance; it appears to be made up of a great many little lobules, connected together by cellular membranes, and these give it an irregular feel on the surface. Such tumours are sometimes separated with difficulty; they form *adhesions* to the surrounding parts by their capsules, which also become thickened. Sometimes there are cross bands of cellular structure, which give great trouble when you are trying to get out the tumour. To show you that adhesions of the capsule to the surrounding parts sometimes occasion difficulties, I will tell you a case.

A man had a tumour of this sort which hung down from his rump; it gave him little pain, and he continued to thump upon the swelling each time that he sat down. It increased very much in size, and he wished to have it removed. Mr. Long performed the operation; he made an incision upon the swelling, but found great diffieulty in separating the skin from

its surface. Ah! thought he, it is thus "bad begun, but worse remains behind." But what was to be done? Why he was in the boat, and must go on; he did go on; and after a little farther dissection he separated the skin, but expected to meet with more trouble afterwards. He pulled the tumour on one side to get at it better, when out it came, such a bundle of it, made up of little lobules, like a bunch of grapes.

There is a species of sarcoma, resembling the structure of the pancreas, which I have called

The Pancreatic Sarcoma.—It appears to be made up of irregularly-shaped masses, connected together by a fibrous substance, like the pancreas, and also resembles it in colour. It is found more frequently in lymphatic glands than in the cellular substance; very often in and about the female breast, particularly between it and the axilla, where there is an absorbent gland found, and sometimes it appears as if the gland was converted into this kind of structure; but I have frequently found it surrounded by a capsule, which has induced me to think that it is a distinct tumour. Such tumours increase very gradually, not tending to inflammation or suppuration. They are generally removed after having acquired a certain size, on account of the anxiety which they produce if allowed to remain.

A lady applied to me, having a tumour of this kind between the breast and axilla of the size of a goose egg, and as it had produced serious alarm, I was requested to remove it, which I did. When I cut into the tumour, I found it composed of the structure which I have described. I went back to the lady and said, "Madam, you may make yourself perfectly easy, it is not cancer, and it will never return." Dr. Bouttatz, of Moscow, has given an account of a tumour of this kind, which grew from a part which could not be supposed to be glandular. He says, that when travelling in Italy, he saw a man with a large tumour hanging over the side of the face, and that he was induced to examine it. It was seven inches long, and three inches and a half in circumference: it had been forming several years, and he persuaded the man to have it removed. He found that it grew beneath the conjunctiva of the lower part of the globe and lower lid, and protruded between the lids. He cut through the conjunctiva very carefully and removed it, and much to his surprise the cornea, even where it had been covered by the tumour, remained

transparent. He has given a good plate of the tumour, and his description of its substance corresponds very closely with that which I have called pancreatic. One would almost have thought that the doctor had some dealings with the old one to have described a tumour so exactly, if he had not seen it; all I know is, that there is a very great similarity in our descriptions of the tumour, as much so as you will find in the description of any other tumours. It had the ordinary characters of such swellings; it was slow in its progress, not prone to inflammation, nor disposed to suppurate.

The next species of sarcomatous tumour which I shall mention is the

Cystic, or Cellular Sarcoma.—It is a tumour made of little cells or cysts, and may be named therefore the cystic, or cellular sarcoma. There are some varieties of this tumour. It occurs frequently in the testis and ovary. In one kind of this disease the testis may be enlarged to six times its natural size; made up of a number of cells, containing a *serous fluid*, perhaps about the size of small grapes; it occurs frequently from a blow, and the sides of the cysts are occasionally very vascular, so as to admit of being injected. Sir Charles Blicke removed a tumour of this kind from the face of a boy; it was found to consist of a number of cells, containing a serous fluid.

In the testis, these cysts are found to contain occasionally a *caseous substance;* it is like cheese in consistence, has an unctuous appearance, and is of a yellowish cast. The sides of the cyst are generally vascular, but the cysts vary in size. Mr. Ramsden removed a testicle which contained the material I have just described.

Of the Mammary Sarcoma.—This is a kind of tumour which very much resembles the structure of mammary glands, or udders. If you cut into it, you will find that it is of a firm whitish appearance, like udder; and I have therefore been induced to name it mammary sarcoma. Sometimes it has a brownish or reddish tint. I think, on the whole, that this diseased structure is very liable to degenerate into an intractable ulcer, which will communicate the disease to the parts in its immediate neighbourhood. This used to be a very frequent occurrence when Mr. Alanson's mode of healing the parts by adhesion, after the removal of the tumour, was first introduced. I recollect a case in which a tumour of this kind hung

down on the inner side of the thigh of a female, and from one of the labia pudendi. The tumour was examined carefully to ascertain that there was no hernia in it, and Sir CHARLES BLICKE removed it by making an incision on each side of its base, and separating its attachment. The edges of the skin were brought together, but it left a nasty, fretful, ragged ulcer, which healed very slowly. This tumour appeared to possess no distinct capsule, but extended itself gradually into the surrounding parts. I have placed this species of tumour in this part of the arrangement, because it appears to hold a middle place between such as are mild in their character, and such as have a malignant tendency. It has been said, what a lamentable thing it is that the number of practitioners should have increased so much of late years; but diseases have certainly very much increased also. Such diseases as were very rare when I was a young man are now very common, and that which I have just been describing may be considered as an example. I shall speak now of the

Pulpy, or Medullary Sarcoma.—This is a disease which has been very frequently found in the testicle, and has been therefore called the soft cancer of the testicle. But it is not cancer, and it is found in other parts of the body, therefore it has no right to either of these terms. I have called it medullary sarcoma, because the substance of which the tumour is composed very much resembles the pulpy medullary structure of the brain. I cannot explain the nature of this disease better than by giving you the following case:—

A man had a swelling in his left testicle, which supervened upon an attack of gonorrhœa; the testicle became very much enlarged, but occasioned no trouble, excepting that which arose from its bulk. About a year afterwards a gland enlarged in the left groin, and in two years there were several enlarged in each groin; I think four. These continued to enlarge very much, and if I say that they were as large as good turnips, I shall not exceed the mark; I do not mean to say that they were as large as some turnips which I have heard of, as being a yard in circumference, but I say they were as large as ordinary sized turnips. One of these grew faster than the rest, distended the skin, and burst. The exposed tumour inflamed and sloughed till it came entirely away. The sloughing exposed its vessels; they gave way, and a profuse hæmorrhage followed. Now, in hæmorrhage arising from a surface like

this, it would be of no use to be picking out little vessels with a forceps or ternaculum, for if you were to put a ligature on such, it would cut through the vessel, and the hæmorrhage would return; but you should take a bit of lint, fold it into a little compress, put it upon the surface, and make pressure on it with your finger. The blood cannot flow then; the lint adheres in a short time to the bleeding surface, and when you begin to feel it sticking, you may take away your finger very carefully. If hæmorrhage should return, the same method must be adopted. The sloughing of this gland relieved the distended skin; granulations shot out, and the part cicatrized. A similar occurrence happened in the right groin, and again in the others, when the patient, worn out by irritation and exhaustion, died. Now the ulceration of the skin, arising from distention merely, and the subsequent healing of the ulcer, show that it is *different from cancer*, which communicates diseased action to the surrounding parts. Neither has it the hardness nor the disposition to ulcerate, which distinguish cancer. This disease is readily propagated in the course of the absorbents, and their glands readily assume this diseased action. This was shown in the dissection of the case I have just recited; the glands in the pelvis were enormously swollen; the glands in the loins were also enlarged, as were those even in the chest.

This disease sometimes extends itself by the absorbents, even in a direction retrograde to the course of the absorbed fluids; and I think it must do this by imparting an irritation to the vessels, as well as by furnishing a matter; it may, by being absorbed, communicate disease to the glands in the direct course of absorption; but the absorbents below may also become diseased. The following case will show this:—A boy was brought to this hospital with a tumour on the front of his thigh; it had been growing several months, till it reached the size of an orange. The glands in the groin swelled; the limb became œdematous; the boy's health declined, and he died. It was found on dissection, that the glands within the pelvis were very much enlarged, as were also the glands in the ham; they appeared to be of the same kind of structure as the original tumour.

There are two kinds of pulpy material found in these tumours; one is whitish, of a milky, or more dusky hue; the other of a reddish colour, or rather of a brownish red. We have specimens of each kind in the Museum.

Tuberculated Sarcoma.—This species of sarcoma is principally made up of a collection of small, fine, roundish tumours, of different colours, connected together by cellular substance. These tubercles vary in size, from a pea to a horse bean, of a brownish red, or of a yellow tint. I have most frequently observed these tumours in the lymphatic glands, particularly of the neck. I attended a gentleman, who had a cluster of these tubercles in his arm-pit; it was about the size of an egg, and its surface was irregular; this led me to examine other parts of his body, and I found that the glands above the collar-bone, in the side of the neck, were also enlarged, and on further enquiry I found that he had another tubercle in his groin. I was consulted in this case to give my opinion about sending the patient to the sea side. I met the physician who had been attending him, and I said that I really did not think the patient could live many weeks; that it would be of no use to remove the tumour, for if I did it would appear again; and that I did not see of what use it would be to a man to send him to the sea side after that; it would only be removing him from his friends, and perhaps many conveniences which he might procure at home. I saw him again at the end of a fortnight, and these tubercles had multiplied over the skin both in front and on the back part of his body; they were hard and painful, and I think I shall never forget what he said to me: "I am lying, Sir, upon hobnails, and every one of them is sensitive." The disease increased, the skin peeled off in sloughs from the surface of the glands in the axilla, and in about five weeks from the time I saw him he died.

Of the Carcinomatous Sarcoma.—This kind of tumour, on account of its peculiar hardness, is called scirrhus, while it remains free from ulceration. After such a tumour has ulcerated, it is called cancer. I shall continue to use the word carcinoma for the first stage, and ulcerated carcinoma for the second; and as this disease is not so peculiarly hard in every instance as to merit the term scirrhus, and however indurated it may be it must be regarded as a fleshy tumour; I have termed it carcinomatous sarcoma. It sometimes acquires for itself a slight capsule, but more generally it appears to be a new formation. The *boundaries* of the disease *cannot be* clearly *ascertained;* it begins in a small spot, and extends in all directions, like rays from a centre. It may be distinguished by this circumstance from other diseases, which at the commencement appear to

involve the whole of the part in which they are found. It is generally slow, but unremitting in its progress; and it excites the contiguous parts, whatever may be their structure, to the same diseased actions. In the medullary sarcoma, the disease is propagated along the course of the absorbents, but it does not excite the diseased action in the surrounding parts. In the tuberculated species, the ulceration, as far as I have seen, does not spread along the skin, but destroys only that part which covers the diseased glands.

The circumstance of a disposition remaining in the surrounding parts to assume this diseased action, points out the propriety of removing a portion of those structures immediately surrounding the diseased part. Very much has been said and written on cancer, and you should read what has been written It is impossible in a lecture room, to give you a detailed account of cancer. But I know no better definition of this disease than has been given by Dr. Baillie. He says the diseased part is peculiarly hard, that there are intermixed, firm, whitish bands, which extend in all directions from the middle towards the circumference of a carcinomatous tumour, like rays from a centre. It was this appearance which, there is no doubt, obtained for it the name of cancer. These diseased parts, projecting out of the surrounding structures, should be carefully dissected out in an operation for the removal of a carcinomatous tumour. There is no remedy for it but the knife; to forbear to operate is to consign the patient to misery.

The ulceration and self-destroying process of cancer is so horrible, that a patient gets rid of a quantity of disease upon easy terms, by having it removed with the knife. Oh! a cancerous sore is dreadful; its edges are thickened, and the surface secretes a thin irritating ichor; the sore opens like a flower, as Mr. Hunter used to say. This ichor is so irritable, that Dr. Crawford was led to make some experiments to ascertain its nature, and he said that it contained hydro-sulphuret of ammonia; he recommended chlorine as an application calculated to remove the fœtor of the discharge; carbonic acid gas has been used with the same intention, and various metallic preparations. Narcotics were found to be the best applications, and they are also the only medicines which allay the sufferings of the patient. In my next lecture I shall conclude the subject of tumours.

LECTURE VIII.

Pursuing the subject of tumours, the description of the encysted will follow those of the sarcomatous kind.

ENCYSTED TUMOURS.

These tumours are so alike in feel to some of the species which I have before described, that they are not unfrequently mistaken for them. Yet they possess sufficient characteristics by which they may be distinguished if examined attentively. I say that we may generally distinguish them prior to the performance of an operation. Encysted tumours have a much *smoother surface;* they are more *regular in shape* than the sarcomatous; and if you examine them attentively, you will find that they have a *pulpy feel.* I have known, however, many tumours dispersed which had been taken for wens; and I have known tumours removed, supposed to be wens, which, on dissection, were found to be a soft, regularly shaped sarcoma. The necessity for this distinction is not very great, as both kinds of swellings generally require to be removed.

The cysts of these tumours are composed of several lamellæ, which are sometimes very closely compacted. Sometimes they adhere very firmly to the surrounding parts; at other times they are attached so loosely, that when you make an incision upon the cyst, the swelling starts out without further dissection. These tumours, from the nature of their different contents, have been called *steatomatous,* from the matter with which the cyst has been filled resembling fat; *meliceritous,* from its resembling honey; and *atheromatous,* when the contents have been of a consistence between the two former. Wens have sometimes burst, and a peculiar structure has sprouted from the sides of the cavity. Horny excrescences have been in this way produced, of which there is a very curious example in the British Museum. Cases have been described by Sir Everard Home, in which this horny material has been shown to proceed from the walls of the cyst. These excrescences frequently take place in quadrupeds, and the na-

ture of such excrescences approaches very near to the structure of horn; but when they happen in man, they resemble more the structure of the nail. That the internal surface of the cyst secretes the substance of the different densities which I have described, I think there can be no doubt. When a wen has burst, I have seen granulations arise from the surface; but these are generally flabby, and not disposed to heal. Sir Astley Cooper is of opinion, that these tumours are formed by a distention of the sebaceous follicles, or of the *oil bags* of the skin, as I call them. He supposes that the mouth of the follicle becomes obstructed from some cause; that the secretion continues, and distends the follicle into a cyst, which eventually becomes lined with cuticle. Sir Astley Cooper recommends that a puncture should be made into the tumour, and the contents be pressed out; that this plan may be repeated whenever it distends again. If very large, they require to be removed, and the way he recommends this to be done, is to first lay open the cyst, empty it of its contents, and then dissect it out. I do not think this is so well as taking the cyst out entire, for it is very difficult to get out the cyst after it has been evacuated, on account of the adhesions which sometimes are found. Although I have known cases where, in attempting to dissect the cyst out entire, it was punctured, and a part of the cyst was left; yet by bringing the healthy skin in contact with the remaining portion of the cyst, adhesion took place, and there was no subsequent diseased action produced.

It has been supposed that these tumours are of the nature of hydatids, but that they have secreting surfaces the following case will show:—A gentleman had a wen in his cheek, just upon the buccinator muscle; it protruded nearly as much into his mouth as it did externally, and it appeared as if he had a plum in his mouth. It was so near the parotid duct, that its removal would have been attended with difficulty. The wen burst, and he applied to Mr. Hunter, who tried many stimulating applications to make the sides of the cyst adhere, but to no purpose. The opening closed, the cyst filled again, and was more distended than before. The patient came to me, wishing to have the swelling lessened, although unwilling to have it extirpated. He told me a story that he had heard, of a gentleman named Chisholm, having a tumour of the same sort cured by having strong brine rubbed on it by the direction

of Mr. WARNER, and he wished me to do the same. I told him, that the only benefit to be expected from that was, by irritating and ulcerating the cheek to let out the contents of the cyst; but that I thought that the same thing might be accomplished in a much more easy and direct way, by pricking it with a lancet. He was pleased at the proposal, and consented. I just punctured the tumour, and squeezed out the contents, and pretty stinking stuff, I remember, it was; of the meliceritous kind. After it had been squeezed out, the wound made by the puncture healed, and the sac distended again, ulceration took place at the cicatrix, and it discharged itself. The same thing has happened frequently since, and left little or no scar. I do not, however, mean to recommend such a practice generally, for I consider that it is dangerous to irritate encysted tumours: the tumour which I have described to you was of an indolent kind; but there are other tumours which are not so, as I shall presently show you.

I said that it is difficult always to distinguish encysted from sarcomatous tumours, although it may be generally done.

A gentleman had a tumour, supposed to be sarcomatous, beneath the integuments on the lower edge of the pectoral muscle. He had felt pain in it occasionally, and it had at such times very much increased in size, and made him feverish and irritable. The tumour was about four inches in length and three in breadth; it had become a source of irritation, and he consented to have it removed; the integuments were divided, and the tumour dissected out; it had adhered in part to the edge of the muscle. The wound soon healed, and the patient's health became much better than it was before the operation. I took the tumour with me, that I might examine it; I cut into it, and found that it was of a soft sarcomatous substance, contained within a thin capsule, almost caseous; it had the firmness and yellowish appearance of cheese, but was not unctuous. About three months after two other tumours formed; one above and the other below the cicatrix; but they were not attended to by the patient till they were almost as large as walnuts. Being acquainted with the nature of the former tumours, and supposing that these were of the same description, the patient being at the same time adverse to the operation as it was described to him, it was agreed to puncture the upper tumour, to press out its contents, and await the consequence. An abscess lancet was introduced, and the contents

of the tumour were found the same as in the former one. Violent erysipelatous inflammation took place; it was followed by sloughing; the inflammation extended down that side to the groin, and to the opposite side of the chest. The constitutional derangement was as violent as the local, and he died in a week.

This case shows the danger of disturbing wens of an irritable nature, or in an irritable habit. Some individuals have a disposition to form wens in various parts of the body, and they sometimes appear to be hereditary.

The greatest number of encysted tumours do well; but there are some which, when they become open, produce great, and sometimes fatal, irritation. Some cysts become not only very vascular, but have a great disposition to bleed from their internal surfaces. This hæmatodal disposition appears to be owing to a diseased state of the vessels, and was first very plainly and accurately described by Mr. Hey. When I first read Mr. Hey's book, I did not exactly understand the manner in which he used the term. I thought he had substituted hæmatodes for hæmatoides, and I recollect a remark which I made on reading it: I said it ought not to be called fungus hæmatoides, but a fungus hæmasarcoides. I afterwards saw, however, that the term which Mr. Hey had used was quite correct, and that he had described well the tendency of such parts to hæmorrhage.

It was, at the time Mr. Hey described it, a very rare disease, and in the course of a very long and extensive practice, he only met with three cases. It was so rare, that the London surgeons had never met with it; they did not know it: I speak plainly, but I speak truly, when I say they did not know it. This hæmatodal disposition in the vessels of an encysted tumour was peculiarly shown in the following case:—A young woman had a tumour formed beneath the triceps muscle, near the elbow; it continued to increase, and in about twelve months it appeared beneath the integuments on the outer side of the arm, a little above the elbow. It had a feeling of fluctuation, and as the tumour appeared more inclined to extend itself than to give way, it was considered right to puncture it in the most projecting part. It was punctured by Sir Gilbert Blane; some serous fluid was let out; he wished to ascertain the nature of the cyst, and poked his finger into it; it began to bleed; pressure was made, but the bleeding did not stop; he put in his

finger again, but the more he poked, the worse the hæmorrhage became. It was very alarming; the vessels could not be got at; the opening was enlarged, to try to secure them; the surgeons became frightened, the tourniquet was obliged to be put on, and the amputation of the arm was decided on. This was done, but it was determined on in a moment of great agitation. I took the limb with me, and examined it; I looked at the cyst most carefully; I found that there was a layer of blood lining the inside of the cyst, which coagulum had been disturbed by the introduction of the finger, and produced the hæmorrhage. I looked further, and I discovered what alarmed me very much, which was, that a part of the cyst had been removed, and left on the stump. I attended very anxiously to the state of the stump; the wound appeared to heal well, but the edges of it separated, considerable inflammation succeeded, and a fungus shot forth, and the girl died. The fungus was of a dark colour, soft, and had a tendency to hæmorrhage. Be it known unto you, that it was the fungus hæmatodes of Mr. Hey. I remember having called one morning on Mr. Pearson, the biographer of Mr. Hey, and he told me that he had some manuscript cases of fungus hæmatodes, which were to be added to those already published. He said that he had inquired of a great many surgeons in London, if they had ever met with such cases, but they all said they had not. If you please, said Mr. Pearson, I will read you one; so he began a case of fungus which had required amputation, and in which a part of the cyst had been divided. "Oh! stop," said I, "you may shut the book, I can tell you the end of that case; the stump threw out a fungus of the same kind, and the patient died." "You have seen the case then," said he. "Yes, I have, but only one, and I never wish to see such another."

There is a species of *encysted tumour*, which contains a kind of *serum* and *hydatids*, like the cysts sometimes found in the liver. Sometimes they contain a number of granular substances of a white colour, and an oval figure, somewhat like pearl barley, or the size of small currants. I consider that these swellings are enlargements of the *bursæ;* I have always found them in those situations where I know bursæ are placed, especially about the top of the thigh and hip, the shoulder, elbow, and clavicle. A young lady had a large swelling beneath the biceps of the arm, it protruded on each side of the muscle, and reached near to the elbow joint. I punctured the

swelling, and let out about six ounces of serum, with some of these little granular bodies; laid a bit of simple dressing over the part, and the arm was supported in a sling. Serous fluid continued to escape for a few days, and the wound closed. I considered, then, if I could keep the cyst open with a tent for a little time, I might succeed in preventing an adhesion of it; but the irritation it produced soon made me remove the tent. I did not dare to persevere with it. Some time after it filled again, and, instructed by my former experience, I opened the cyst, and dilated the wound to the extent of an inch and a half. It was dressed lightly with a bit of simple ointment; it healed in three weeks, and since that time there has been no further collection of fluid.

I can tell you another case of this kind:—A gentleman had a collection of fluid beneath the fascia of the ring finger and palmar fascia. The collection seemed to have begun in the palm, and had extended itself to the theca of the finger. As the fluid was nearest to the surface at this part, it was agreed in consultation that it should be opened here. An opening was made into it of about an inch and a half in length, and a large quantity of fluid gushed out, containing the largest granules I ever saw; they were as large as small grapes. I pressed on the hand to squeeze them all out, and got out as many as I could; still I was uncertain whether they were all out. I dressed the wound superficially, and ordered a bread and water poultice to be applied. Fearing, however, that some of these granules might have been left, I took it into my head, three days after the operation, to inject some lukewarm water into the cavity, but no granules returned with it. This caused great nervous irritation in the part and in the constitution. Bless my soul! I saw the error, and I determined to inject no more warm water. About six weeks after, the skin had swollen, granulations shot out and projected very much above the level of the surrounding skin; it then became afterwards absorbed, and the wound healed.

From what I have seen of these swellings, I should not be inclined to inject them, or to irritate them, by keeping a tent in them; but to lay the part freely open, squeeze out the contents, put on a bread and water poultice, and attend to the state of the general health.

To return to the subject of sarcomatous tumours, they, in common with the encysted, may be considered as edifices built

up by diseased actions, and which these actions continue afterwards to inhabit. The treatment of both encysted and sarcomatous tumours may be considered the same; it is that of lessening the temperature of the part, and applying leeches when the inflammation is active, and the use of stimulants of the kind I have before described, when the inflammatory action has quite subsided, and the tumour is of an indolent character. We suppose, in all cases where tumours are formed, an increase, and in some degree a disordered action, of the vessels which form them. In the growth and reproduction of destroyed parts, a gelatinous material is first effused, which afterwards becomes vascular; and I have adduced this process as the simplest manner in which tumours form. It is probable that all tumours are at first formed in this way, but that the peculiarities which they afterwards exhibit depend upon some subsequent diseased peculiarity. Although these swellings will frequently, by surgical treatment, remain stationary for many years, by lessening increased action in and about the part, by soothing applications and attention to the general health; yet they will often continue to increase in size; they become sources of uneasiness and anxiety, and require to be removed; and they appear best removed in the way I have pointed out.

That languid form of inflammation, which builds up new structures and alters the functions of parts in the way which I have been describing, sometimes produces other effects. Now one of the consequences of disordered action is the formation of

CHRONIC ABSCESS.

Chronic abscesses have thin capsules, do not alter the structure of the surrounding parts, nor produce any constitutional disturbance. Abscesses, on the contrary, which are the result of that violent inflammatory action which is called phlegmonoid, are attended with considerable disturbance in the neighbouring parts; the inflammation, which was violent in the centre, and had terminated in suppuration, had, at the same time, induced the deposit of gelatinous matter in the surrounding cellular substance, and in this way built up a mound to prevent the further extension of the matter. It appears that the parts immediately covering such abscess partake of the irritation by which the matter makes its way to the surface and is

discharged. Chronic abscesses, on the contrary, excite very little adhesion in the surrounding parts, the matter makes its way in all directions, and continues to do so until the distention which such large accumulations will produce, excites irritation and ulceration at some part for their removal.

To show you that abscesses are sometimes of this indolent nature, I will tell you a case :—A gentleman applied to me with a small globular tumour, just beneath the integuments on the upper part of the pectoral muscle. Its formation had not been attended with much pain ; it was soft, and had a fluctuating feel ; the integuments were not at all discoloured, but appeared perfectly healthy and natural. I concluded that it was chronic abscess : I punctured it, and about twelve ounces of healthy looking pus escaped. The orifice was closed by sticking plaster; the matter re-accumulated, but was not confined as before within a thin cyst, but spread beneath the integuments towards the axilla ; this swelling was again punctured, and the matter was discharged.

There is no doubt but that the state of the general health very much influences the state of these abscesses. The cysts of chronic abscesses are secreting and absorbing surfaces ; if secretion goes on more briskly than absorption, the abscess increases; if absorption be more active than secretion, it diminishes ; and if secretion and absorption be equal, the abscess remains stationary, and frequently is so for years. Knowing the danger sometimes arising from the opening of such abscesses, and being convinced that the state of the general health very much influenced the progress or decrease of such abscesses, I have sometimes succeeded in removing them; and to show you what may be done, I shall repeat the following case :—A gentleman, of about twenty-six years of age, applied to me, having a large swelling on his thigh. The abscess had distended the fascia from the knee pan to near the middle of the thigh ; it bulged out very much posteriorly. He looked unhealthy, was languid and irritable; his tongue was furred and his bowels irregular. I adopted the plan that I thought likely to remove the disorder of the digestive organs, but still the abscess increased, and I was obliged to make an opening into it, when about thirty ounces of pus came out ; large flakey substances often appeared at the opening and obstructed the flow of pus ; these were picked away, and out it would gush again. Towards the end some clots of blood appeared ;

and as they were large and numerous, and could be got out but with difficulty, I thought it would be better to close the wound, even before the whole of the matter was evacuated, than irritate the cavity by poking it too much. I brought the edges of the wound together by plaster, and put a roller round the limb. The cavity, however, filled again rapidly, protruded the fascia, and then remained for some time stationary. For about three weeks the patient slept in the country, and continued to take a little medicine to regulate the secretion from the liver and bowels. His health gradually improved, and in about six months he had not the least remains of the disease. Some time after this, he took it into his head to go into the militia; and what by marching, counter-marching, and irregularities attending the habits of young soldiers, his health became very much disturbed. He perceived some fluctuation in his thigh. He relinquished his mode of life, and on the restoration of his health, the abscess again disappeared.

We should then, in all these cases, particularly attend to the state of the digestive organs, and by so doing I have often seen them disappear; but even before opening such abscesses, it is necessary to allay, as much as we can, the constitutional disturbance, or bad consequences would sometimes be the result.

I shall, in the next lecture, describe the worst form of chronic abscess, namely, the Lumbar Abscess.

LECTURE IX.

When I mentioned to you the subject of chronic abscesses, I said they generally formed in persons of bad habits, in whom the general health is much disordered; but I have also to say, that when they become open they produce a great deal of constitutional irritation. In former times, this was attributed to the absorption of matter, but I do not hear of any body believing such stuff now-a-days; and, upon my life, I do not see that there is any good in discussing these points, or in taking up your time in endeavouring to prove, that the mischief arising from the opening of chronic abscesses depends

upon a specific inflammation of their cysts. I know that a great deal has been said upon this point; and it has also been said that the absorption of the matter was the cause of hectic fever, but it was quite nonsensical. The cysts of such abscesses are secreting and absorbing surfaces, therefore absorption must have been going on from the commencement, yet no hectic is produced until the abscess becomes open. There was an observation made by a person, at one of the sittings of the Royal Academy of Medicine of France: "L'accroissement de scavoir n'est pas pareille a celle-la des plusieurs choses; puisque elle soit en clarté du soleil se flectri."

I am a bad French speaker, I allow, but I have been accustomed to translate it thus: "The increase of knowledge is not like that of other things; for as our opportunities increase, there is often a great diminution in its bulk." But I do not mean to go into this now, because I might talk a long time to no purpose, and I might give you but little information; but I think it must be manifest to your own senses, that the mischief arises from an inflammation of the cyst of the abscess; the constitution must suffer more or less, as the abscess is either small or large. A chronic abscess may be opened when it does not contain more than four ounces of matter, and yet produce considerable constitutional disturbance. Now what would you think of its being left till it contained four quarts! I punctured an abscess in a man's thigh once, and let out four ale-house quarts of matter. I saw an ale-house quart in the room, and I had the curiosity to measure it. Now, if you have inflammation occurring in the cyst of an abscess equal to the surface of a gallon measure, it must be productive of more considerable effect on the constitution than when it occurs on a surface equal to that of a four ounce measure.

LUMBAR ABSCESS.

A lumbar abscess is the most important specimen that I know of chronic abscess in general; not that I wish you to believe, or that I mean to assert, that lumbar abscesses are necessarily chronic abscesses. They may be formed with considerable inflammation; but even if they were phlegmonous in their origin, and they are so in general, yet they become of the nature of chronic abscesses in their progress, and they are chronic abscesses in their termination. The matter forming in

the loins may be attended with considerable inflammation; the matter falls down into the lower part of the loins, but then it does not produce much irritation: it is, in the language of Mr. HUNTER, "an abscess *in* a part, and not an abscess *of* a a part." The surrounding parts, in which the matter is found, have no participation in the disease, but that which arises from mere distention; this is generally allowed: the matter accumulates to a very great degree, the parts are very much distended, the integuments become inflamed and ulcerate, and in this way give vent to the contents of the abscess.

It is rather a curious thing, and may be termed wonderful, that when I first began to attend to lumbar abscesses, I met with eight lumbar abscesses in succession, which did not appear to be connected with disease of the vertebral column, but I have since seen these abscesses combined with such diseases. Unusual cases may happen so frequently in succession as to appear to any individual sufficient to warrant him in saying that they are always of the character which he may have described; but more extensive experience would show him that such conclusions were erroneous, and should serve to convince us, that such cases are not to be implicitly relied on in the investigation of disease. Now I admit the truth of what is the general opinion, namely, that lumbar abscesses are frequently connected with and produced by disease of the vertebral column; and then I say, that when such a lumbar abscess becomes open, it is something more than an abscess, it is a chronic abscess, but is leading into a pile of diseased bones. The bursting of a lumbar abscess is an event very much to be feared, if not attended to with caution, for if you remember what I said about chronic abscesses in general, you will be at no loss to know why they are so; the cysts of such abscesses are very thin, and there is little consolidation of the surrounding parts. The action in the cysts is of a very low kind, and adhesion cannot very readily take place; but if by evacuating the matter you can prevent the further separation of parts, the cavity which contained the abscess may shrink very much, and therefore there is not so much to be afterwards united. I will tell you what I have proposed to do in lumbar abscesses, and I may say it is the result of certain practices founded upon certain principles.

Treatment of Lumbar Abscess.--Believing that lumbar abscess was connected with a diseased state of the vertebral column, I

would direct gentle pressure to be made on the abscess, and puncture it with an abscess lancet; continue the pressure as long as the matter continues to flow, and carefully close the opening by sticking plaster. The patient should be required to be in bed, and not allow the diseased parts to move about. What good can you expect to do if motion be allowed? you would excite more irritation and disturbance than the means you employ could do good. In addition to preserving the parts in a quiet and undisturbed state, and believing that the abscess was connected with a diseased state of the vertebral column, I would employ counter irritation; it is a safe and frequently a useful method, and tends to diminish the diseased action in the parts beneath; but at the same time I would sedulously apply myself to the regulation of the patient's health, and to the state of the functions of the digestive organs. This is saying in brief, what explains a great deal.

Try to improve the health, and I say that *lumbar abscesses may sometimes be dispersed*. If it should, notwithstanding your endeavours to improve the general health, continue to increase, and if it should so increase as to show any disposition to burst, then I would rather puncture it than allow it to break of itself. But there are numerous instances recorded in professional books, of lumbar abscesses dispersing by the observance of rest and attention to the general health. A Monsieur DAVILLE has given an instance of this, and so have several other persons. I used also to relate an instance of this thing, namely, of the disposition that there sometimes is in a lumbar abscess to disperse. I do not know whether I ought to give it, and yet I see no necessity why I should not. I was requested to see a young lady, a long way in the country, even in Wales, and in a very remote part of the principality too, who had a lumbar abscess. I went, and I found that there was a large collection of matter in the fore part of the thigh, that the spine was much distorted, the countenance flushed, and the constitution altogether in a very irritable state. I pointed out the impropriety, to the father, of having this abscess opened in the present state of his daughter's health, and gave it as my opinion, that it would be attended with very serious consequences; but at the same time I told him, that if she were in Bartholomew's Hospital I should have no doubt of her getting well; and that although she was then amongst the mountains of Wales, and had "all other appliances to boot," yet I was afraid that she

would not do well, and that the cause of it rested principally with himself. "How?" said he, "I do not understand that." I repeated to him what I had before said, and explained what I meant by saying that he would not have resolution. He said, he was determined that he would observe any plan which I could point out. I told the other medical attendants, that there was very little that needed to be done; that she should be kept in a state of quietude; that some counter irritation should be made, as by issues, and that the state of the digestive organs should be attended to; but at the same time I admitted, that if the abscess should continue to increase and distend the parts much, that it might then be opened with no more risk than if done earlier.

By this treatment the abscess, instead of continuing to increase, became lessened; her general health very much improved; and for the first twelve months I had nothing but letters of gratitude and exultation. "My daughter," the gentleman used to say, "is getting stronger every day, and the disease is gradually subsiding." After this time, the tone of the letters began to alter; he said, that he thought the issues were, by the drain, producing a state of weakness, although he had admitted that his daughter was daily growing in strength and in stature, and wanted my opinion. I told him what that was, that the only chance for her recovery was the perseverance in the same plan. Well, but from some reason or other, they got tired of the advice I had given, and sent to London for a machine, which I think they called a Spinal Stays. This was sent down, and I believe a person was sent down to apply it. The gentleman some time after sent me another letter, speaking very highly of the spinal stays, and added, that by screwing it up they could raise his daughter in height two inches. I wrote, in reply, to the following effect: "In the name of common sense, Sir, only consider what a state that vertebral column must be in, which will admit of such elongation by any artificial means." I advised them, by all means, to lay it aside, and return to the former plan.

About six years after this the patient went, for change of air, to Bristol, and there, for some phthisical complaint, took muriate of lime under Dr. Beddoes, and either by this, or something else, the phthisical complaint was removed. Let not the law alone, it was said, boast of a glorious uncertainty! Oh! this was a thing just fitted to cut a figure in medical

books: here were letters from her father, who was a Member of Parliament, to state the condition in which she was in before and after; and a case of a wonderful cure was recorded of a disease of seven years standing, which was at the same time acknowledged to have been accomplished seven years before.

Now, even in some cases where the abscess was connected with disease of the vertebral column, I should entertain hopes, but not sanguine hopes, that, by proper management, the abscess might be dispersed without breaking. When Nature sets about a process, she generally sets about it in a determinate way, as if she meant to bring it to a termination. Nevertheless, in spite of all our attempts, these abscesses will continue to increase; the parts will become more and more distended, almost on the point of inflaming the skin, and then I should open it; I should do this for an Hibernian reason, that it never might be opened. I should open it, reckoning on my own care and capability of keeping it closed.

The abscess, being emptied, would be at liberty to contract. The abscess was first in the loins, but the matter falls down and distends the fascia of the thigh; but you could not open it until it had reached a certain point of distention, because you would be in danger of wounding the large vessels, and so on. Well, the plan to be adopted is this: you direct an assistant to make gentle pressure on the upper part of the abscess, so as to distend the lower part, and then with an abscess lancet you puncture it; you carry the lancet in as far as the shoulder. I used to do this *obliquely*, but I do *not* now think that this is of *importance*. The puncture through the integuments will be about three-fourths of an inch in length, and the puncture through the fascia about half an inch. You have made the puncture, and out runs a stream of matter; you continue the pressure upon the abscess, so that this stream may be uninterrupted; presently something chokes up the opening you have made; this is the coagulable lymph, as it has been called, but I think it is the fibrin of the blood: you take this away by a small forceps, and out the matter runs again. When you find the abscess very much shrunk, you carefully bring the edges of the wound together, and desire the patient to cough; down comes another quantity of fluid, and you make your pressure on the abscess, and let the orifice be open, and out it runs again. You desire him to cough as often as you find any matter

coming down, and then you carefully close the opening, wipe the edges of the wound, and put a bit of lint over the opening, and on that a strap or two of adhesive plaster, just as you should after bleeding a man in the arm. I generally put a few straps on the part, so as to make gentle pressure, but I use no farther bandaging, and the patient can then tell if the plasters should get at all loose. The wound should be dressed every second day, and by this management, no more inflammation will ensue than if it had never been opened. Now the reason why you are to be so careful in tapping these abscesses is, not only to let out the matter, but to prevent the letting in of air. The old surgeons used to be terribly afraid of this air getting into the abscess; but the air itself does not appear to me to be the cause of the great constitutional irritation, for we see air escaping into the cellular membrane when the lungs are wounded, without producing inflammation there. Air has also been blown into the different cavities of the body, without producing any such effect. I need not detain you any further in discussing a point which has been long since set aside, but go on to a case which will be of service to relate to you.

I say that I do not think that the mischief arises from letting air into an abscess, if you have let out the matter. I remember a surgeon, who said he had punctured a lumbar abscess according to Mr. Abernethy's method; but the assistant informed me, before I saw the patient, that, when holding the edges of the wound apart by probes to let out the flakes of matter, some bubbles of air went with a gurgling noise into the abscess. I saw the patient on the third day, and found the thigh covered by a number of straps of plaster; he was in a state of very high fever, and restless; the limb was very much swollen; the fascia of the thigh was as much distended as before the operation. I desired the plasters to be taken off immediately, and out rushed a blast of fœtid air; the air had caused a putrefactive process to take place in the matter of the abscess, and out it came; it was stinking, horribly stinking stuff. Some of this putrified matter had been absorbed, and the man died in a very short time of a fever almost as bad as the plague. I was present at the opening of the body, and there were large quantities of a bloody fluid effused into all the large cavities.

Well, then, when no more matter comes down on coughing, you bring the edges of the wound, as I before said, very care-

fully together, and adhesion takes place. Now the circumstance of its being emptied and then filled by the impetus of coughing, leads us to know that it is an abscess *in* a part, and not *of* a part. After this, I say to the patient, now, the well doing of this wound depends very much on your own care and attention. I generally dress the wound every second or third day, it frequently unites by adhesion, but sometimes by granulation. The abscess not being kept open, the patient escapes the constitutional disturbance.

The matter being evacuated, the cavity of the abscess is at liberty to contract, and it does so to a certain extent; but the matter accumulates again, and I have generally observed that it does so in a given ratio. If, for example, an abscess when first punctured should contain thirty ounces, when punctured again at the end of a fortnight, it will be found to contain twenty ounces; at the end of another fortnight it would contain between thirteen and fifteen ounces; and so continue to diminish in about the proportion of one-third at the expiration of the same time.

After puncturing the abscess two or three times, there is such a diminution of secretion, that the patient fancies he is almost well, and because he can walk with a stick or crutch, he leaves the hospital, and getting amongst his friends, he indulges in eating and drinking, is seized with irritative fever, and dies; but if he could be sent into the country, it would do him much good. The patient should be kept from going about, as a state of rest is necessary for the restoration of the parts: he should be placed in an easy position, and remain on his bed. I have examined the bodies of some persons who have died after the abscesses have been treated in this way, and have found that they have been contracted to mere fistulous tubes, leading to a piece of diseased bone. Lumbar abscesses have also got well after they have been once opened, and I have found such a change in the appearance and strength of the patient as is really astonishing.

When these abscesses are allowed to break of themselves, there is such an irritation produced in the cyst of the abscess, as causes a high degree of irritative fever; the pulse is 130 in a minute; the patient gets no sleep; the mind is in the greatest state of agitation, approaching almost to delirium, and so on; and the patient generally dies. Now it is from observing the result of such cases, and, on the other hand, observing the

beneficial effect of the management which I have now pointed out, but more at large in the "Surgical Observations," that I have been induced to recommend the adoption of such practice to others.

When matter forms beneath fascia, there is no opportunity allowed for the cavity of that abscess to contract, because, as the matter distends the fascia, it separates it from the surrounding parts, and therefore it must yield to the distention; but what you ought to do is to puncture the abscess whilst it is small, if you find it disposed to increase, and not suffer the detachment of the fascia to proceed to such an extent. This is what I endeavoured to explain, by saying, that an abscess may be opened when it contains four ounces, or it may be allowed to remain until it will contain four quarts.

When speaking at first of chronic abscesses, which are in many points like phlegmonous ones, I said that the cysts of such abscesses were of a villous structure, that they were secreting and absorbing surfaces, and that they sometimes were dispersed by the absorbents taking up the fluid in a less time than it was secreted. Now as this is the case, what is the rule for the puncturing of such abscesses? If an abscess be enlarging in its circumference, if it be not becoming pyramidical, or tending to burst, you had better puncture it, and bring it to a crisis, because you would, by so doing, prevent any great extension of the mischief which the separation of parts would occasion; but it should be done in the cautious manner I have pointed out. But if, on the other hand, an abscess become pyramidical, and tending to burst, and if it be not enlarging in its circumference, why, you had better leave it to Nature's process, and lay a bread and water poultice over the part. I cannot say that I like to interfere in any of Nature's processes, except under circumstances of necessity, for Nature does all things in the best possible manner. If you puncture an abscess when it does not want a puncture, why, the wound heals, and the matter collects again. Nature often manifests a disposition to bring an abscess to the surface in one direction; but the surgeon says, "No, no; that is not right; we must make an opening somewhere else:" but Nature is generally right, and the opening which Nature makes does not heal until the sides of the abscess are consolidated.

Now I say this is the rule, but I have sometimes been tempted to act differently in cases where the absorbent glands have

suppurated in the necks of children; for if such abscesses are allowed to break, they generally leave a scar, which is very unpleasant; and persons say, Oh! such a person is scrofulous. But when the fluid can be distinctly felt, and as soon as the least blush of redness appears, it is best, I think, to open it with the point of a lancet, and let out the contents. The place generally heals well, and little deformity is left. I have seen these abscesses when opened prematurely, in an irritable and scrofulous constitution, produce more ulceration than would have been the case if left alone. I opened an abscess in the neck of a little girl, and discharged two ounces of matter from it, but no scar could be seen.

Sometimes these abscesses produce fistulæ; and what is the reason why fistulæ do not get well? What, but that the cysts of the abscesses with which they communicate are in an unhealthy state; the constitution is also deranged; and there are no healthy granulations produced to fill up the cavity. Now, I believe this is simply the fact; but we are always trying to make diseases well; we want to lay open fistulæ; we are not content to wait a little till the action in the cyst shall be changed; we want to excite action, and we inject stimulating fluids, and so on. I have often seen little fistulæ leading from suppurating glands in the groin laid open, and when they have been laid open, their edges have remained callous, uneven, and indisposed to heal. All this sort of practice is finical and unnecessary. I say, that all you have to do is to lay a bread and water poultice on the part, allow a little time, and restore the disturbed state of the digestive organs. I know that what I am now telling you is contrary to the general opinion of the profession; but I feel that I have good reason for saying what I do, and I am satisfied that, barring the cases of *fistula in ano*, the observations which I have made are perfectly correct. The state of the fistulæ is dependent on the state of the cyst of the abscess, and the state of the abscess is dependent on the state of the general health. Many surgeons have an idea too, that it is necessary to make counter openings in cases where the abscesses are extensive; but I had a good lesson on this subject when I was a young man. I attended a young lad who had a diseased hip; it suppurated, and there were large collections of matter deep amongst the muscles of the thigh; the abscess ulcerated, but the surgeon said, here is a collection of matter behind, we must make a counter opening.

The counter opening was made, but mark the end of it; I say, mark the end of it: the new opening was soon closed, although made in the most depending part of the abscess, and the old one remained open.

I have now described to you the nature, and some of the consequences, of chronic inflammation. I have said that it builds up new structures, that it raises interstitial structures, and that it forms extensive abscesses.—In my next lecture, I shall describe to you some other kinds of inflammation.

LECTURE X.

IRRITATIVE INFLAMMATION.

I AM about to call your attention to a species of inflammation, which I describe as the irritative; but I do not know whether I am right or not in making the distinction; you must judge of it for yourselves: it is what the surgeons have called *Erysipelas*.

It is necessary then to trace the history of erysipelas, to know whether the distinction be properly marked, and whether it can be properly supported.

There is an inflammation which is remarkably disposed to produce mortification: it is an inflammation which is red and painful, frequently occasioning the death of the part which it attacks. I say also, that it is progressive in many instances; it comes on in fits, and produces a great deal of sloughing; it subsides a little, after an interval it comes on again, and the part perishes. Now I say that this should not be called erysipelas, which is a disease that is much diffused, and it has no definite margin. It may produce sloughing, yet it is rare to find sloughing in that inflammation, which I should call *distinct erysipelas*.

I say that the same disease produces extensive mortification, especially in the legs; and I shall have to give you a history of this mortification, when I shall speak presently on that subject. Inflammation shall come on, a patch of mortification

occurs near the ankle, the part sloughs; the inflammation returns again, and another part sloughs, and this extends up the limb. It is an inflammation varying in its nature, but very painful, which is extremely prone to terminate in mortification; and I believe it may and ought to be contra-distinguished from erysipelas.

There are many inflammations of a constitutional nature which do not terminate in mortification. Now I question whether what they call erysipelatous sore throat ought to be called so; it is a painful sore, not diffused, not spreading, not travelling about like erysipelas.

The inflammation of which I am speaking sometimes attacks absorbent glands, as those in the groin, and comes on so violently as to produce a degree of irritative fever; the gland becomes excessively irritable; if you touch it with the finger he shrinks away. It is very painful; his pulse becomes very quick, his skin hot, the secretions are checked, and he is in that state which I have before described as the *irritative fever*. There is great prostration of strength, the patient cannot sleep, his bowels are irregular, and so on. Well, you apply a bread and water poultice, he lies in bed, you use means to tranquillize him, and he gets better. The parts become indolent, the bubo may suppurate, and there is nothing extraordinary in that; he gets well, and goes about. It is nonsense to say that this irritation is the result of suppuration in the glands.

The more you attend to disease, the more you will be convinced that it is the state of the constitution that determines the character of local disease; and that local disease may be also traced as the cause of bringing on a proportionate constitutional derangement. A kind of corresponding constitutional derangement is produced, and I have already given you examples of this in the history of phlegmonous inflammation: I said, that when the inflammation was phlegmonous, there was inflammatory fever; a bold and decided fever, attended with a consciousness of strength; but the minute the secretion is established this irritation is relieved, and there is a cessation of all the fever. When the wound has been discharging properly, when the distention is taken off from the surrounding parts, when the parts are flabby about the wound, when the patient has become weak, relaxed and debilitated, ah! there is a kind of inflammation produced, which they call erysipelas. I might call it irritative inflammation if I chose. As soon as

this state of things happens, then there is irritative fever. I call the inflammation in the cyst of a lumbar abscess irritative inflammation. Has any man a right to call this erysipelas, I would ask? While this kind of irritative inflammation continues, there is irritative fever, which frequently terminates in hectic fever.

I say that the character of the inflammation very much depends on the state of the constitution, and we have accordingly either a decided development of heat, and a quickness of the pulse, if the inflammation be active; if it be of the nature of phlegmon, we have a sneaking, disguised sort of fever; if the inflammation be of a low and weak kind, are not the sanguiferous and secerning systems disturbed? How can these be so, but through the nervous energy. The local irritation produces, by this means, a general disturbance of the whole frame; the sanguiferous, secerning, and nervous systems are all disturbed, and nothing is going on right; the tongue is furred, the liver does not secrete, the bowels are constipated, and the skin is hot. They will call all this uproar, *fever*.

Now all these things have I seen, and I say that this sort of inflammation may be tranquillized, or it may be roused to suppuration. It does not necessarily terminate in mortification, although, when excessive, it is very liable to do so. I know the foible of my own mind; you may call it what you please —fancy, theory, speculation, or any thing you like; but what is it? Why, it is an honest endeavour to try to find out the nature of things by continued observation, and so on. Call it, I say, what you please; imagination, fancy, or speculation; it may have led me to think as I do, and what I think I say; that there is not only some sort of disturbance *locally*, but I say that there is a disturbance also *generally*. It is that sort of inflammation which Mr. Hunter described as *violent* inflammation, with little power, and it is what I shall call *irritative* inflammation. I can say no more about it; and I shall, in the next place, go on to speak of

ERYSIPELAS.

You will see how far the distinction which I have made can be supported. Now as to this said *erysipelas*, we can only describe it where we see it on the surface of the body. The skin is red, as red as can be; you touch it with your finger, and it is pale in a minute; it seems to be an inflammation of the minute

vessels only, the larger arteries not partaking of the inflammation; and there seems to be no impediment to the transmission of the blood from the arteries to the veins, for there is none of that resistance which you meet with in phlegmon. There is increased secretion too going on, totally unlike what happens in phlegmon. You know the subjacent parts are œdematous; there is no defined margin, no circumscription of the disease, no adhesions to check its progress, and it is disposed to spread; egad, it is a travelling disease; and, as I say, the parts are disposed to swell.

I remember a man who was attacked by this sort of inflammation in his leg, it travelled up that leg, up that side of his body, crossed over his chest, and went down the other side, even to his toes, and then, you know, it could go no farther. One part was attacked by the inflammation, as it ceased in the adjacent part. I think, then, that it deserves to be called a travelling disease; it may be said to do this by continuity of surface; but it seems sometimes to jump about from one part to another, and this is what they have called a *metastasis,* or a translation of the disease. Erysipelas sometimes comes on without any local excitement, as if it were from constitutional causes; this I shall call *medical erysipelas.* At other times it comes on from local excitement; something teases and provokes the part to inflammation, and this I call *surgical erysipelas.*

Now, as to the medical erysipelas, I will tell you what the doctors say about that; they say you had better not meddle with it at all; you may powder it a little if you please, but do not attempt to repel it; for if you do, you will have it affect some other part, perhaps some affection of a vital organ may take place. Now this is true enough, for if you try to drive it away, if you put on your cold washes, or play any of your surgical tricks, why you have a metastasis, as they call it, and the man dies.

You cannot suppose that there is an erysipelatous inflammation to be found in the brain. Oh! no, there is no such thing; but you may say that there is the *mort de la téte,* the *mort des pneumons,* or the *mort du cœur,* if you please. This repulsion produces sometimes suffocation, and then is there any difficulty in accounting for it? why, I should think not; the man dies, in that case, for want of breath.

The occurrence of erysipelas shows something very wrong about a man's health; let something be applied to provoke a

part a little, let there be an increased action produced, and in a healthy man you will have a phlegmon; you will have none of this sneaking inflammation, which I am now telling you about. The general health was disturbed before the functions of the digestive organs were disordered; the person has nausea, and loss of appetite, and the actions of the different parts are not healthy;—and that settles it. It is produced by the state of the general health, and it acts upon the general health. I should put a little bread and water poultice on the part, and try to soothe the disturbance which has been produced.

But, as to surgical erysipelas, this is what we have most to do with, and here there are many things to be done; but the principal one is to do away with the causes, on the part of the constitution, which have produced this peculiarity of local inflammation. But, oh! what are they? I should be glad to ask who can tell? But if I do not know exactly the state of the part which produces this peculiar inflammation, I know the state that the constitution is in, and on what that state principally depends. (I am never afraid to speak what is in my mind, and I think I am right; but, if I am wrong, I shall be very happy to have my errors pointed out and corrected.) I'll be hanged if erysipelas is not always the result of a disordered state of the digestive organs. I never see it come on if the digestive organs be right, and it goes away as soon as they are put right. Now what is the medical practice? They powder the part a little, and they give bark, and so on. But what else has been done? It is not I alone who am so mad as you may think me, in saying that erysipelas depends upon the causes I have mentioned; there are others who think much the same; there is Richter, the German surgeon: they do not say that he is crazy too, I should think. He says that it arises from a *gastric irritation*. Dessault, a French surgeon, says, that it is the result of a *bilious cause*, and he has recommended tartar emetic. I do not know what they mean by the gastric irritation and the bilious affection, but I know that there is a peculiarity of the general health on which the disease very much depends, and that it is wrong to repel it. For whenever a disease comes on locally, without immediate injury, it is, as it were, a fixation of the constitutional disease; it is a focus in which the diseased action is to be concentrated; and, if it be seated in an unimportant part, in the name of God, let it go on there. When you come to consider the essential nature of

local disease, you will find that it is essentially modified by changes in the general health. Well, this erysipelas often occurs in persons who have gout and rheumatism, but would you repel it in them? I recollect a fine, healthy, good, honest fellow, who had an attack of erysipelas in his hand, (indeed he was a servant of mine,) and not liking this inflammation about his hand, he plunged into cold water; he was soon after seized with insensibility, he fell down in a state of torpor, and soon afterwards died. Many brains and lungs have I examined after persons have died of this metastasis, but were there any traces of inflammation in these organs of an erysipelatous kind? Oh! no; it could not be expected. Well, I say it is the result of some previous *irritation* in the *digestive organs;* but what it is, or how to put it to rights, I do not know. Oh! if I could but put all the disorders of the digestive organs to rights, I should discover a better thing than the philosopher's stone.

When the animal and vegetable matter does not digest which gets into a man's stomach, they produce there great irritation, and the secretions of the intestinal canal, and of the liver, become disordered. The only way to get rid of this irritation, is to clear out all this stuff. I know of nothing better than to excite the action of the liver gently, and not tease and irritate the stomach and bowels; you want to get rid of an irritation, and not create one, by teasing and worrying a man's guts out. If the man have a dryness of the skin, you may give him a little antimony; but it is horribly absurd to be cramming a man with physic. I say it is horribly absurd, and I have no patience to hear and see what I do, as if medicine could cure a disease. Now what are medicines? They are the means which we employ to correct faulty actions in the various functions of the body.

Going, one day, round the hospital, I saw a patient who had an ulcerated leg, as if it had been of ten years' standing. What do you call this? I asked. O! said the dresser, it is a case of erysipelas, and he only came in last week. Good God, said I, is it possible? True, I assure you, Sir, the leg has sloughed, and that has made it in the state in which you see it. I was induced to ask the patient what his previous health had been, and when the complaint first appeared. He said that it came about a fortnight before, first in his throat, and he could not swallow; then the pain went into his loins, and he could not walk; his appetite became bad, and his bowels wrong, very wrong: and then, Sir, said he, it fell into my legs. Here is

a very distinct case. Well, I said, you must take care not to offend the stomach, keep the bowels regular, and apply something soothing to the part. Oh! Lord, what an excellent thing that is, not to offend the stomach; but if this be attended to, and the parts kept tranquil, the patient will generally come round, if there be no inveterate disease.

To return to erysipelas; it is said to terminate in mortification; but, upon my life, it is a very rare occurrence, as I think. It is a question, whether the inflammation of which I was first speaking, and which terminates so readily in mortification, ought not to have a different name, and this is the point with which I set out. Erysipelas may go away again: Does it ever suppurate? I should like to know that. All I can tell you is, that the inflammation which takes place under the erysipelatous part is very often followed by suppuration, but whether erysipelas itself does suppurate or not I cannot tell.

Here I have been in the habit of selecting a case which is intended to shew the principal circumstances of the disease as I have described it. It was the case of a gentleman who was a pupil here; and a finer, handsomer, fellow I never saw in my life, the present company always excepted. He went into the army, and afterwards was with Sir JOHN MOORE, at Corunna; there he shared in the misfortunes of his countrymen; and before his health was perfectly restored, he was ordered to Walcheren, and was there attacked by the fever which was so destructive, and which was followed, even in those who recovered, by great disorder of the digestive organs. In many cases, which I had the opportunity of examining, abscesses in the liver and spleen were found, and in others, disease in the digestive organs quite enough to kill them. He recovered, however, from the effects of the fever, and went into the country. About a year and a half, or two years after, he came to London on some regimental business, and stopped at a coffee-house in St. James's-street. He was laid up there by an attack of low fever, and erysipelas came on in his side; he was attended by some army medical men, who checked the inflammation by some means, and the fever was very much increased; the erysipelas returned, and the fever in a great measure left him. His tongue became very much swollen, and his head very painful; he could not speak to the persons about him, but he made a sign, as well as he could, for them to give him pen, ink, and paper, and he wrote on it to request them to send

for Mr. ABERNETHY, at such a place. I immediately went, and found him with his tongue in the state which I before described; he could not articulate; his skin hot and dry, and his pulse quick. I saw that there was something terribly wrong about his stomach and bowels. I asked if his bowels had been relieved; yes, Sir, was the reply; they have been properly attended to. This was not satisfactory to me, and I thought I might obtain the information I wanted from the nurse; I asked her how they appeared. Oh! very proper, very proper, Sir. The gentleman heard what I asked, and made signs for paper, and wrote, "quite black and horribly offensive." I thought that by this time I had seen quite enough of my company to see that they were firm and resolute fellows; but I did not like to contend with armed medical men, so I said I would request a physician of the hospital to see him; but, as it was probable that the physician could not come before the morning, I would advise him to take half a grain of calomel and five grains of jalap every four hours, mixed in a little treacle; this was to be put on a spatula, wiped upon the tongue, and then sucked down. I recommended this to be done every four hours, until the bowels were freely discharged, and left him, saying that I would send a physician early in the morning.

These are very good medicines, this calomel and jalap, and this is also a very good way of getting them down; especially in tetanus and with mad folks; you may push a gag between their teeth, and then put the physic upon their tongue with a spatula; shut their mouths, and, egad! it must go down. It does not seem to produce watery stools like salts. Well, but to return to the history of the case; I called the next morning on our patient, and was very glad to hear him say, when I came into the room, Good morning to you, Mr. ABERNETHY, I am very glad to see you, I am much obliged for your advice, and so on. Ho! he had his jawing tacks on board, as a sailor would say; how was this brought about? He proceeded to tell me, that during the night he had several very copious alvine discharges, very black and offensive; that the irritation had subsided, and that before morning he was able to use his tongue. About ten days after, the physician called on me, and said, I wish you would see our friend; there is a large collection of matter under the integuments on his side. I saw him, punctured the abscess; let out about three pints of matter, and he got well very rapidly.

I now go on to speak of other inflammations, and next of the common boil, or faruncle; but there is also the carbuncle or anthrax. I have put these diseases under the same head, because the inflammation attending them appears to be very much alike. The inflammation in all is attended with a sense of heat and burning; the inflammation in all has a tendency to produce sloughing in the surrounding cellular substance. They seem to resemble each other so much in their circumstances, that I think we may consider them as varieties of the same disease.

THE FARUNCULUS, OR BOIL.

Shall I give you the history of a boil? Well, I will do so, and then have done with it; although I have no doubt but that many of you know it already. A person has a little heat and itching in his cheek, he scratches it a bit, and, instead of its going away, it continues to swell. It swells till it becomes the size of a walnut. There is next a thickening of the skin; by degrees it ulcerates in one or more points; if in one place, it enlarges; but if in more than one place they run together, and a gap is made in the skin, and out comes a sloughy substance mixed with pus; the part gets well directly; it is astonishing to see how rapidly the place fills up. It seems as if the inflammation in the disease was rather peculiar, as it causes the death of the cellular substance beneath the skin of the inflamed part, whilst the skin, from having more vitality, does not slough. This detached cellular substance with matter in its interstices forms the core, as it is called; and the inflammation would subside if not kept up by this irritation, which must be discharged by ulceration. It is a disease to which young persons are much more subject than adults. I have seen very few in old persons, rarely beyond the middle period of life. They appear also more frequently upon the front of the body than on the back part, and on the trunk more frequently than on the extremities. They occur in parts which are nearest to the centre of the circulation, and are often produced by riding. I have known young men who have joined volunteer regiments, and from not being accustomed to ride, have, after a hard day's duty, boils upon their buttocks as big as tea cups.

LECTURE XI.

CARBUNCLE.

Carbuncle generally arises after some disorder has taken place in the digestive organs, and it is an inflammation which generally attacks a particular structure. Mr. Hunter says, that he never saw carbuncle in the hospital but once, and that was in a gentleman's servant, a butler, who usually is a person who eats and drinks as much as he can. He finds, perhaps, that there is a part of his body getting very sore, generally on the fore part; he scratches it a little, and he finds that the skin at that part becomes a little thicker, and the more he scratches it the worse it becomes; the swelling continues to increase, remains of a white colour, and there is considerable pain attending it. Now, what can I say about it? All I can say is, that it becomes purplish, and the patient becomes very much out of health; he is what they generally call unwell. The skin covering such a swelling has a brawny hardness. Now, as the inflammation proceeds, ulceration takes place at one or more openings; the matter contained beneath, with the sloughs of the cellular membrane, make their escape. The matter being discharged the health mends, the aperture fills up, after the sloughs of cellular membrane have been removed from the subjacent parts. Sometimes sloughing has taken place in the integuments over these carbuncles, to a sufficient extent to admit of the matter beneath being got rid of; but these cases are few in number. An opening by the surgeon generally becomes necessary, and the part is immediately improving. I have seen many surgeons, however, hesitate to make an opening into such swellings, but they have been obliged to do at last what they have been reluctant to do at first, and the patients have done well. But as to the propriety of operating, I should never feel any hesitation to do it; I should just take a double edged knife and make a simple incision upon the part. By doing so you bring the disease completely to a crisis; a discharge takes place from the surrounding cellular substance, the tension of the skin and the irritation in a great measure subside, and the disease spreads no longer.

Now I think, undoubtedly, that this is the most proper mode of treating the disease; formerly, it was recommended to make two incisions, so that a crucial aperture might be formed, but I do not know that this is necessary. I generally make a straight cut on the carbuncle longitudinally, and, in all instances which I have seen, I have found it amply sufficient. You bring it, I say, completely to a crisis, so that it spreads no more.

Mr. HUNTER says, that he has known people have carbuncles after they have met with some injuries, as blows on the legs and other parts, by which the system has become disordered, and who have done very well; so that it appears that there is no uniform state of constitution which produces them, but that they arise from a disturbed state of health. He seems to recommend the bark and soda; he thinks that they do good in this disease. I have no doubt that the *cause* of the complaint is a disordered state of the digestive organs, and of the mode of treating that disorder I have already spoken. If I could set all the digestive organs to rights; if, I say, I could set this disorder to rights, egad! I should do better than if I discovered a philosopher's stone, and have the knowledge of the art of prolonging life.

ANTHRAX.

With regard to what has been called anthrax; the definition of it is not clear as it is given in surgical books. The swellings supposed to be anthrax have always begun, as far as I have seen, in the absorbent glands, such as those of the axilla and neck. There is considerable tumefaction and swelling, a purplish kind of tumour, a dusky red with a purplish hue; it is also sometimes of a blackish purple; the part becomes hard, and afterwards has a quaggy feel; the part is opened, and a quantity of cellular substance, mixed with matter, comes away, like what happens in carbuncle. Now I conceive that it is to be treated in the same way as carbuncle, and I should deem him to be a very intelligent and decisive surgeon, who should at once discover anthrax, and treat it in this way. Now I attribute this inflammation, as I do that of erysipelas, to a disordered state of the digestive organs. With regard to carbuncle, its very history seems to indicate that it occurs in people who have lived luxuriously and somewhat intemperately.

I have never seen it but in persons who have lived very high, or who have much disordered their digestive organs.

Now then, I say, that there is a peculiar state of constitution produced by some previous disorder of the digestive organs, which causes this peculiarity in the local inflammation. I might tell you many cases to show that this is the case. I shall select the following one :—There was a man, who had made his fortune by plastering, whitewashing, and so on, and he took it into his head that he would be a gentleman ; this was to be done, according to his idea, by eating and drinking as much as he could, and by not doing any work. He found, after a short time, that his appetite became bad ; he could not eat so much as he did when he was first a gentleman, and this very much grieved him ; he found, soon after, some troublesome swellings form about different parts of his body, one of them was very large, and I was consulted ; I found it to be a very large carbuncle, and I opened it. I told him that he must live more abstemiously, take more exercise, and attend to the state of the digestive organs. He did so, and the carbuncles got well. Three years after, I was sent for again to this person, and I found that he had been inattentive to the regulations which I had laid down for him, and that he had other carbuncles forming in different parts of his body. I told him what I thought of the matter ; and he then began to tell me that he paid his apothecary so much every year for medicine, and that he thought that his health ought to have been kept in good repair ; but, said I, you must recollect that what might be very good reasoning in repairing a house will not be so here ; for a man's body is not often in the best state of health, because he has a large portion of materials put into it. Oh ! his digestive organs had been long out of order, he complained of his waistcoats getting tight ; there was serious visceral disease, and he died. A large quantity of water was found in his belly, and he had a diseased liver.

I recollect another case, where the person was not a *gourmand* ; he did not eat too much, but he drank too much ; his health was very much disordered, a large carbuncle formed upon his shoulder ; this produced a great deal of constitutional irritation ; he sprung up one night in bed, quite delirious, and afterwards fell into a state of insensibility and died. Here was evidently cerebral effusion, but how induced I cannot tell. It was not like an attack of apoplexy. It was a cerebral effusion,

induced by stomachic irritation. Undoubtedly there is sloughing in boils, in anthrax, in carbuncle, and you may say that erysipelas is sometimes disposed to mortify; but I want to speak of mortification as a distinct subject.

MORTIFICATION.

Mortification is the want of life in a part. But mark you how differently this state may be induced; parts may die from exhaustion produced by inflammation; they may die from mere weakness, mere want of circulation; they may die from a great variety of causes. Mr. HUNTER said, that mortification is not simply the death of a part, for parts may die to a certain extent, and yet not mortify. Parts which have little power of life, mortify; it is a vital process, it is the last act of life; it is the conversion of a part into a slough. If you put on a caustic you exhaust the vital principle of a part; what you have touched by the caustic comes off in the shape of a slough; the part is dead. It is not only deprived of life, but it is changed in appearance; it is a black, fibrous, homogeneous mass; you cannot dissect it; you find that its original structure is altered; this is mortification. Now, considering it to be the last act of life, we may suppose that it is the result of much increased action. Gangrene has been called by different names; there is the gangrena sicca, the gangrena humida, and so on. Gangrene is sometimes attended by vesication, but vesication is sometimes the result of phlegmonous inflammation. There has been of late much talk of mortification of limbs, in which there is no sloughing, but I do not know why you should call that gangrene; the parts are dead, and you might as well say they are mortified. I have been accustomed to consider mortification as the conversion of parts into a slough, and when I speak of it in this way, I speak as a surgeon. I hear, however, other persons talk of the pale gangrene, and I have various opinions respecting it. They say it is the effect of the circulation being stopped or deficient, and they say this opinion is supported by dissections. I hear them say, also, that when the veins are affected, then we are likely to have the gangrena humida, and more likely to have effusion; all this may be true. All this I say may be true, but I think we have not sufficient experience to warrant us in asserting that it is so.

Mortification often takes place in a limb suddenly, after

violent inflammation, with great pain; but I have seen instances in which it has occurred without much pain. Mortification sometimes happens from simple weakness in parts, especially in the extreme parts of the body. I have seen patients brought into this hospital who have been exposed to hardships, who have suffered frequent fastings, who have been exposed to inclemency of weather, and who have had considerable mental anxiety. I have seen such persons lose their fingers, toes, lips, noses, and ears. There is, in such cases, a diminution of the vital energy from cold in the parts remote from the centre of the circulation, and as the nose is very much exposed to the influence of cold, it very often mortifies.

Now it is necessary that you should attend to mortification as the result of cold, on account of the treatment which it demands. This is not uncommon in cold climates, where the inhabitants have to travel in sledges over the snow of the different villages in which a little society is to be found, and where, as they say, they very much enjoy themselves. Soon after the person has come into a heated room, one of the company says, Your nose is frosted, Sir; he immediately leaves the room and rubs it over with snow, for he knows, that if he should remain in the room after this admonition had been given, he would atone for it by losing his nose. But such is the case, that as parts will sometimes die from weakness, so a part from being excited beyond its powers, will produce death. Now the object in the treatment is plain; if you apply powerful stimuli to a part which has been very much weakened, you will cause a destruction of it; the heat, for example, of a room after a man's nose has been exposed to the influence of a very low temperature, is sufficient to destroy its vitality. The stimulus, then, necessary to raise the vital energy from the low condition in which it may be found, should be applied gradually; the friction with snow is very proper, and should be practised in like cases. Ice, when applied to hernia, has produced such a state of parts as might be called frost bitten. Such things have happened, such things may happen, and therefore I say that you should apply stimuli to such parts, but that you should do so by degrees. Excess of action is very likely to produce mortification, especially when connected with that state of system as is found in irritable or erysipelatous inflammation.

If mortification happens from weakness, it is very often to

be found in persons in whom the whole system is in an irritable state from the great disorder of the digestive organs; and if it happen from excess of action, stimulants must be equally improper. You must soothe the part by applying the bread poultice; do not stimulate it, there is too much action in the part already. It is said in books of surgery, that stimulants are necessary, and that scarifications are necessary. We do not make applications to the dead part, but we ought to make them to the living. But I shall state to you a case in which scarifications are called for, that is, when erysipelas attacks a man's leg, and there is effusion beneath the integuments, the cellular membrane sloughs, and the disease spreads. You ask, "*cui bono?*" Why, if you cut through the skin, you relax the surrounding parts; this takes off the tension, and very much contributes to diminish the inflammatory process. By cutting through the skin, you cut through the surrounding cellular substance, and allow it to make its escape. Do not stimulate the parts, however, I repeat, but put on a mild poultice. I will venture to say, that I have seen several lives saved by this practice, by scarifying the part, and letting out the collection of matter, and the sloughs of the cellular membrane. I do not think, I say, that stimulants are good applications; it is not reasonable that they should be; for it is too much action that has killed the part. Be good enough to mind that I do not speak of the mortification of burns; unquestionably there are many cases in which stimulants are very necessary.

To call a mortification a nervous mortification might at first appear to a medical man a little extraordinary, but you may be easily convinced on the subject, viz. that mortification does not exclusively arise from alteration of the function of the nerves of the part, but partly of the sanguiferous system. I consider this subject to be one calculated to excite considerable interest.

There is a very peculiar state of health which leads to a peculiar local action, which I am about to describe. In that state of health, sloughing takes place very readily, as it seems to be attributable to the state of the general health. Now I shall not be able to explain this subject in a long time, indeed I should never do it so well by any other method as the relation of the following case:—

The man had an inflammation about the ankle which

sloughed. His pulse was strong at first, and regular; he was very uneasy in mind, and the functions of the digestive organs were very much disturbed. The patient said, that every thing which he took lay like new bread in his stomach, he became very fretful and angry, and I attended him with a physician, and his usual medical attendant. I said, that I considered the cause of the sloughy state of the sore was to be found in the disordered state of the digestive organs. I believe they considered me a very peculiar personage for saying so, and I ordered purgatives. I will tell what happened. When he got on the close-stool, he discharged a volley of shot, about sixty as he told me, he was very much relieved, and fell into a tranquil sleep. You know, I dare say, that it is no uncommon thing for jockeys to give the running horses of the opposite party shot in their corn; this artifice very much disturbs the digestive process, and their respiration becomes very much impaired. A gentleman that I know had a very beautiful horse, and expected the horse to win some race; he lost it, however, and he lost another. Very much surprised at this, he was one day at Tattersall's, and there mentioned the circumstance of his horse being off his feed, being short in his wind, and so on, as the time for running drew near, whereas he was in perfect health before. Tattersall then told him what these fellows were in the habit of doing, and on inquiry, he had found that they had served him in this way. The boxing men say, that wind is strength and strength is wind. However, to return to the case, the sloughing extended over one-third of the leg, and the disturbed state of his health was manifest by the disordered secretion from his bowels. The medicine which they had before given him to procure sleep, was of no avail; but what could not be accomplished by opium before, yet after the operation of the purgatives was effected without opium. I found him, when I called in the morning, in a tranquil sleep, the skin covered with a gentle perspiration, and the pulse more tranquil than it had been before. I congratulated the patient, and said, "Now, Sir, your troubles are at an end." "What! Sir," said the physician to me, "you are a fool." "Oh! am I; that I think a little time will show, and then I do not care what you may have thought me, provided I am but found to be right, and then, I dare say, the patient will not think me a fool: ha! ha! Now, I said, I will tell you why I said that to the patient, it was to give him a better medicine

than you could give him, it was to give him the cordial hope. Granulations sprung up in a day or two, cicatrization rapidly took place, and the leg healed.

The forms of disease which I have been mentioning this evening may be traced to that state of health which, in the early part of these Lectures, I mentioned as being the fruitful parent of a very numerous and dissimilar progeny of local diseases; a state of health produced through the reciprocal action of the digestive organs on the nervous system, and the nervous system on the digestive organs.

I remember a man who had a little slough on his leg, which was rather longer in separating than usual, and it was proposed to stimulate the part. Lord bless my soul, said I, all you have to do is to put on a bread and water poultice, let the foot rest on a stool, and attend to the state of the bowels. The effect was only tried a little time, the slough separated, and the patient did very well. I have known cases where the sloughs have been three or four months separating, and in that state of the general health which I have been describing, it is of no use to stimulate; I have known stale-beer-ground poultices applied, but the sloughs have only increased. Keep on a mild poultice, and allay the constitutional disturbance.

As to the *mortification of the toes*, which happens in *old people*, it is generally dependent on an ossification of the small arteries, the blood, of course, is prevented from circulating equally through the parts; sometimes it is attended with pain, and sometimes not. Sometimes accompanied by a degree of irritative inflammation, the cause of which will be found in the disordered state of the constitution.

In the *treatment of mortification of the toes*, it is wrong to apply stimulating applications. What I usually give as a medicine in these cases, is the camphor mixture, with a little volatile alkali; or, if you please, you may give a little cardiac confection. Mr. Pott has written about the advantage of opium in this affection. If you find that there should be much pain, then opium will give great relief, but unless there should be much pain, I do not see what advantage is to be gained from it; there are many instances of mortification having taken place without any pain being felt.

LECTURE XII.

OF ŒDEMA.

HAVING already spoken of the various kinds of inflammatory affections, and the usual products of them, I have yet another affection to mention, and I am rather puzzled what to think of it; it is called *Œdema.* It is said, in surgical books, to be owing to an increased secretion of the arteries, or to a deficient absorption. I am in the habit of saying that we have no need of a ghost from the grave to tell us that. It is, I think, what you may call a truism; it needs no great penetration to see that, in general, one or other of these things produces this affection.

Œdema is divided in books of nosology, into the *Œdema aquosum, Œdema simplex,* and *Œdema solidum;* there is also the *Œdema cum erythemate,* which is the aqueous or gelatinous œdema combined with a superficial erysipelatous inflammation, called *Erythema.* As to this œdema solidum, it is nothing but a gelatinous deposition like what you find in chronic inflammation. It is an increased arterious secretion, inducing a deposition of a gelatinous matter; Oh! there can be no doubt of it: a man's whole limb is enlarged in this way, and then it is a case of what they call *Elephantiasis,* or *Bucnemia.* If it be not produced in this way I do not know what else could do it. But in the œdema of a mere common character, I mean the aqueous œdema, which you frequently see, Mr. HUNTER says, that it more probably arises from too great a deposit of fluid, than from a defective absorption, and for the following reason —from seeing these swellings get well as the powers of life decline; for then the circulation is carried on with less force, and the absorbents take up what has been poured out. There is not the least doubt, for it is proved by many cases, that absorption goes on when secretion has ceased; absorption goes on not only a little time before death, but at, and even after death. There is a vital activity in the absorbent vessels and they continue their functions as long as life exists. Well, I have told you as much as I can do about this disease, and I

might go on to the treatment; but prior to this, I must tell you that I have even seen œdema the subject of metastasis. I remember one very remarkable case of it in this hospital. A woman had an œdematous swelling over one half of her face; it came on suddenly, but she had been out of health before. You might say that it was erysipelas, and not œdema; but the skin was quite pale, and of the common temperature. She took some medicine, but what I do not know. The œdema left her face and attacked her forearm; it swelled very much even as far as the elbow, and was attended with pain and pitting. The œdema, as I say, left her face; and attacked the arm; at the elbow there was a distinct circular line, a line of demarcation, as I may call it. The œdema had travelled on the arm to this place of demarcation, above which the skin had the natural appearance, and thinking it was not at all œdematous, Oh! a person would say, it could not be a distinct line; but I say it was a very distinct line, and that is all I can say of it. She lived many hours after this metastasis, and then she died. I was particularly anxious about the examination of this person, to try if we could discover the cause of death; but really we could find nothing wrong. She seemed to die from a difficulty of breathing, from an affection of the lungs; but I could see nothing wrong there, nor in her brain; and as to her digestive organs, to say the truth I could not see that they were unsound.

Treatment of Œdema.—Well, now as to the treatment of this disease. As it is probable that in general there is an increased deposition, we must try to check it, and to cause absorption. Upon my life, I know of nothing better than bandaging and gentle friction, and attention to the general health. Pressure promotes absorption, it supports the weak vessels. Well, so much for that.

The effusion may be the result of increased arterial action. When there is local disease, we denominate it from its most predominant character; and there are certain diseases which we call inflammatory, believing them to arise from an increased arterial action of the part; but the whole of the part is disordered at the same time. Then, in other cases, we denominate the disease from its most predominant character; we believe it to arise from an indolent action in the absorbent vessels of the part. There are chasms formed by the absorbents, in different parts of the body; the absorbents take away what the

arteries have deposited, and then the principal thing is increased absorption.

Absorption.—The definition which that thinking man JOHN HUNTER gave of it is very correct; he says, that there may be increased absorbent action so as to take away what has been deposited in the interstices of parts, so as to create a wasting of the parts. Interstitial deposits are removed by interstitial absorption, as happens in tumours, and with the fat, and so on. He divides absorption into *interstitial absorption*, and *progressive absorption*.

Now progressive absorption is really a term that would puzzle a person, just at first, to make out what is meant by it. It means the absorption of parts progressively or successively one after the other. It is this process of absorption which causes abscesses to approach toward the surface of the body; or aneurisms to absorb parts that are placed over them. A man has an aneurism in his chest, the ribs and other structures are taken away as it enlarges, nay, even the vertebral column itself is removed. This is what he means by progressive absorption, and all this happens without inflammation, without suppuration; it is simply the successive or progressive removal of parts. But this also gives progress to foreign bodies. A man has a pin, needle, or thorn, run into his hand, and it comes out at his shoulder. How does this happen? It is all against its own gravity;—why a path must be made for it, and the absorbents take away the parts; the muscular pressure and every thing of that kind urges it on; this is the cause of the travelling of foreign bodies through animal substances. A man swallowed a number of pins, and they came out at various parts of the body. A gentleman, who was a student, here sent me a case of that kind, in which by some accident (for I may say it was a mere accident) the woman knew how many pins she put into her mouth. I forget now exactly the number, but it was between forty and sixty; they all came out in about six months, from different parts of the body, shoulder, breast, belly, legs, arms, and back. During the whole of this time, she had most horrible nervous irritation. At last they were all discharged and she got very well. There has been a curious case published lately, where a young woman swallowed a number of pins, which worked their way out, but produced great irritation and pain; she had at last a pin-cushion full; but the most singular part of the story is,

that she did not know how they got into her. Now I say, however, that there is not always this nervous irritation when foreign bodies are travelling about, for people are not sometimes aware that they have these things in them.

I remember a man who came up to London with a sore leg; it proved very untractable, and he consulted me. I examined the sore, and I saw a little black speck in the centre of the sore; I touched it with a probe, and said, What is this? I took hold of it with a dressing forceps, and drew out a long black thorn; the wound healed directly. Asking the man about it, he said that he was coursing some months before, and in getting through a thicket or some kind of hedge, he ran a thorn into the back part of his leg; he joined in the hunt, and felt no more of it until it came to the surface, and then the inflammation was set up. These foreign bodies, I say, often travel quietly along until they come near the surface, and then inflammation is set up as if to get rid of them. This is the general course of procedure in such cases. Now this is a subject which must be understood, or you can never understand the things that you must meet with in your profession; this is the way in which foreign bodies travel, as bullets, broken pieces of bone, and so on; therefore, I say, it is of consequence to understand what Mr. Hunter calls progressive absorption, which is the taking away of animal matter progressively, so as to give exit to foreign bodies. It is really very curious; you may have a thing travelling in the course of the absorbents, without producing any disturbance of the sanguiferous or nervous systems.

A man consulted me who supposed that he had hernia, but he had none; there were three distinct lines or grooves made on the fore part of his belly by the absorption of the interstitial cellular structure. It was a very curious case, showing an absorption of parts without any inflammatory affection, without any nervous disturbance, and without pain. Then again, we have another kind of absorption. Mr. Hunter said, that parts failing in their vital functions, having diminished powers of life, were more likely to be absorbed than others; and in this way he accounted for another kind of absorption, which he has called

The Interstitial Absorption.—Of this I shall next speak. But he seemed to be very much puzzled how to express what he meant by it; he had very clear and precise notions respecting

it, but it is not always easy to make others understand a thing. If we look to the state of a part of the body when it wastes, we find that there is absorption going on, probably increased absorption. Mr. HUNTER said that this absorption was a process instituted by Nature, as it were to prevent the mortification of parts. It is a diseased action, or rather an action the consequence of disease, but having this beneficial effect, the prevention of a worse disease which is the death and perishing of the part. Now if you bind a pea upon a man's arm, what happens? It sinks into a nice cellular cavity of a corresponding form; how can you account for this sinking in of the pea, unless the parts on which it pressed were taken away by this process; the absorption begins on the surface, the life of the part seems threatened by the pressure, and it is taken away. Pressure promotes absorption, and when the vital energies of a part are weakened by pressure, they are taken away. In bedridden persons you may see this happening. You may be aware, as Mr. HUNTER says, of the impossibility of producing on all occasions that kind of proof which would convince a sceptic; but in this dearth of direct evidence, he seems to turn the attention particularly to a point brought forward by a man who had no theory to support; who was not a medical man; the chaplain of Lord Anson's voyage. He says, that during their voyage the sailors had experienced great fatigue; that they suffered great privations from having merely salt provisions, and the scurvy broke out amongst them. He says, that those men who formerly had ulcers had them break out again, and that those who had their bones broken had them severed again. Well, there was a state of ill health, and what Mr. HUNTER said happened, he said that newly formed parts have less life than old formed parts, and would sooner separate. You will be convinced that this is the truth, if you look at granulations; they have less vital energy, from being newly formed parts, and will more readily be absorbed.

As to this third process of absorption, namely the *ulceration*, it is a very compound disease. Parts may ulcerate with divers degrees of inflammation, or they may ulcerate without any inflammation at all. In general, the inflammation which attends ulceration is of an irritative kind, and such as would occasion the death of the part, if the absorbents did not remove it. I say, it seems to be taken away with a view of preventing the death of the part by the sloughing process. During this

action of the absorbents a chasm is produced, a discharge takes place from it, and that is all I know about it. This chasm is irregular, it has divers depths, it is not circular, it is irregular on its edges, as they say, with ragged edges; absorption has gone on more rapidly in one part than another, and the bowl of the ulcer is produced.

This ulcerative absorption being quieted, or being stopt, another process takes place, there is a filling up of the chasm by granulation; and then there is another process takes place, which is the formation of skin over the granulations. But during the whole process we call it an ulcer. I say, that if we were to watch this, the progress of this species of absorption, we should find it a very complex process, partly destructive, partly reparatory.

Well, now, if ulcers take place in the way I have called your attention you must soothe and quiet the ulcer, so as to lessen its excessive action, which leads to that state which would cause a removal of the parts. I have seen many cases in this hospital of that irritative inflammation attending ulceration which has been called erysipelas, which have been cured by soothing applications; such as simple dressings, bread and water poultices, and opium; the bread poultice, made either simply with water, or with poppy water, and by regulating the state of the general health. You go on with such measures until the disease is arrested, and then an attempt at reparation is made; granulations are produced. These granulations are small depositions of jelly, or of a gelatinous substance, into which vessels pullulate and are organized.

These granulations produce new secretions of jelly, which also become organized, and in this way the bowl of the ulcer is filled up.

Cicatrization.—After the bowl of the ulcer has been filled up by granulations, then comes the skinning process. The new skin you find commencing from the edges of the surrounding skin, from which it shoots like a pellicle over the surface of the granulations. The skin in general shoots from the surrounding skin, and if you place any thing on the edges of the sore, you prevent the vessels from shooting over the surface of the ulcer, and it cannot heal. The general rule, however, is not without an exception; for I have seen the skinning process take place in the middle of an ulcer, and extend to the surrounding skin. As to the nature of this growth which has

taken place, some question has arisen whether it deserves to be called true skin ; I say it does. It is not like the other skin, thick and hard ; but it has a cuticular covering, and therefore deserves to be called skin ; it is a tender and delicate skin, but it is a true skin. Has it a *rete mucosum?* No, says Mr. Hunter, I do not think it has, for the cicatrix of an African is not black. Yet he met with some instances afterwards in which this happened, so as to shew that this is a skinning process.

LECTURE XIII.

ULCER.

When the reparation of an ulcer is carried on in a healthy manner, the granulations which fill up the chasm are small, conical in figure, and florid in appearance. They are florid, because the circulation goes on well in the part. But when the process of reparation is not carried on in a healthy manner, the granulations are large, flabby, flat on their surface, and of a bad colour. When this process is carried on in a healthy manner, the secretion from the surface of the sore is a good pus ; and even, when ulcers are *sinuous*, as they say, you may judge of the state of the surfaces of the ulcer by the discharge which takes place ; this is the information obtained by common observation.

When the process of reparation is not carried on in a healthy manner, the discharge varies exceedingly. Sometimes there is a watery discharge called *ichor;* sometimes it is so acrimonious, that when it runs over the sound skin, it will excoriate it. It is an acrid ichor; sometimes it is so acrid, that it is necessary to butter as it were the edges of the sore, to wash the parts round very frequently, and clean them thoroughly. It was one of these propositions of Mr. Hunter, whether the secretion acted on the surface of the sores, so as to keep up the irritation in them; but he thought from observation that it did not, and for many reasons certainly, which I do not now want to relate, because I should be leading you away from the point that I wish more particularly to dwell upon. Another

description of discharge is, that which has been called *sanies*, which means a viscid discharge ; sometimes it is rather bloody and then it is called a *bloody sanies*. Sometimes a thick discharge adheres to the edges of a wound, of a dark colour, which has been called *sordes*. Well, all these circumstances are observable in the incarnazation of a sore, and afford an evidence of the unhealthy state of the part. We have to correct these unhealthy dispositions in the healing of sores, and the first step is to stop the progress of the disease.

I think that Sir EVERARD HOME'S book is the most scientific which has been published on ulcers, and I have been in the habit of making some comments on it. He has divided ulcers into such as are *weak* or *indolent*, and *irritable* sores, to which he has added such ulcers as manifest some *morbid peculiarity*. He describes the kinds of sores very well which he speaks of.

As to the *weak sores*, he says that they are pale and flabby, that the granulations have not a healthy appearance, and that they are slow in healing. He says, that if you dress such a sore with bark, it will improve in its appearance, and that if there be two such sores, one near the knee and the other on the ankle, and you give the bark internally, the sore on the knee will heal first.

As to the *irritable* sores, I can make out nothing different in the description from the circumstances which generally attend the commencement of an ulcer. The bottom of the sore is rough, the edges are ragged and irritable, and the granulations are very few and indolent. What you have to do, is to oppose the disease which has produced the ulceration, by quieting the irritable state of the parts. I know nothing so good for this purpose as a weak solution of opium in water. You may rub down two scruples of opium in eight ounces of warm water, and strain the solution through a little tow, or you may dissolve in some cases half a drachm ; some pieces of lint should be dipt in the solution and laid upon the sore. It is something which quiets irritation, and I should tell you also, that it should he applied luke-warm ; all applications to sores, as dressings, should be of the same temperature as the sore. Cold water is not a pretty thing to apply to a sore, especially an irritable sore ; I say it is not a pretty thing, for we have no right to chill parts, as I said when speaking of the regulation of temperature. When you apply a solution of opium to sores, you should touch the edges of the sores with spermaceti

salve to prevent the lint sticking, and over it put a bread and water poultice made with poppy water, or you might put over it linen folded, dipt in the poppy water, and over the whole you should apply a light bandage to steady the dressings.

Keeping the patient with a sore leg in bed is a thing of great consequence. In the horizontal position the blood returns from the part with greater ease, but when the patient is erect, it has to mount up against its own gravity. Sir Everard Home has shown in the description which he has given of weak ulcers, that the granulations were red whilst the patient lay in bed, but when he stood up, the granulations became immediately purple. This seems to show the weakness of the newly formed vessels, and the advantage to be derived from an attention to posture. I say that your object should be to quiet irritation.

After you have quieted the irritation, granulations will grow, but these granulations will not be healthy; ulcers do not form in healthy people.

INDOLENT ULCER.

The granulations grow after the irritation has been quieted, but they are of an indolent kind, and now constitute the common ulcer which we see so often in the hospital. The granulations are large and flabby, the surface of the granulations is not of a good colour, the discharge is not good, and the edges of the sore become thickened. Weakness, it is said, is the essential character of the ulcer, but it is only the substitution of one name for another; when weakness is provoked it becomes irritable, and when not provoked, it degenerates into indolence. This is what nobody can deny, I think; it is what is called a truism. In the hospital you will have frequent opportunities of observing these sores. The great art in the management of all ulcers is to quiet them when they are irritable, and to stimulate them gently when they are indolent. Then as to the substances employed for the stimulation of indolent sores, they are very numerous; there are various ointments made with the different metallic oxides; there is the red precipitate, the white precipitate, the different preparations of zinc and lead. I knew a gentleman who once told me, when speaking of the various stimulating dressings which had been applied to sores, that he never attended to all the farrago

which has been laid down in books, but that he was in the habit of curing all the sore legs which fell under his notice by using only one thing, which was a solution of the corrosive sublimate in water. He said that he proportioned the strength of the wash to the state of the sore; if the sore were very weak and indolent, he applied it strong, if not very indolent, he used the wash weaker. The fact is, that they are sores which you may dress with a great variety of stimulants; you may sometimes dress them with a thing which will agree very well for a time, it will then lose its effect and not agree, and you are obliged to apply another, and new things appear to please them.

Plaster of Paris was introduced as a dressing for sores by Mr. Forsyth, and for some time it succeeded very well; it worked wonders of course. It was a new sort of excitant to the sore, and the sore did very well with it for a time. But mistakes were sometimes made; this plastering was sometimes made to irritable instead of indolent sores. I know this, that you may succeed in exciting a good condition of the sore, and it may become again irritable from some constitutional or other cause, and the granulations will all become absorbed, the sore becomes indolent again. The essential character of the sore is the same; there is weakness, and if not provoked it degenerates into indolence. I have said enough of the character of these ulcers, and I shall now proceed to the treatment.

Treatment of Ulcers.—First, of the treatment of *common ulcers*, ulcers in which there is no morbid propensities. I have told you as much as I know as to the principle on which all ulcers should be treated. The irritable sore must be soothed, and the indolent sore excited.

There is nothing that can be applied to a sore when in a quiet state so good as Baynton's bandage. First, you appease the part, then you apply a slight stimulus, such as the sore may tolerate without resentment, without exciting any inflammatory action; and this is what I call the surgery of *susceptible surfaces*. You diminish sensibility by exciting the part to a certain degree, short of that which produces re-action. Now I never can explain my meaning half so well as by referring you to a fact of which you must be peculiarly conscious, I refer to an inflamed eye; what is necessary to be attended to in the case of an inflamed eye is necessary to be observed in the

manner of applying BAYNTON's bandage, if you expect it to be attended with any success.

Well, what do you do when a man's eye is inflamed? You tell him to apply leeches, to use cold washes, or the solution of opium, to take aperient medicine, and so on; you do every thing, in fact, to quiet and tranquillize the part, to allay the inflammatory action, and when you have succeeded in doing this, you adopt a different plan: you tell him to use an eye-water, and you give him a weak solution of the sulphate of zinc, say one grain to the ounce, and you tell him to use it frequently, to use it every six hours, to apply it by the means of the eye-cup, or by putting a little of it into the eye. The first time he applies it, he puts his finger to the eye and rubs it, for he finds that it smarts uncommonly; he probably turns about, stamps on the floor, raves from the violence of the pain, and is almost disposed to damn the doctor who puts him to this, as he imagines, unnecessary pain. The pain, however, soon goes away, and he is induced, from his eye still continuing weak, to use it again, he does so, and finds that it smarts much less than it did the first time; he tries it again, and finds that it smarts still less. He says to himself, hang it, did not the doctor tell me to use it every six hours? I will try it again; and so he goes on till he finds it makes no more impression on his eye than water by itself.

The strength of the collyrium is increased, he uses it without producing any serious inconvenience, and the eye is cured. Now, I ask you, if this does not show a morbid susceptibility of the surface to which the stuff is applied, and this morbid susceptibility is removed by the application of a stimulus not sufficient to produce re-action or any sort of inflammatory process.

There is a great necessity for applying stimulants to sores, but the time for using them must be regulated by the condition of the sore. If you apply a stimulant after you have excited the irritability of a sore, granulations are formed, which continue to grow; but if you increase your stimulants the granulations will slough. But while the sore is indolent it will bear the application of a stimulant. Now, as to the manner in which BAYNTON's sticking plaster bandage should be applied, and the good effects which it is capable of producing, they are both points worthy of attention; it is ridiculous to suppose that there was any peculiar benefit to be derived

from its being applied by any particular individual, for what can be done by one can also be done by another.

It is a bandage made of straps of adhesive plaster put carefully round the limb, having the sore first protected by a little lint dipped in a solution of opium, or by a bit of lint having a little spermaceti salve spread on it. The straps of plaster are put round the lower part of the limb first, and then made gradually to come upwards; over the whole you may pass a light roller. Great attention is necessary as to the manner in which it should be applied, and I wish to call the attention of the dressers of this Hospital particularly to this subject, for unless it is done well, it cannot produce those marvellous effects of which it is capable. I recollect the case of a clergyman which very well illustrates its utility. He called on me and said, that he entertained a great respect for me, and wished very much for my opinion respecting his leg. He showed me his leg, and there were two sores on it, which he said had been very intractable, and that he had for many months felt great inconvenience and pain from their continuance. I said, oh! if they are dressed properly, they will be well in a fortnight. A fortnight! surely Sir you are in jest, I have been under the management of some of the first surgeons in London for many months, and now you say I may be well in a fortnight. He appeared very much hurt at what I had said to him. Nay, Sir, said I, you need not be wroth at what I told you; I saw that you were exceedingly anxious, that your mind was in an irritable state, and I said it to pacify and tranquillize you, and I again repeat, that if your leg be properly dressed it will be well in a fortnight. He said that if that were the case, although he had very important business to take him out of town, he would remain for that time at least. I ordered him merely a dose or two of blue pill, and a little aperient medicine, applied Baynton's bandage, advised him to keep at home and raise the leg in a horizontal position. And, before the fortnight was over, aye, even before five days were well over, his leg was quite well.

I remember also the case of a young lady, who had a bad ankle, it was near Christmas time, and she wished very much to join in a dance with her other sisters, who were to go to a ball in a week. I said that a week was a short time to heal a sore in, but, however, we should try what could be done. I put on the bandage I have just been speaking of, and she went

to the ball at the end of the time I have mentioned, and danced without any inconvenience.

It appears to me that Mr. BAYNTON did not, at first, exactly understand the principles on which this bandage of his was to act; he imagined that the benefit was obtained by its approximating the edges of the sores; but I think it acts differently; it acts by giving a moderate and equable support to the newly formed vessels of the sores, and supplies the deficiency of the proper natural coverings. I remember a man in this hospital who had a large ulcer on his leg; he could not even put his foot out of bed without producing most excruciating pain; he had the bandage applied, and was desired to stand up by the side of his bed, he did so without making any complaint. The vessels of the leg had received the support which their weakened state required, and, therefore, were not over distended as before.

Then again, if it should be necessary to lessen the temperature of the limb, you can do this very easily by wetting the bandage with water, and allowing it to evaporate. Mr. BAYNTON was aware of this, and he recommended the patients, when the heat of the limb became high, to sprinkle water over the bandages, and to do this there is nothing better than a watering pot. The bandage retains as much of the fluid as is necessary for the process of evaporation to take place. It is a very useful, and, generally, a successful method of treating sore legs.

But to return to the various methods which have been proposed, and the various stimulants which have been recommended for the treatment of ulcers. It is impossible to describe all the appearances which present themselves in the progress of ulcers, or to detail to you the stimulants that are applicable to particular cases; the practice in these respects is rather empirical, for we are guided principally by experience. Descriptions cannot convey to you correct ideas on these points; you must see them to understand them.

I said there were various methods of stimulation practised, and sometimes with very good effect, but I am satisfied that I have seen ulcers get well simply by attending to the constitutional treatment. A man in the hospital had a large sore on his leg for a very long time; it would improve under a new stimulant for a short time, and then require another. I said one day, when going round the house, I wish you would dress

this man's leg with a little simple ointment, merely attend to the state of his digestive organs, and give him a little sarsaparilla. They did so, and he got speedily well.

I have said that if a man would give me a Pharmacopœia, I would undertake to say that there should not be one thing contained in it which, sooner or later, I have not seen applied as dressings to sores. All the substances which it contains may be ranked under particular classes, and their application to ulcers may at one time or another be useful.

There are substances which may be regarded as stimulants, the different preparations of the metals, of mercury, zinc, copper, and so on. The mineral acids may be ranked under this head, and they certainly, when diluted, are very useful applications, as the nitric or muriatic acids. I once took it into my head to try the effect of bathing the limb in an acid, where there was a diseased bone. I put some nitric acid into a tub of water, and told the man to soak his leg for a quarter of an hour in it, for the purpose of dissolving the earthy matter of the bone, but I found it produced a great deal of inflammation, and I was obliged to give it up. Then again, there are substances which may be called tonics; these, when applied to sores, often produce very good effects; such as the astringent vegetable barks, and so on; other substances, which may be called cordials, as the different balsams: the gum Benjamin, the solution of it, called Turlington's Drops, and to some indolent sores it is a very good application. Other things act as sedatives or narcotics, and you know there is a large variety of substances that act in this way. There are opium, hemlock, and belladonna, and so on. As to the latter substance, I once had a case in this hospital where the sore had been very irritable and obstinate for a long time, and we had tried almost every application without any benefit, when I recommended him to use an ointment, in which some belladonna had been rubbed down. He did so, and passed a very quiet night; he was quite delighted when I saw him in the morning. Well, I thought this was a very good application, but just as I was going away from his bed, he said, do you know, Sir, that I had a very odd thing happened to me in the morning. I was dizzy, and could not see any thing for an hour. Oh! were you? said I; but, egad, I did not think then it was a very good application, and I ordered him to leave it off immediately.

Having now described those ulcers, which may be called

simple, from their not having any morbid peculiarity, such as the indolent ulcers, which Sir EVERARD HOME has described, and which he says bark will cure; such as the irritable ulcer, for which he recommends conium; and the ulcer also with hard edges, and of a sluggish character, for which he recommends mercurial ointment: there are other ulcers, he says, which a solution of common sea-salt will cure; these I take it for granted are the scrofulous ulcers. I shall next speak of *ulcers* which are characterized by having some *morbid peculiarity*.

The cancerous ulcer is of this description: it is an ulceration attended with violent pain, the discharge from which is horribly fetid, and it is a very intractable disease. Different applications have been made to such ulcers with a view of correcting the nature of the discharge, and allaying the irritability of the sore. The oxides of the various metals, carbonic acid gas evolved from fermenting poultices; weak solutions of the oxymuriatic acid, chlorine, as it has been lately called, but I do not know what they call it now, they change the names of things so often.

The discharges from such sores have been made the subject of analysis by Dr. CRAWFORD: he found that this ichorous irritating fluid contained sulphurated hydrogen gas; it is owing to its containing this, in combination with other things, that the common adhesive plaster, containing lead in its composition, becomes blackened. It is sufficiently powerful, very frequently, even to blacken silver probes that you may use in dressing. I know of nothing better for the correction of such discharges than fresh-burnt charcoal. There was a druggist, a scientific sort of man, and he had a jar of water, in which some leeches had been kept, and in which many of them died, and the water became very putrid. He wished to ascertain what could be accomplished by the charcoal, in changing the appearance and quality of the water. He burnt some wood, powdered it almost directly, and put it into the water; in a short time it became clear, and was deprived of its putrescent matter. It appears to me, that it is the best application that can be used to such sores, joined to the hemlock poultice. You must quiet the irritation of the system as much as you can, and give anodynes, and so on, and attend to the state of the digestive organs.—In my next lecture I shall speak of disorders successively affecting the nervous system.

LECTURE XIV.

VARICOSE ULCERS.

Sir Everard Home, speaking of ulcers depending on a varicose state of the veins, recommends that the veins should be tied, but it is a practice which appears to me extremely dangerous, being frequently followed by inflammation of the part, and great constitutional irritation. Mr. Brodie, finding that ligatures on the veins produced these bad effects, was induced to try what their division would do. For this purpose he makes use of a curved knife with a cutting convex edge, which is first introduced horizontally under the skin and vein; the blade is then turned upwards, and the vein divided as the knife is withdrawn. This is a part of new surgery, however, into which I shall not now enter; you must read Mr. Brodie's papers for yourselves, for I cannot give you any information on the subject derived from my own personal knowledge. Mr. Brodie states, that he found the division of the vein below the sore more successful than the division above; now this does not at all accord with Sir Everard Home's notions. In short, it is a subject I do not understand myself; as far as I know, no harm results from the practice ; as to the good done by it, you must decide for yourselves, I am not competent to give you any information on it; and any observations which I might now offer on the subject would be derived from the opinions of others, and not from my own personal experience.

I have been talking of local diseases, as they may occur in any part of the body, in consequence of an undue action in the arterial system of the part, or as a consequence of increased morbid action of the absorbents of the part, and have now to speak of something wrong in the nervous system of the part.

NERVOUS AFFECTIONS.

When any disease, however, comes on in a part, I do not believe any one system in particular is affected, but that every system of the part suffers ; the vascular, absorbent, and nervous systems are equally wrong, and yet we name the disease

from its most prominent character; as, for instance, in local diseases, when the vascular system is affected, you may have inflammation, ulceration, and something wrong in the nervous actions of the part, and these derive their name from the characteristic circumstance of the disease. When the nerves are affected locally, it most frequently depends on general disturbance of the whole nervous system. You may have numbness in a part and local pain; but, in general, these, together with weakness and twitching of the muscles, (for here I must connect the muscular and nervous systems together,) depend on something wrong in the nervous system. In short, I believe every disease is originally nervous. Disorder of the nerves is first excited, and this leads to increased action of the vessels, and increased action of the absorbents of the part. Some diseases affect the vitality of the part. There will be acute pain and increased vascular action in the part, such as in inflammation, gout, and rheumatism, and these will move all of a sudden from one part to another. I say, therefore, that these diseases are originally nervous. Egad! the pain will be one instant in one part of the body, and in the next in another; and does not this show that an affection of the vitality of a part, capable of sudden shifting and changes, is an affection of the nervous system. I say that there is a certain *disorder* of the nerves of the part antecedent to *disease* in the part. Suppose that there is disturbance in any part, you cannot say that it is disease; it begins with irritation, and therefore I object to the word disease in that sense. Functional disorder precedes any change in the structure of the part, but I believe it to be originally nervous. I say to persons who come to me with weakness of the muscles, spasm, and irritation, occurring in any part of the body, that it is the result of something wrong in the nervous system as a whole. I could quote plenty of cases illustrating this, showing that local irritation and disease are the effect of something wrong in the nervous system. This is very well exemplified in the complaints which attend dentition in children, where the nervous irritation sometimes disorders all the parts of the body; there are convulsions, weakness of the muscles, the arms are turned out, and sometimes inwards, and so on. The larynx also becomes affected, and ah! persons lose their voices all of a sudden, all through an affection of the nerves of the part.

Then again the nerves of a part may be influenced by some-

thing operating on the trunks of the nerves leading to those parts; as in injuries or affections of the spinal column, all the nerves in the parts supplied from below the part of the medulla spinalis injured, will be deprived of their sensibility. Again, affections of the nerves of the cerebrum may produce local affections in every part of the body. We cannot conceive disease to be merely local; there must always be a plurality of parts affected. The subject is one difficult to explain; but what I want chiefly to call your attention to is, a painful affection of the nerves of a part called

TIC DOULOUREUX.

It was a rare thing, in the early part of my life, to see many cases of disease produced by increased vascular action, which are now common; and so it is with diseases depending on an undue nervous action in a part; for I do not think there was such a thing as *tic douloureux* ever dreamt of in my time. How this comes, I dont know. There has been a great increase of medical men, it is true, of late years, but, upon my life, diseases have increased in proportion; that is a great comfort. As to the disease, it is attended with the most intense pain; there is no increased vascular action in the lip or cheek, for they look as usual; there is no increased vascular action generally, nor any error in the absorbent system; but, as to the pain, it is most excruciating; ah! often beyond endurance. I have seen people with this disease perfectly free from pain one instant and who, whilst talking or doing something else, have been suddenly seized with the most racking and excruciating pain imaginable. I remember one young woman in particular, who, when the paroxysms of pain came on, would tear handfuls of hair from her head, and required three strapping young men to hold her; so tormenting is the pain, so formidable are the effects which it produces! Well, it is a very curious disease; and some persons have been led to divide the nerves of the part, but, I say, you might as well expect to cure a man with gout of the pain in his great toe, by dividing the nerves going to the toe, as to cure a person of tic douloureux by the division of a nerve. In both cases the complaint is perfectly constitutional. You may see a person with gout who is almost unable to move with the pain; but produce a shock on the nervous system, by telling him that the house is on fire, and he will

scamper about like a lamplighter. As SMOLLETT tells us, in one of his novels, of Captain LISMAHAGO, who went into a house and cried out to an old gentleman with the gout, "Mad dog, mad dog!" when he jumped up, and ran out of the house even into a pond of water opposite.

In this complaint, one part alone is not affected; the disease flies from one part to another, and the division of the nerves does no good. Can it be cured? Undoubtedly, if you can tranquillize the disorder of the nervous system. But how is this to be done? Why, I don't know, nor do I believe any body else knows. As I say to patients who call upon me with these kinds of affections, if I could quiet irritable nerves, strengthen weak ones, and regulate the distribution of the nervous energy, then I could cure all the diseases you would bring to me. This may be wrong, or let it be otherwise, I know of no other medium through which these affections can be cured, than that of the nervous system; and, I believe, that if the patient be put on a rational plan of diet, and his digestive organs got into a right state, these affections will not come twice, at least if the patient remain in good health. I have seen many cases of this kind, and feel strongly convinced as to the truth of what I say. I once lodged in the country during the summer, where I used to see a man go by the window every day, making all the wry faces in the world, one day having the mouth and chin drawn up, another, grinning like a "Cheshire cat." I wondered what all this meant. At last he called on me one day, because I suppose I was a London surgeon, to consult me. He was a respectable farmer, and a well educated man. He asked me whether it was possible for him to be cured? Most undoubtedly, I told him. Regulate your diet and bowels; take a less quantity of wine than you have been accustomed to (for he was in comfortable circumstances); in short, keep your system as quiescent as possible. By attention to these circumstances he became quite well, as you will see by the following anecdote, which he afterwards told me:—"I returned," he said, "to the country, (for he was a man of fortune, and farmed his own estate,) and met a surgeon in the market, who had formerly attended me. I said to him, 'Doctor, I will buy sixpennyworth of pears, and lay you a bet that I eat my half of them before you will yours.'" Whether he gained the bet or not I don't know, but the surgeon was astonished, and said to him, "I thought that you could not

venture to eat pears." To which he replied, "Six weeks ago, I could sooner have done as MUCIUS SCEVOLA did, thrust my right hand into the fire, than have taken a cold pear into my mouth." Now, I have seen several cases of this kind.

The most severe case which I ever saw, was in a physician of this town, who suffered for many years the most excruciating agonies, and at last died. The nerve was so cut about, that he said himself, when he shaved, or was shaved, the skin was quite insensible. The pain in these cases is mostly produced in the trunk of the nerve, and not in the branches. I do not myself think division of these of any use, but rely on the constitutional treatment of the disease.

I once divided a nerve in a lady's finger for tic douloureux. The pain was so great that she could not put her hand into her pocket; she had cramps of the muscles and spasms, and, in fact, I was afraid that she would have lock-jaw. The complaint continued for many years, gradually augmenting in severity, and I thought the case would justify the division of the nerve. The temperature of the arm was above the natural standard, the pain extended up the whole course of the radial nerve, but it began from the extreme point. I cut down on the nerve, exposed it, divided it, and took away three quarters of an inch of the nerve on the second joint of the finger. It was a long time before she was physiologist enough to find out that the division of the nerve had done her any good, for she had all the pain for some time after, as before the operation. The arm above the part where the nerve was divided was particularly sensible, and the pain was referred to the end of the finger. She became, however, a little better, the arm regained its natural temperature; the pain subsided, and she lost all those general nervous feelings to which she had for so long a time been subject. But this unfortunate girl died a year afterwards of extensive disease in the abdominal viscera. Now I say, that I have seen several cases of this kind, and I am convinced that the treatment of tic douloureux should be constitutional treatment, and such as is calculated to allay the irritability of the nervous system. It seems to me as truly a constitutional disorder, as either gout or rheumatism. In the physician's body, whose case I mentioned to you just now, no organic disease could be found in any part. In many cases which have been examined, no organic change could be found; the complaint consisted in functional derangement, and not in any structural change.

Well, I proceed, for you see my arrangement is, to speak first of general diseases, which are named from their most prominent character, and then of local diseases, such as affect the vascular, absorbent, or nervous systems of parts. I shall now speak of those diseases which occur generally throughout the body, such diseases as are found to affect the various structures, as the bones, joints, &c. Here I cannot enlarge on the subject, for my object, in these lectures, is to supply that part of surgery which is not included in the anatomical course. It matters not what I begin with, I may speak first of

THE DISEASES OF THE SKIN.

And here I may not go into the subject scientifically or satisfactorily to yourselves or the public, but I will tell you what I know about it. The new names which are used to denominate the affections of the skin, bother me; and I am in the habit of saying of them, as HUDIBRAS says of rhetoricians:

> "For all the rhetoricians' rules
> Teach nothing but to name the tools."

But they do not tell you how to cure the disease. In diseases of the skin, many are of the papular kind, others pustular, and some vesicular. The eruptions, which are red and slightly protuberant, are called papular eruptions, and are made an order in the class of diseases of the skin. Some end in a kind of suppurative inflammation, and are called pustular eruptions. Others end in an effusion under the cuticle, and are named vesicular. Some are attended by none of these effects, but produce an alteration in the whole covering of the skin, and are called leprosy. It is all right to have these names, for the sake of distinction, but persons go farther, and subdivide them into genera and species, give them break-tooth and puzzling names, in consequence of every insignificant appearance, which do not at all signify the nature of the disease, and are therefore unnecessary. They may lead to nice distinctions of the disease, but, if carried to no ulterior object, are of no use whatever.

There are causes which act on the exterior of the body, as cold and wet, which weaken the circulation of the skin, that may lead to cutaneous disease; and many medical men, in order to keep up a certain degree of energy in the action of the skin, employ tonics in cutaneous diseases. The decoction of the

elm bark is one very much used. But I think the great cause of all variations in the states of the skin is to be met with in the digestive organs. CUVIER, speaking of the alimentary canal, says, that it is an internal sac of the skin, that the skin is inflected and lines the intestinal canal. You may have perceived that diseases of the skin arise in general from irritation of the alimentary canal, making allowance that weakness and defective circulation may sometimes be the cause of them. The main cause, however, is to be found in the state of the digestive organs. I beg you to remark the strong sympathy which exists between the skin and the alimentary canal. Watch the state of the skin at the time you give an emetic; a few minutes after a person has taken it, the skin becomes cold and shrivelled, like what is called goose-skin; but as soon as he begins to throw up, the skin is covered with cold clammy, and afterwards with hot, burning sweats; a simultaneous change in the state of the skin is taking place with that of the stomach. A person shall eat heartily of mussels, and in ten minutes afterwards the whole skin shall be covered with a rash. What other inference can be drawn from these facts but that there exists the greatest sympathy between the skin and stomach. You can see no other general causes which produce cutaneous disease but disorders of the digestive organs. It might be granted, perhaps, that there existed a sympathy between the skin and the lungs. I thought at one time evidence of this existed—it may be granted, however; but the great means of curing cutaneous diseases is by tranquillizing the state of the alimentary canal. I have continually observed in patients with these affections, that as the digestive organs were set to rights, so the cutaneous diseases were cured. In some instances where the people have had scabs all over the body, by attending to the state of the alimentary canal, the scabs have entirely disappeared, and firm sleek skin has been produced in the spots which were covered by them. But I grant that a deficiency in the circulation of the skin, produced by exposure to wet, cold, and the changes of temperature, may be causes of cutaneous disease. Many cases of this kind have been admitted into the hospital, with various forms of cutaneous disease, and have been all cured, by keeping them in bed, the warm bath, cleanliness, regulating their diet, and paying attention to the state of the digestive organs. There are other cutaneous diseases which will be benefited by external applications and the use of

the diluted sulphuric acid, but the main object is to set the digestive organs to rights.'

I do not want to speak of cutaneous disease generally, but only of the surgery of cutaneous diseases, of the surgical applications which are necessary in these affections. When the skin is red, irritable, and covered with scabs, what are you to do? Why you will be obliged to allow of a free discharge from the surface of the skin, or else you will never do any good. For this purpose you apply poultices with light dressings to the part, and these soften and remove the scabs. The scabs form a source of irritation to the parts, and the discharge is confined beneath the scabs, and produces irritation. Poultices soften the scabs, and cleanse the sore of an irritating substance when they often get well. Cleansing the surface then, I hold to be good, for if an acrimonious discharge is confined to the surface on which it is poured out, egad! what can you expect but that a sore will form beneath it. To *cleanse, to dry, to anoint,* then, are three points to be attended to in those diseases of the skin which do not require dressings. Where dressings are employed, the great secret is to cleanse them of the acrimonious matter, and to dry them well before the ointment is applied. I find it necessary to explain these points well to the patient, on the principle of common sense. You see dressings sometimes put on to wet sores; why you might as well apply water upon a greasy surface, as ointment to a wet surface; therefore the parts must first be well dried. To cleanse, to dry, to anoint, are three points to be performed in the treatment of cutaneous diseases. In an irritable state of the skin, the principle on which I think anointing good, is, that as there are on the surface of the skin, myriads of follicles, or oil-bags, which form the natural ointment to the skin, so it is deprived of this ointment at the time when it is most wanted.

The best medicated pomatum, for so I call it, is hog's lard, rubbed down with the liquor plumbi subacetatis, this is the name of it I believe; I mean Goulard's extract, in the proportion of one drachm of the latter to an ounce of lard. There are many diseases of the skin in which the unguentum hydrargyri nitratis is of use; it should not be applied too sharp; two-thirds or three-fourths of lard, to one-third or one-fourth of the ointment. I have seen many cutaneous diseases cured by the unguentum hydrargyri nitratis. Tar ointment and tar

water are also good applications. This leads me to speak of washes, which produce certain good effects in cutaneous irritation: a solution of sulphate of zinc and of Goulard's extract are good washes. Also water impregnated with sulphuretted hydrogen, by a solution of sulphuret of potass, formerly called hepar sulphuris, and the tar-water, frequently do great good.

AFFECTIONS OF THE SKIN PRODUCING ULCERS.

Now, there are many ulcers occasioned by disease of the skin. Tubercles form first, and these produce ulcers; the ulcers originating in the cutaneous disease. A morbid action in a part is established, it spreads, and becomes an ulcer. There is one form of cutaneous disease, producing ulcer, called the

Porrigo Lupina.—This begins by a chain of tubercles, which are in proximity to one another. The feel of the skin is like a cord extending from one point to another, and it seems as if the disease was in the absorbent vessels. These spread and continue till they form a foul sore. In the early part of my life, I was taught that this disease was sure to be cured by mercury, which altered the state of the action of the part. I have seen it used to salivation and not cure; yet a certain quantity is useful in this disease, to regulate the secretions, and alter the state of the digestive organs; with this view, its exhibition will be found of use.

Under this head of ulcers, may be classed the

Herpes Nasi, and Herpes exedens Nasi.—The Herpes Nasi is the ulcer which begins in the nose, and communicates itself to the surrounding parts. The first sore is getting well, when another, close to the old one, breaks out. Thus the disease, as John Hunter says, wears itself out in one situation, but the surrounding parts become affected by it. Thus the healing of the sore in one part shows that the disease is curable; and I am persuaded that it is kept up by disorder of the digestive organs. I remember a striking case of this: A woman, whose appearance was so much disfigured that she could not go into service, was admitted as a patient into the hospital. Being extremely disfigured, and unable to get a place in service, I said to the students, you see such and such a case, (alluding to the ulcers of this girl,) for God's sake, let us see what will happen by paying attention to the state of the digestive organs,

and not to dressings. Well, by regulating her diet, and the state of her digestive organs, the disease was cured; and on going round, the students were perfectly astonished; for a sore that before was irritable and spreading had now got quite well. Such was the success of this simple treatment.

Now there is another disease which disfigures the countenance most horribly, and which now and then comes under our notice; it is what is called *Noli me tangere,* or Herpes exedens nasi. The sore goes on to the destruction of the parts until the patient dies. I know no disease of a more terrific nature than this. So much for the treatment of cutaneous diseases in general; but I believe that they are in all cases kept up by a deranged state of the digestive organs.

I shall now speak of some diseases in particular, and first of the

Itch.—And what is the itch? It is a vesicular eruption which, some say, contains a small insect that produces the irritation. I do not want, however, to discuss this subject. A pimple appears on the surface of the body, which bursts, and the discharge from it is applied to a new surface on which another pimple appears, and so on. Then it begins at a point, spreads all over the body, and sometimes forms scabs, in which case it is exceedingly difficult to know whether it is itch or not. Well, brimstone is an infallible cure for the itch, and in dubious cases I often say to the patients, "Try it;" if it is the itch, it is an expeditious mode, and will cure it in twenty-four hours; and if it is not itch, it will not be made worse. In the itch you order an ointment to be used, composed of equal parts of sulphur and lard rubbed down together, coloured, if you like, with a little cinnabar, and scented with a few drops of oil of rosemary. This is the itch ointment, and you order a good quantity of it to be rubbed in, full half a pound at a time; and what I say is, "Now, Sir, all you have to do is to anoint your whole person by the fire-side, at bed time, with this ointment, particularly the parts affected; then put on a suit of under garments, go to bed, and remain there till the same hour the next night; then take a warm bath, clean yourself well, when all the itch will be gone, but you must be very careful to destroy all the clothes which you have before worn. But there are many diseases like itch, but which are not itch, dependent on disorder of the digestive organs. A gentleman came to me and said, that he wanted to have my opinion as to a disease which

he had been told was itch. If it is itch, he said, it has been very expensive, for it has already cost me 600*l.* Egad! I said, you have paid your medical men very liberally. Six hundred pounds! At last it came out that he had destroyed his whole wardrobe three times over. On examination, I found that it was not itch. I gave him some medicines to set his digestive organs to rights, and he was soon cured. I have seen many similar instances.

There is another infectious disease, called

Tinea.—This is a papular eruption which infects the heads of children, inoculating the surrounding parts, and forming a zone of papular eruption, called the *Ring-worm*. I have now to describe the various species of *Porrigo*.

I have heard of so many nostrums for this complaint that I am quite weary of them. When the disease gets to the surrounding parts you shave the head, wash the sores very clean, and having well dried them you apply some simple ointment. The ointment which I order consists of lard and white precipitate, and is called, I believe, Unguentum Hydrargyri Album; it is made of one drachm of the precipitate to an ounce of lard. I feel confident that the stubbles of hair keep up the irritation in those cases, and therefore great attention should be paid to the keeping the head quite bare and free from all hair. Now where the disease has been of long continuance, and is kept up by a disordered state of the alimentary canal, keep away all dressings, and try to put the digestive organs into a sound state. In these cases scabs form on the head, and by the discharge accumulating on the head a large mass of filth would be formed, which would puzzle a man to say what was the original nature of the disease. I remember a case of this kind, where a large conical scab had formed on the head of a person, and some physicians said it belonged to this order, and others to that, but they all agreed it was a very curious case, and said we will have an artist to draw it. But I said to the dresser, simply lay a bread and water poultice on the part, and attend to the state of the digestive organs; and, egad! before the artist came the scabs were gone, and the patient was cured. I mention this to shew you how sores of years standing may be cured by attention to the state of the alimentary canal. You are to judge whether what I mention to you in these lectures is correct, by the result of your own observations. I only tell you what is the result of my experience.

LECTURE XV.

I SHALL now speak of the diseases which attack those parts possessing the least vital energy, and therefore most susceptible of their influence; these are the bones, joints, tendons, or sinews, the absorbent and glandular systems. Now, in point of susceptibilty, I know no part so highly susceptible as the absorbent vessels. The increased susceptibility and irritation of these vessels extend sometimes to such a degree as the profession knows little about. With respect to the glands, they are also in a high state of irritability, and sometimes from the effect of a specific poison. But as to the absorbents, they become susceptible of diseased action from the slightest causes, whenever the health is disordered, and to such an extent as the profession, I again repeat, has no adequate conception.

IRRITATION OF THE ABSORBENTS.

A prick from a clean needle will produce considerable irritation in the course of the absorbents, and extensive disease very frequently comes on, which is attended with the most terrific effects. Of this I have seen many instances. I will mention one. There was a young lady, the daughter-in-law of a medical man, and who lived in the house with him, who pricked her finger with a sewing needle. Probably a few days elapsed, irritation of the absorbents came on, which extended to the axilla, and so excessively disturbed, nervous, and agitated was she, that they thought she would die, and they sent for me. I went, and on seeing the patient said, apply a bread and water poultice to the absorbents, and set the bowels to rights. The tongue was furred, and the bowels were in a deranged state. I ordered her calomel and jalap, and some saline medicines, and in a short time all became quiet; the inflammation of the absorbents subsided, and she was quite cured. Upon a relapse, however, of visceral irritation, there was a return of the irritation of the absorbents, but it was soon set to rights by the same means as before. Now we hear a good deal of morbid poisons, but there was no morbid poison communicated

by the prick of the needle. The prick of a needle, or cut of a knife, causes irritation of the absorbents, because the person is out of health at the time, and then people alarm and fidget themselves about a morbid poison; it is all nonsense, and their alarm and fidgetting only make them worse. I have pricked myself a hundred times, and only absorbed a morbid poison once; and then I had complete symptoms of pseudo-syphilis. I had ulceration of the throat, pain in my bones, for which they would formerly have salivated me till all the teeth dropped out of my head; and I was clearly infected with a poison. But in cuts from dissecting, I see no evidence of a poison being absorbed, I see evidence only of constitutional irritation, and a consequent disturbance of the secretions; the system first suffering, and the part afterwards becoming affected. I have seen very many bad cases, but have seen no evidence of a poison being absorbed. A medical man punctured his finger, irritation extended along the absorbents of the arm, and an abscess formed in the axilla. He was in such a state from the idea that a morbid poison had been absorbed, and thinking that he should have tetanus, that he would gladly have consented to the application of a blister down the whole length of the spine; but I laughed him out of his morbid opinion, and convinced him that no poison had been absorbed. The abscess broke, and a sore, bad enough to be sure, ensued; but in time it healed; and he had no secondary symptoms whatever, it was merely the result of constitutional derangement.

Absorbents of a limb become affected from constitutional disorder.

A gentleman, at present attending the hospital, had an inflammation of the absorbents of the lower extremity, dependent on a disease of the viscera. You could evidently feel an enlargement of the absorbents along the course of the femoral vessels. I have related cases of this kind, which you can read over at your leisure. I once performed the operation of tying the external iliac in a person whose health was deranged. The irritation, which afterwards ensued, produced inflammation of the absorbents throughout the limb. Œdema of the limb comes on like the puerperal œdema, or phlegmasia dolens, as it is called. Absorption afterwards took place, and the patient got well. This case I have also related, and you can read it at your leisure. I can only tell you, that the disease seems to me entirely dependent on a state of the bowels. When I visited

him every morning, I could always tell by the appearance of the stools whether his limb was better or not.

I saw a lady who had inflammation of the absorbents of the lower extremity ensue in the same sort of way. I have seen many cases of—what may I call it? *phlegmasia dolens*, the puerperal œdema, or œdema of the lower extremities, from inflammation of the absorbent vessels. I by no means wish to deny the opinion that seems to be prevalent, of inflammation of the veins being the cause of the œdema; I allude to opinions that have been lately broached and supported to a certain extent, by some medical gentlemen of this city, that inflammation of the vein is the cause of the effusion which takes place in the leg, and of course, by local measures, they try to subdue the inflammation in the vein, and by the observance of the horizontal position, to facilitate the return of the blood by the anastomosing vessels. When the inflammation of the vein is the cause of the œdema, we have of necessity an enlargement of the circuitous vessels, because the trunk which should return the blood to the larger venous trunks becomes obstructed, and the horizontal position is necessary to favour the return of the blood. But I mean to speak of a case in which there was no apparent enlargement of the superficial vessels; a case of œdema, which depends upon an irritation, and perhaps an obstruction, of the absorbent vessels; a state of the absorbents which, I say, the profession have not as yet sufficiently considered.

I will state a case to you. I remember having seen a gentleman, about two years ago, who was in a very disordered state of health; he had great disorder of the digestive organs; he had œdema come on in the arm, beginning about the elbow, and extending nearly as high up as the shoulder; the arm was swollen from the œdema, and was purple; in short, I had never seen such a case before, but in two persons who had been bitten by a viper (and that is rather a curious circumstance, that a man who has been bitten by a viper shall have œdema come on, and discoloration of the arm, from the finger even to the elbow). It is a very singular thing that it should result from the bite of a viper, yet this peculiar appearance was found in this gentleman's case. It led me to suppose that the veins were affected. I examined with great attention the veins which are situated on the inner side of the biceps muscle leading to the axilla, and there are absorbents there as well as veins, but I could not feel that the veins were thickened, or harder than

usual. I ordered him to go to bed, to keep the limb in the horizontal position, for that is of great importance in the treatment of inflammation, whether in the lower or upper extremity, for if you let the arm hang down, the blood has to ascend against its own gravity, as is the case in the lower extremities when a man is standing. I put him under a strict regimen with regard to diet, told him to be very careful about regulating the state of his digestive organs; ordered him a few saline draughts to allay the irritability of his stomach, and appease the febrile symptoms a little, and advised him to apply a bread and water poultice to the part. After a considerable time, perhaps a week, the swelling subsided a little, but the skin became irritable, and pustules formed very generally over the limb, which discharged an ichorous matter; I desired him to leave off the poultice, to wash the arm with a little tepid water, to cleanse it well from the discharge, then to use something that would correct the irritation in the skin, such as a weak solution of corrosive sublimate, and afterwards to apply some simple dressing. The excoriations got well, and very soon he got well altogether. I say, I could not, in this case, find that the veins were inflamed; for if they had been, I should certainly have felt them hardened and cylindrical, but there were no such swellings to be felt.

As he was getting very materially better, I visited him one morning, and found him in agitation, and he had been coughing up some blood. Now this was a curious thing, the man had been subject to a cough and great expectoration; when he became ill, his habitual cough suddenly left him; and when his health got better, his habitual cough returned.

It is very curious to observe the changes which take place in the system, as the result of disordered health; one disorder appears to supersede another. A physician of great repute was so convinced of this, that he wrote a book entitled, "*De conversione Morborum in Morbos.*" I mention this *en passant*, as they say, as being worthy of remark. I could tell you histories of such cases without number, but I will merely mention one.

A young man came up from Wales with an inflammation of the absorbents of the thigh, an abscess formed in the thigh and it broke; it was treated by soothing measures, and I endeavoured to put his constitution to rights, by improving the state of his digestive organs. He got well, and went again

into the country. There he hunted a great deal, and joined in all the active exercises which young men are accustomed to amuse themselves with; he partook liberally, there is no doubt, of cwrw dâ, and was laid up again, and again got better. When he came to London the third time, he came believing that he had inflammation of the hip-joint He had great œdema and swelling, and from his gait you would have supposed that the joint was really diseased; I did not think so, I thought that it was the result of a disordered state of the constitution, and of the absorbent vessels. I treated him accordingly, and he got perfectly well.

SCROFULA.

Now the glands are peculiarly liable to be disordered in a bad state of health, and we call it Scrofula; that is, if the disordered state continue for a long time. To say that the absorbent glands are not subject to scrofula would be absurd. We know that they are; there is every symptom of scrofula; there is an indolent tumefaction, succeeded by a more active degree of inflammation, followed by a secretion of a curdy kind of matter, called scrofulous matter. I am very averse to calling all intractable diseases cancerous, or all indolent diseases scrofulous. There are many indolent diseases of the absorbent glands that are not scrofulous.

With regard to scrofula, in nosological books I think it is described in this manner, as the *scrofula glandinosa, scrofula ossea,* and *scrofula cutanea.* These are the principal forms of what are recognized as belonging to the disease called scrofula. The *scrofula ossea* is an affection of spongy ends of the bones, characterized by all the circumstances which I have before mentioned. I do not mean to say that the bones themselves are the parts first affected, but the surrounding parts swell and become enlarged. As to the *scrofula cutanea,* it appears frequently to be connected with a state of the fascia; there is a discoloration of the skin, and a collection of fluid beneath it, it bursts, and degenerates into a languid sore, and does not spread, but is more tardy in healing than a healthy sore.

Indolent tumefactions of the upper lip are supposed to be marks of scrofula, and these sometimes ulcerate. But many similar symptoms occur during a disordered state of health, without of necessity constituting a scrofulous disease. But scro-

fula or not scrofula, these affections are the result of a disordered state of the constitution, and are to be removed by improving the general health. In every case of local disease, the result of constitutional causes, I know of nothing to be done, in a surgical point of view, but to soothe the part and try to improve the state of the general health. Whoever practises according to these simple principles will, I am sure, be very successful, judging from the result of my own experience. I tell you nothing but what is the opinion of the profession at large; for supposing you were to take a child, having enlargement of the glands of the neck, enlargement of the joints, and so on, to a surgeon, what would any medical man say? Why, he would say you had better go to the sea; and what would he mean by going to the sea? What but to get the child into a better state of health; not that there is any thing specific in the sea-water to cure scrofula, for there are many children who live on the sea-shore who are subjects of scrofula. He means simply, that you are to change the air and attend to the general health. I have seen many cases where the removal of persons from inland situations to the sea-shore has been attended with the best possible advantage, but still the sea-air is not specific. I say you had better put your digestive organs to rights, and in addition to the cases which I have already told you, I will give the following one:—Two children, living in the same house, had enlarged salivary glands, and osseous deposits on the fingers and toes, with all the other characteristics of scrofula. I advised them to be attentive to the diet and digestive organs, and in a short time they were nearly well. They consulted another medical man, who recommended them to go to the sea-side; they were not so attentive to these points as they should have been, and returned much worse than they went, and were very much inclined to blame the person who had recommended them to go there; but he told the mother that the children should be well fed and clothed, and so on. However, by returning to the former plan of treatment which I had pointed out, they became again perfectly well, and grew up to be fine women.

Now I come to a division of surgery which I shall not discuss at length, I mean those diseases which occur generally throughout the body, because the structure on which they occur are generally distributed. These lectures I merely consider as an adjunct to the anatomical, and I fill up in these

what I am obliged to omit in them. I have made an arrangement of surgical subjects for myself, because, I believe, till my time, no regular course of surgical lectures was delivered. Mr. POTT and Mr. JUSTIN delivered lectures on surgery, but they were not complete courses, for Mr. POTT only lectured on those subjects on which he had published. I have considered those diseases which arise spontaneously, and shall now proceed with those which arise from accident, or those—diseases shall I call them?—which are induced by extraneous causes, and these I have divided into such as are mechanical and chemical, or poisonous: and first,

OF THE EFFECTS OF MECHANICAL INJURIES.

Now these are very complex, but to understand them in combination we must first study them separately. Parts may be bruised or injured by pressure, and these are of a very bad kind: the parts may be so compressed, the structures so injured, as to lead to the destruction of the part; as for instance, a blow here, on the front of my tibia *(pointing to the shin)* might be such, that the parts between the bone and the compressing force might be bruised, or so much injured, as to destroy their vitality: such injuries are very bad. Short of destruction of the parts, what effect do these injuries produce? Why, they are all succeeded by inflammation; a bruise, for instance, may be attended by such a degree of inflammation as does not destroy the surrounding parts; the vessels may be ruptured, which is the most striking characteristic, and effusion into the cellular tissue takes place. These effusions of blood may take place, but are not always dependent on rupture of the vessels; a person, by just touching the arm of a delicate female, will turn it black and blue. Now this is not the effect of any rupture of vessels, but the effect of the inflammation which comes on. The boxers apply spirit to their bruises, and old women apply rags wetted with vinegar to theirs, by which means the effused blood is removed and the inflammation lessened.

Certain it is that effusion sometimes takes place from rupture of vessels, for I once struck my thigh against a tree, and swelling came on as big as my fist; a lump formed, and the blood was infiltrated from my thigh down to the foot. No part in particular was much injured, and the treatment was such as to allay the inflammation. I say all injuries of this

kind are followed by inflammation; bruises, spasms, or parts may be extended and sprained, and these are no slight injuries. You know what inflammation follows sprains of sinewy parts; I need not enlarge on this subject; there is jarring and concussion, and this is no slight injury. In jarring, all the viscera may be shaken, and great constitutional irritation induced. But there is no case in which this is more evident than in concussion of the brain. By the injury done to it, its functions become suspended, and then return, and inflammation occurs proportioned to the degree of injury which has been received. I say, inflammation follows all mechanical injuries. A part is wounded; what is done? the fibres are divided by concussion, compression, or sprain, or till the cohesion is broken. When there is contusion, or destruction of parts, it is called a contused wound, and when this is great, the part so injured perishes by sloughing; but where the part is broken by little contusions or destruction, just so as to permit union by adhesion, it is called simple incised wound.

Such is the nature of wounds, and the whole object is to approximate the edges of the wound together if possible, and keep down the inflammation. But suppose you cannot close the wound, and there is a chasm left; how is this chasm filled up? Why by the granular process, as in ulcers: adhesive matter is thrown out, this becomes an organized substance, the granulations are rendered vascular, and the part gradually acquires the character of those parts with which it is connected. Wounds then are filled up by a supply of organized matter, which acquires the nature of the parts to which it is joined.

The next thing to be considered is, how far parts, which are divided and detached, are capable of uniting by adhesion? You know I do nothing but follow Mr. Hunter. If there is any good in what I teach (and I do not know that there is any) it has been obtained by looking at diseases as Mr. Hunter has directed us, and adverting to their progress, as on those physiological notions which he first broached. Now Mr. Hunter divided inflammation into three stages: the adhesive, the suppurative, and sloughing inflammation. These are the three degrees of inflammation; the suppurative by a higher stage, which, if there is excessive action, leads to the third kind, the sloughing inflammation, that terminates in the destruction of the part.

I say that wounded parts will unite by the adhesive inflam-

mation. People have cavilled about this, and said that inflammation is a diseased process, and not a healthy one. Now Mr. HUNTER thought there must be some increased action in the vessels of the parts, and that this increased action was necessary to a certain degree for the effusion of the gelatinous matter with which the parts are to be agglutinated. Divided parts will agglutinate from the lowest specimen of life in vegetation, to the highest order of the animal creation. But Mr. HUNTER knew that already, and he therefore wanted to know more than was already known on the subject. Limbs of trees, for example, might be torn off, and parts of other trees might be made to grow on the same trunk. In the lower order of animals it also takes place in a very remarkable manner, but this did not satisfy Mr. HUNTER; he knew that TALIACOTIUS had cut new noses out of people's foreheads, and so on. But what did he first do? Why, at a very early period of his life, he wrote upon the natural history of the human teeth, which led him to become acquainted with Mr. SPENCER, and he observed what happened with regard to the transplantation of teeth, of which you will find an account in his history of the teeth. There was a time when, if a gentleman or lady wanted a tooth, some poor creature, or girl of the town, or a chimney-sweeper, who had a good front tooth, was induced to part with it for money, and thus the tooth from the poor person was put into the mouth of the rich. For a time it went on very well, but afterwards they had inflammation and sloughing of the gums, and symptoms resembling syphilis. I was very glad that their iniquity was punished, and the practice put a stop to.

Mr. HUNTER, with a view of ascertaining how far this power of transplantation would succeed, placed the tooth of a dog into the comb of a cock, and there it remained, and vessels were found to have shot into it when the part was injected. By a series of experiments, such as placing the testicles of the male into the abdomen of the female bird, and transplanting the spurs of the cock on the legs of the hen, and vice versa, he established a most useful rule in the practice of surgery, namely, that however parts are torn or partially detached, it is our duty to replace them, and then they will unite by the adhesive inflammation.

LECTURE XVI.

ON THE UNION OF WOUNDS BY ADHESION.

In the latter part of my last lecture I was speaking of the injuries done to the living body by mechanical causes, and I said, that parts very much torn and injured by violence being done to them, might, by approximating their surfaces, be made to unite. This union by adhesion, or union by the first intention, as the old surgeons named it, is a subject of great importance in the practice of surgery. Even parts that are completely divided may be made to adhere again, and to grow; but such parts never properly recover the feeling and functions which they possessed before. I am satisfied that the degree of sensibility in such parts is always imperfect, and the power of motion, if the part divided were muscular, is not restored to the extent which it before possessed.

So well convinced are surgeons that parts will unite, and not only that the severed parts will unite, but also that portions of skin taken from distant parts of the body will unite to each other, that they have been induced to manufacture new noses, in the way which Taliacotius recommended; but I never saw one of them yet, that could be said to be equal to a pasteboard nose. I remember a man came to this hospital, whilst I was an apprentice, who had chopped off the top of his finger; he brought the part which had been divided in his pocket; the surface of it was washed very nicely and was placed accurately upon the part from which it was removed; it adhered, the circulation was restored in it, but it was a very long time before the sensibility, or power of moving it, returned.

The parts newly united have an unpleasant jarring feel for a long time afterwards; if rubbed or struck, there is great uneasiness, and frequent pain produced; the parts do not appear to possess that share of the distribution of the nervous energy as the other parts of the body. I once tried the experiment upon myself: I was cutting a piece of cheese, the knife slipped, and I sliced off a piece of my thumb; I went out of the room, and thought I would try what could be done by replacing it; I replaced it very nicely, bound it up with a piece

of string, and it united—united perfectly; but I almost regretted afterwards that I had attempted it, for there were such unpleasant sensations in the part, produced by such trifling causes, that I would rather have been without it almost than with it. The part shrivelled and contracted in wrinkles, and I have the mark of it to this day. Still, however, it is very usefully applied in the practice of surgery, is this power of uniting parts by adhesion. A *new lip* may be formed from the skin under the chin, and really this is a very important and useful substitution, both in the process of mastication and for the purposes of speech. A gentleman of this town cut out a piece of the integument from beneath the chin, just of the size he thought would do for the lip; he fitted it very nicely into a grooved edge which he had cut to receive it, twisted it about, and it united perfectly, although it was a long time before the sensation returned in the part.

Now the consideration of this subject shows the advantage which may be taken of it in the treatment of injuries done to the soft parts. You should bring the parts into their natural situations as near as you can, and if there be any hollow left, it will fill up by the second mode of union, namely, by granulation. I cannot say that I like sutures, I would rather bring the parts together by sticking plaster, it appears to be the best mode of assisting the union of the parts. The extent to which parts will unite by the adhesive process is really sometimes surprising; I can give you an instance of this: I removed a large adipose tumour from the thigh of a lady; the extent of the incision required for the removal of it, was full a foot in length; I brought the edges of the wound together by sticking plaster, but left an opening above and below for the exit of any blood which might escape from the minute vessels; I placed the limb in that position in the bed in which it might be retained for any length of time—gave directions to the nurse that she should sponge the limb now and then with a little water, whenever it felt hotter than it should do, and gave her a little aperient medicine, and so on, to regulate her bowels. There was no hæmorrhage, no feverish irritation, no swelling; there was a sort of scab which formed over the edges of the wound; in less than a week I touched this scab over with a little mild ointment, softened it a little, and it came off; I found that the wound had united throughout its whole length, except in two places, where there were two little openings, about the size of

a pin's head, but in a day or two more these were closed as completely as the other part.

HÆMORRHAGE FROM WOUNDS.

You frequently have bleeding from wounded surfaces, and the bleeding may be considerable, although the vessels from which the blood proceeds are very small. It is of no use to put up wounds, or to bandage wounds, if the bleeding continue, because you would have to open them again to secure the vessels, and then you would be placed just in the same circumstances as when you began. It often happens that, after a trifling operation, several vessels will require to be secured; the parts are brought into contact, the patient is put into bed, hæmorrhage occurs from the small vessels, the wound is distended with coagulum, the straps of plaster give way, and out it comes. Now the coagulum of blood, confined in this way, invariably proves an irritant to the part, and tends to keep up the hæmorrhage. Well, what is to be done? There is really very little to do; you should remove all the dressings, wipe away the coagulum carefully, and if you see a vessel bleeding, secure it by a ligature. There are many vessels which will not bleed on the surface of a stump at the time of an operation, but will afterwards cause great trouble. I could tell, by putting my finger over the surface of a wound, when I touched an artery, for there is something peculiarly hard and resisting in the structure of an artery, when compared with the other parts. Wash the surface with a sponge and a little warm water, and if it will bleed at all it will then. Patients are generally cold and chilly for some time after an operation, therefore you should apply some warm water to the surface of the wound to see if any vessels there are disposed to bleed, for after the patient is put to bed and gets warm, re-action is established, and then you might have troublesome hæmorrhage. Surgeons are great wound-makers, and therefore they should be very careful and anxious to promote the healing of wounds also. I will tell you what the German surgeons do; they think so little of hæmorrhage, that they say they hardly ever need to put a ligature on a vessel; they stop the hæmorrhage by applying cold; they often put ice to the surface of a stump to check the hæmorrhage, but there is something in that which I do not like; I do not see that we have any right to take the

law in our own hands and reduce parts below their natural temperature. They leave the stump naked, exposed to the air, after the hæmorrhage has ceased, for ten or twelve hours, or until the face of it becomes covered with a gelatinous matter, and then they bring the parts together and they unite; there is no fear of hæmorrhage afterwards; there is a natural glue already prepared to unite the parts. But I do not see why we should leave the parts exposed so long; it may be said that the exposure prevents the hæmorrhage, but I will undertake to to say, that if the following practice were adopted there would be no such danger.

I should secure, certainly, the larger arteries with ligatures; I should let the small ones bleed a little and then they would stop; but I would not dress the wound, and then carry the patient to bed; I consider this a very objectionable thing, for the mere movement necessary to take him there would derange the situation of the things on the wound, and perhaps be the cause of producing hæmorrhage. I would secure the wound after I had put the patient in bed and placed the limb just in that position in which I intended it to remain. I would put on the straps of sticking plaster so as to bring the edges of the wound properly together, but I would leave openings above and below for the escape of any blood, if it should be effused. I have also used a varnish over the adhesive plaster, a solution of sealing wax in alcohol, and the mode of applying this I have described in the "Surgical Observations." If you do not allow the blood to find its way out, the coagulum separates the sides of the wound, and keeps up an irritation, and prevents the union of the parts by adhesion. If you do this, and take care that there be no accumulation of heat about the part, by the proper regulation of temperature, and avoid any causes of irritation to the system, I would engage that the wound should heal very well.

This reminds me of a subject which I should have spoken of when considering Ulcers, namely,

HÆMORRHAGE FROM ULCERS.

Hæmorrhage may take place from ulcers as well as from wounds, and it is produced by two causes; a sort of increased action is established, which produces a hæmorrhagic disposition in the newly-formed vessels of the part; and bleeding

also occurs from the complete relaxation, or weakness of the vessels. Various methods have been recommended for the purpose of restraining such hæmorrhage; they have advised the use of styptics and pressure, and so on; but as to styptics I do not like them, they are irritating applications, and frequently do more harm than good. When it arises from the first cause, lessening the temperature of the part will generally succeed, and give at the same time something to act on the bowels; you will find that hæmorrhage will cease. Mr. HUNTER relates a case in which there was hæmorrhage from an ulcer; he thought the sore had an irritable appearance, and that if it were soothed, and the irritation allayed, that the bleeding would cease; on this principle he applied a poultice to the wound, and the bleeding did cease. Then as to pressure; I do not like the idea of pushing a foreign body into a wound, to suppress the bleeding, but if it should arise from some injury done to a large vessel at the bottom of the wound, I should allow it to be very necessary to secure it.

There is a hæmorrhagic disposition, and I dare say many of you will smile at what I am going to say; there is a hæmorrhagic disposition connected with, and dependent upon, the state of the digestive organs; I'll be hanged if it be not so. You all know what a great influence the stomach has on the heart and large vessels; well, it also exerts a very great influence on the capillaries. An irritation in the stomach will excite the heart's action, and that again exerts itself on the minute vessels. A lady consulted Mr. HUNTER for a vomiting of blood; she had frequent returns of this complaint, and she was determined to hear what he had to say; she described the nature of the complaint, which was merely that which I tell you. He asked if she had a sort of premonition, or had any peculiar sensation about the stomach, by which she could tell when this vomiting of blood was to happen; she said she had; that she felt an uneasiness and fulness, accompanied with irritation of the stomach. He considered this, and advised her to drink, whenever she thought the complaint coming on, a tumbler of tepid water. Tepid water you may say, was a very singular remedy; (he did not mention that he advised her to do any thing else, but I take it for granted that he ordered something to keep her bowels regular) it produced the effect which Mr. HUNTER intended; it allayed the irritability of the stomach. and she never had another return of the disease.

There is a case recorded in the Medical Journal, by a man who had no theory to support; it is the case of what is called "The Bleeding Family." The gentleman states, that if one of them pricked a finger he could not stop the hæmorrhage, nor from a slight bruise or injury. At last he found out, that nothing answered so well as giving them a purgative, and he said, that he afterwards always succeeded in stopping it by this measure. Does not this show that it is a state of the capillaries very much dependent on the state of the nervous system, which is influenced very much by the condition of the digestive organs. I am convinced of the truth of what I tell you, but do not wish you implicitly to credit any thing which I may say, but exert your own understandings, and if you think that what has been said is not reasonable, then do not believe it. I wish this Class, and every member composing it, to be:—

"Nullius addictus in verba magistri jurare;"

but to exercise their own judgments, and have what I say either confirmed or refuted by their observation and experience. So much for hæmorrhage from ulcers; I have pointed out the frequent cause of this disease, and I have told you frequently how to remedy it.

Here again is a great chasm produced in the series of subjects which are usually treated of in a surgical course, but the diseases and accidents of the larger arteries, and the mode of securing them under different circumstances, are considered in the anatomical course, with the diseases of many other parts; therefore little or nothing remains for me to say of them here.

Reverting to the injuries produced in the body by mechanical agency, I may next speak of the accidents to which bones are liable, and shall therefore say something about

FRACTURES.

Bones, like other parts of the body, are frequently injured; the earthy matter is often broken through, and the soft parts of the bones, the sentient and vital parts, suffer laceration; there is in fact, in every fracture a lacerated wound produced; but from the vitality of the part being low, much less inflammation and constitutional re-action follow than in lacerated wounds of other parts. You bring the divided parts together

as neatly as you can, and the wound unites as any other wound would do; if the parts are lying in contact they become united by an organized medium, and after a time the *earthy matter* is deposited in this *uniting medium*, and the strength of the former structure is restored; but if the parts be not lying in contact, the space is filled up with granulations, a deposit of earthy matter takes place, and the union is accomplished in the same way.

But there is a peculiarity in the bones of young people; there is little earthy matter in them in proportion to the soft matter; the lime, constituting the solidity of the structure, may be broken through and yet the soft parts not divided. You may bend the bones of children very much without fracturing them, and this induces me to state a case which may be useful for you to attend to as young men: a child may be running across a carpet and fall down, with his thigh bent under him; he is taken up by his mother, or nurse, but it is found that he cannot stand; you may be sent for to examine the leg; you feel it over, but you do not detect the usual signs of fracture; there is no crepitus produced on twisting the limb about, and perhaps it is not much displaced. Well, you are asked what you think of it, and perhaps you may say, "I think it is only a sprain, madam, and I will send you some Goulard's wash, and you are to wet some linen rags and apply to the part." The child becomes restless and uneasy, and tosses himself about; the thigh becomes painful and swollen; it is clear that your Goulard's wash is not doing all that should be done. The friends perhaps, are dissatisfied and call in another person, and he being better acquainted with these sort of things says, "Whether the limb be broken or not, a state of rest is necessary for the recovery of the part, and the child must be kept in bed." He very properly applies a splint to steady the limb, and gives strict injunctions that the child shall be kept quiet in bed; he may direct the wash to be kept on the part, and now it does good. The swelling subsides, the pain goes away, and in about a week there is nothing left of the swelling.

I mention this to put you on your guard when you may be called to injuries received by the limbs of children; for a case like that which I have just related would operate very much to the disadvantage of a young practitioner. A state of rest is necessary for the restoration of the part. The union of a fracture can never take place unless the parts be kept per-

fectly quiet, and therefore you should never in any way, "aid, assist, or abet" in removing the patient from one place to another, or sanction any such proposals. I remember once I was fool enough myself to be overcome by the importunities of a patient who had a compound fracture of the leg; he had been in bed about six weeks, but still the leg, he said, felt very weak, and he was continually saying to me, "Oh! Sir, if you would only allow me to get on that sofa, pointing to one near his bed, I should be much more comfortable than I am here, I have been in bed now six weeks." I was prevailed upon, I say, to "aid, assist, and abet" in removing him from the bed to the sofa, and I never regretted doing so but once; the leg became uneasy, inflamed, the fracture again disunited, and he had to go through the same confinement again.

Fractures will unite if the parts be put in their proper places, without pain, without swelling, or any uneasiness; of course, I am speaking of the simplest form of these accidents; and if you will only let these alone, you will find that they will go on very comfortably. The older surgeons would never allow a limb to be opened after the fracture had been once set; Mr. Pott would say very often, in going round this hospital, to a patient who had fracture of the leg, "Is the limb easy to you?" If the man said yes, then he would reply, do not let it be disturbed; and he never allowed his patients to be looked at under a month, provided the limb remained easy. But I do not see the necessity for this; I see no harm in looking at a limb, and satisfying ourselves that it is all right.

Nothing appears more absurd to me than the rules and directions which were formerly given in surgical books about the setting of fractures; they spoke of extension, counter extension, coaptation, delegation, and so on, and of the various powers to be used for the accomplishment of all this nonsense. I say it is absurd and nonsensical, for if you only treat the injured part with kindness you may do what you will with it. It was said that great force was necessary to overcome the power of the muscles, but I say there is no such necessity; the resistance which they are capable of making has only a determinate existence, and then you may do what you please: it is seldom a difficulty of more than twenty-four hours' duration. Spasms of limbs are sometimes very severe, that I own; and it is said, that to counteract these you must give opium, I do not coincide with this exactly, for the patient after having

taken opium, will awake with violent twitchings very frequently, and disturb the fracture more than if he had taken no opium. I have known these things to happen so severely that I have been obliged to sit all night at the bed-side of the patient to keep him from sleeping. There is a state of imperfect somnolency induced by opium which has a considerable effect upon the nervous and muscular power, and the spasms into which a limb is thrown, after awaking from the sleep procured by opium, is often very severe. The manner in which I should treat a fracture would be just this: I should have the patient laid in bed, and the limb, resting on a splint, should be placed in that position in which it might remain for any length of time. I should make a gentle effort or two to replace the limb, or at least to bring it as near as I could, into the natural position without much effort. If I found the muscles pulling obstinately, I would let it alone, place the limb on a splint, and then on a pillow, so that it may rest easily. I should, in addition, just give a little something to open the bowels. The resistance made by the muscles is, I say, seldom longer than the period of twenty-four hours, and then you may manage it as you please. When once you have got the limb into its proper shape, the muscles will preserve it as pertinaciously in that position as they before resisted your attempts at doing so. Nay, sometimes they even assist in bringing the fractured ends of the bone into their proper situations. I have known many such cases, but I will tell you the following:—

A medical gentleman was walking across the room, he tripped over the carpet, fell down, and broke his leg; he contrived to get to bed, and with the help of his assistant, he placed it in a tolerably easy position for the night, and in the morning sent for me. I could trace the boundaries of the fracture very distinctly, it was a bad fracture, a very oblique fracture of the tibia, and I tried gently to replace it; I brought it a little nearer to its proper situation than before, but there was such a jerk made by the muscles of the calf of the leg, that I could not succeed in putting it exactly as I wished; I said to the gentleman, you see Sir, that it would be no use to attempt to do any more to it now, let it remain as it is, as it does not give you much pain, and I will see you again to-morrow. I saw him the next morning, but the limb was so swollen that I could not feel the edges of the fracture so well defined as before; I felt it over, however, and tried to restore

it to its natural position, and I found no difficulty in moving the limb then from any resistance of the muscles, but yet I did not get it exactly into its place, of that I am quite sure; but as the swelling went down, I found the leg was as nicely set as could have been wished; I am sure the muscles did it, for I will swear I did not. The fracture was united so nicely that I would defy any man even to say that the leg had been fractured.

You are told that fractures are to unite at a certain time, that you may know to a day when to take off the splints, and so on; but nothing is more ridiculous, in my mind, than to attempt to say any such thing. What time do they allow for a fracture of the leg to unite in? four weeks; and what time is allowed for a fracture of the thigh to unite in? six weeks. Oh! this is all folly, you cannot say when a fracture shall unite, for the rapidity or tardiness with which it is accomplished depends on the constitution of the patient. I might tell you a case which will show you that it is so. A gentleman of fortune happened to break his leg when in the country, and the surgeon who usually visited the house visited him; he seemed to have done every thing that could be done in the management of the fracture, he applied the splints and bandages very properly; but the only fault in his treatment was, that he was over solicitous that the limb should be remarkably straight, and perhaps he moved it a little too often. The gentleman remained in bed six weeks, but was satisfied that he had no power in the limb, and in a short time after he came to London. He sent for me to see him at his town house, and then told me the history of his case; he was limping about on crutches, and could not put that foot to the ground. I examined the fracture and found it had not united. "Now," said he, "Sir, what is your opinion, what would you advise me to do?" Really, Sir, said I, it may appear very cruel advice to say that you should go to bed again after having been there for so long a time, but upon my honour it is the best advice I can give you. He objected to this, and I said I was willing to see what good could be done with the crutches. I told him that the limb must be kept perfectly steady, and if he moved about on his crutches, to be very careful of his leg. I secured the limb as neatly as I could, I bandaged it, and put on Sharpe's splints. He went about six weeks after this to a house he had at Clapham, and there, unfortunately, in going into the garden

slipped his foot and snap went the leg again; he had, of course, the same sort of routine to go through again.

The bones of children, on the contrary, unite with surprising rapidity; sometimes a large bone will unite in a week or ten days, aye, I have even known them do so in five. If a child break his leg, for example, or his arm, you place him in bed, and put on splints to steady the limb, and then the child lies as quiet as possible, and the nurse and mother are continually saying, "little dear, see how good he is, he never moves, Sir, from the position in which we place him, from one end of the day to the other." Ah, certainly he is a very good little boy to lie so quiet. This state of things continues perhaps for about a week, and then the tune is altered. "There is no such thing, Sir, as keeping this child quiet," they will say, "he is continually moving about; we put him at the upper part of the bed in the morning, and you see where he is now. The little rogue, what shall we do by him?" Do by him? why let him alone; if he has crawled from one part of the bed to the other, what does this show but that he finds himself able to get there. Now I say that this shows a degree of intuitive confidence in the power of the part which had been injured. I may say that it is a sort of *animal consciousness* which enables a person to tell when the injured part is in a state fitted again for use.

Supposing that a man should have broken his leg, and at the end of three or four weeks he should ask you to examine it and tell him whether the fracture had united or not: Why, you might feel the part gently, and if there were no motion, you might say it is not worth while for me to break your leg in trying if it be united, but I will tell you what you may do; you say you feel the part strong, leave off the splints, but keep yourself still in bed, and it cannot be any great punishment for you to amuse yourself a week more by rubbing your leg up and down with your hand, and so restore the vigour of the circulation in it; draw it up and down a little every day in the same plane, and if it give you no uneasiness, gradually increase the extent of its motion, but do not put it to the ground. The time that you will employ in this way will not be lost, for you will find that the strength of the limb will be much more restored in a week's discipline of this kind than if you were to use your crutches prematurely. I can tell you a very curious case in proof of the power of this animal consciousness, I was

told it by the gentleman himself who attended the patient. A lady sent for an eminent surgeon to visit a patient at her house that had broken an arm; the surgeon, nothing loth, for he knew he should be well paid for his visit, went; he found that the lady's favourite monkey had fractured his arm, and at the particular solicitations of the lady, was induced to set the arm, which he did very accurately; he was one of the most expert men in applying splints and bandages that I ever saw. The arm was then placed in one of the lady's silk handkerchiefs, which was passed over the neck, and thus the arm was supported in a sling. Whilst the surgeon was doing all this, the monkey eyed him very attentively, aye, egad! as if he were taking lessons on the treatment of fractures, like a surgical pupil, and when it was finished, off he scampered on his three legs or arms, or what you please, and neither maid, mistress, or surgeon could catch him again for three weeks. He was seen every now and then holding it up close to his side, and adjusting the sling to make it comfortable; and as soon as the three weeks were over, he fancied that he could use his paw again, and began to scratch the silk handkerchief, and scratch he did till he got it quite off, and then ran about upon his four legs as before. I know that I do not always express to you very clearly what is in my own mind, but I think that this is a very good illustration of the feeling which I have called animal consciousness.

There are many causes which prevent the union of fractures, but I am afraid you will not be inclined to hear what I have to say on this subject to-night, and I shall therefore postpone it until my next lecture.

LECTURE XVII.

I SAID that I should consider, some of the causes which prevent the union of fractures.

NON-UNION OF FRACTURES.

If a fracture takes place in a bone, and if all other circumstances be favourable, yet if any motion be allowed from day

to day between the ends of the fractured bones, they will not unite by an ossific medium. Rest is necessary for the restoration of the parts. Union of a fractured bone is accomplished in the same sort of way as a wound of the soft parts; lymph is effused; this becomes organized by vessels shooting into it from the broken surfaces, and then earthy or bony matter is deposited, and the part acquires the strength and density of the surrounding part.

It is evident then, that if the least degree of motion be allowed between the broken extremities of the bone, that an *ossific union* cannot take place. Finding that the degree of reparation cannot be made that she wishes, Nature sets about procuring a *flexible union*. The ends of the bones become smoothed over, the ragged particles are absorbed, and the motion allowed between the bones, perhaps assists in this smoothing process. There is a sort of joint formed, and the union is effected by ligament, just as we see the fractures of the patella, and sometimes the olecranon, become united.

Sometimes, however, bones will not unite, although you may keep the broken surfaces in perfect apposition, and preclude the least degree of motion; and I am satisfied, that the cause of this is a certain state of the constitution, which renders the vacular system of the injured part incompetent to secrete the ossific matter. I can relate to you a notable instance of this:—A man came into this hospital with a fracture of the os brachii, about its middle; he was a stout man, a labouring man, a muscular man, not that sort of person to whom you might impute any physical defect, or any deficiency of the vital energy. The fracture was secured in the usual way, and very well secured, as I was given to understand; the arm was kept in splints rather beyond the time usually considered necessary for such a fracture; but when the splints were taken off, there was a movement perceptible between the broken extremities of the bone. The arm was done up again very carefully, and allowed to remain so a longer time than at first; but when examined the second time, the fracture was found not to have united. There was nothing particularly wrong in the man's health; his stomach, bowels, and so on, appeared just as they should be. Well, he remained in the house a long time, and every attention was paid him, but still the fracture would not unite, and he was obliged to leave. He went to his work again, but he found very great inconvenience

from the loss of power which the limb had sustained, and after a certain time he came into the hospital again, and said that he wished we would do something for him; that he was willing to have dóne whatever was considered necessary, and so on. It was really very curious to see this man attempt to lift any thing from the ground; he bent his elbow well enough, but there was another flexion in the middle of the upper arm, by which he lost considerable power.

Mr. Long proposed cutting down upon the end of the bone, and reducing it, as near as could be, to the state of a recent fracture; he made an incision on the outer edge of the deltoid muscle down upon the ends of the bone, and he turned them out through the wound. He chipped and rasped off all the smooth surfaces from each, and then returned them to their proper places. The arm was very properly secured, but the bones did not unite in any other way than they did before. It might have been supposed that an operation of this kind would have been attended with very considerable constitutional irritation, fever, and high local inflammation, but there were no such results; it appeared only like a flea-bite to him, as they say; such was the torpor and non-excitability of his system.

A gentleman in America proposed, that in these cases, where union did not take place, instead of cutting down upon the bone, and removing a portion of it, to pass a seton between the surfaces, and in this way to excite a sufficient degree of irritation in the parts that might lead to the deposit of adhesive, and afterwards of ossific matter. It is a mode that I believe has been frequently practised, but seldom found to succeed. I have known several instances in which it has been tried, but the degree of disturbance in the part, and in the constitution, has been so great, that they were very glad to lay the practice aside.

It has been often requested by patients who have had fractures, whose cases have terminated thus unfortunately, to have the extremity removed. Formerly it was very much the practice with surgeons to do so, but I do not think it commendable. You may ask, What is to be done in such cases? To which I should answer, that I believe that by keeping those fractures, in which flexible union has taken place, perfectly quiet for a longer time than is usually done, that they often will unite. I have known many examples of this kind, but I will only tell

you two. A sailor came into this hospital, who had fractured his arm, by a fall from the main yard on to the deck; it had been treated in the usual way, and been kept quiet for the usual time, but yet a flexible union had taken place. The man was so annoyed at having this joint in his arm, that he came here for the purpose of having it removed. I told him that if it were my case, I should not have my arm so treated, but advised him to have the limb kept perfectly still for some length of time, and that there was a probability of the part uniting by bone. I explained to him what the object was, and how it was to be accomplished; and you must, in such cases, explain to the patients the necessity of strictly preventing any motion between the fractured portions. The patient was kept on what is called a generous diet, his bowels regulated, and his digestive organs improved. Now this man had his attention rewarded by a firm union of the fractured arm; he was very attentive in keeping the splints sufficiently tight, and would not allow of the least motion.

The other case, which I said I should mention, is that of a gentleman of fortune, who broke his thigh in two places. It was a very bad fracture; it was secured in the usual way, as I understood from him, and kept the usual time in the splints; but it was found that neither fracture had united by bone. It was really distressing to see him attempt to walk, he tottered on one side, his thigh bending under him, as if he would fall to the ground. Consultations were held with some of the leading men of the profession, about the propriety of removing the limb, and they appeared to think that it was necessary. I dissented, however; I told him what I have before told you, that I should not be disposed to have the limb removed before I had tried what could be done by strictly preventing motion between the fractures for a longer time than is usually considered necessary. I said, if it were my own thigh, that I should be disposed to treat it in that way. The gentleman consented, the thigh was placed between splints, the limb was properly attended to, and bony union took place. I am induced then to think, that if fractures will not unite by the method which I have mentioned, they will not unite either by sawing off their surfaces, or by exciting irritation in them by setons. I have told you what I know respecting the non-union of fractures, and of the probability which exists of causing even old fractures to unite, by observing perfect quietude between

their surfaces for a considerable time. I shall now say something about

COMPOUND FRACTURES.

There is merely this difference between a simple and a compound fracture; the one unites by adhesion, the other by granulation. In a simple fracture there is blood first effused between the ends of the bone, which becomes, in a great measure, absorbed; the coagulable lymph of the blood remains, and perhaps there is addition made to it; this becomes organized, and thus a vascular medium is established between the broken surfaces. I said, when speaking of the union of the soft parts, that the newly organized structure gradually acquired the structure of the surrounding part; so it happens in the reparation of injuries of the bone, (for you will recollect that I am now speaking of such as are produced by mechanical violence); the uniting medium having been rendered vascular, gradually receives the deposit of ossific matter till the chasm between the two surfaces is completely filled up. Now what happens in simple fracture by the adhesive, takes place in compound fracture by the suppurative inflammation, or by granulation. When you consider that bone, in a healthy condition, is an insensible part, possessing a low degree of vitality, and therefore not being very readily excited to action; when, at the same time, you recollect that the filling up of chasms, even in the soft parts, by granulation is a very tedious process, compared to the adhesive union, you need be at no loss to discover why a compound fracture should be a more important injury than one which is simple.

The mischief does not result from the exposure of the broken surfaces to the air, but to the removal of the blood which was poured out between the surfaces of the fracture. In a compound fracture, the layer of coagulated blood putrifies and passes away by the wound of the soft parts, consequently there is a chasm formed, and how can it be filled up but by granulation? Vessels cannot shoot from one surface to another when separated, but there must be some medium; you cannot glue parts not lying in contact. The hollow then is filled up by granulation, just as a hollow in the soft parts, with this difference, that bone is deposited in the granulations. Here again we may see another cause why an injury of bone should

be much longer repairing than a proportionate injury of the soft parts. The vessels of a bone are extremely minute, they cannot therefore secrete pus so readily, or the adhesive matter that is to form the substance of the granulations. Again, we know that there is a greater degree of constitutional disturbance attending the suppurative than the adhesive inflammation, and that granulations which for a time looked healthy and vigorous, all of a sudden become changed in their character, and even absorbed ; we notice these transitions in the states of ulcers, where the granulating process is immediately brought into view, and we cannot but suppose that the same causes will produce similar deviations from the healthy action in granulations between the extremities of bones.

The consideration of the different methods in which simple and compound fractures unite, and of the dangers and difficulties frequently attendant on compound fractures, induced Mr. Mudge, of Plymouth, to adopt that treatment which appeared to him to render the compound fracture, as nearly as possible, in the same circumstances as the simple ; he accordingly closed very carefully the external wound, he brought its edges together by sticking plaster, and then he varnished the plaster, so that the broken bones were covered almost as effectually as by the integuments. He has recorded many cases in which his practice was successful. It is undoubtedly right to protect the fracture from the danger which would attend the suppurative process, you should therefore, after you have reduced the bones as nearly as you can, bring the edges of the wound together, and you may varnish the dressings, for if you can prevent the escape of the coagulated blood, the fracture may unite by the adhesive process. In addition to this, you should recollect that the soft parts in the immediate neighbourhood of the fracture are frequently very much injured, and the result of injury is an inflammatory process. Now you may very much lessen the subsequent inflammation, and prevent the suppurative inflammation by diminishing the temperature of the part. There is no measure so useful in the treatment of inflammation, as that of the regulation of temperature. Lay thin cloths, wetted with Goulard's lotion, or spirit and water, over the part, to diminish any excessive heat that may accumulate there, and you will do a great deal toward preventing the evils frequently attendant on a compound fracture. Keep the parts at the same time on a splint, and perfectly quiet. It is

a point of very great importance in the treatment of fractures, to place the limb, and to place the patient in that position from which there may be no necessity to remove him for a considerable time.

COMPOUND FRACTURE, WITH HÆMORRHAGE.

It sometimes happens that there is considerable bleeding from a compound fracture; the sharp end of the bone has pierced a vessel, and hæmorrhage follows. It has been recommended immediately to enlarge the wound, if you cannot get at the vessel without, and by tenaculum to pull it forth and secure it. I cannot say that I should approve of such a practice; one wound is bad enough, you need not make two. And as to stuffing foreign bodies into a wound to stop hæmorrhage, oh! that is a very bad practice. I can only tell you that I have known large arteries, aye, even the principal arteries of a limb wounded, and yet the hæmorrhage was stopped by a different method. I remember two cases of compound fracture of the leg, in each of which I had every reason to believe that the anterior tibial artery was wounded. There was a considerable flow of blood from the wound, the cellular membrane was distended with effused blood, even down to the foot. I directed the limb to be put in a proper position, so as to replace, as near as could be done, the ends of the bone, and directed cold rags to be applied to the part, so as to allow of evaporation; the temperature of the part was lowered, and the hæmorrhage ceased. The other case was under the management of one of our dressers here, I think Mr. GEORGE, now settled at Bath; he very properly laid the limb on a splint, so that there might be no motion allowed in the fracture, and then he fitted another splint over the limb, in this manner: he removed a piece of the middle of the splint, so as to leave the part uncovered, but he connected the two pieces together by a bracket of iron, so that the splint acted as effectually as if it had a continuous surface. On this part he applied his cold lotion, and the bleeding was effectually stopped. From these, and many other similar cases, I imagine that you may generally command the bleeding from arteries in compound fractures even of some importance, by the use of cold applications.

OBLIQUE COMPOUND FRACTURE, WITH CONSIDERABLE PROJECTION OF BONE.

A fracture may not only be compound, but there may be considerable projection of bone through the external wound. In a very oblique fracture the bone may protrude very much through a small opening in the integuments. A person may, in jumping from a carriage, or from a height, fracture his leg, and fracture it so obliquely as even to push it through his boot. What are you to do in that case? The bone is protruding considerably through a small opening in the integuments, from the obliquity of the fracture. I remember a time when this used to be a frequent question put to those who presented themselves at Surgeons' Hall, by the leading men there, and especially by Mr. POTT, who was one of the first men of his time as a surgeon. What would you do Sir in such a case—would you enlarge the wound or saw off the bone? I know that the answer expected was, "I would rather enlarge the wound than saw off the bone." Mr. POTT had a great objection, and this objection was general at that time, to the removal of any part of the bone, if it could possibly be avoided. If any spiculæ of bone should make the surface of the fracture appear ragged, then it is a different thing, they may be removed, because in attempting to return them you might irritate very much the neighbouring soft parts. They objected very much to the removal of any considerable portion of the fractured bone, and so do I object to it. Whatever portions of bone you may remove, leave of necessity a chasm to be filled up, which must be greater or less in proportion to the size of the pieces of bone which you may remove. The only way in which it can be filled up is by granulation, and the demands made upon the constitution, under the suppurative process, are very great, producing that degree of irritability called hectic fever. I say, then, that I would rather enlarge the wound than saw off any considerable portion of the bone.

COMMINUTED COMPOUND FRACTURE.

You may have a compound fracture, in which the bone is very much smashed, broken into a great many pieces, which might cause you some perplexity. It has been said, that you

should take away the loose and detached portions, but I do not see what advantage can be derived from picking away the smaller pieces of bone. The only mode in which the fracture can unite is by the separate pieces of bone becoming vascular, and the probability appears to me, that they will become more so when the pieces are small than when they are very large. If there should be any projecting, detached pieces of bone, which appear very much to disturb the proper position of the fracture, you should take them away ; but it is impossible to lay down any such precise rules in surgery that will direct one in all cases, unaided by the conviction of a man's own judgment. You must, therefore, be very much guided by circumstances ; if you could replace the different pieces of bone, and by giving the part a moderate degree of support, retain them there, I think you had better not take any away. It is of no use to strap and bandage a fracture to make it unite by main force—that will never do. The support you should give a fracture should be gentle and equable, just such as it would derive from the healthy state of the parts.

I remember being told the history of a case of compound fracture by Mr. Pott, which shows what nature alone will do in such injuries. Mr. Pott was a man on whose veracity I could rely, and when I told the story at the College of Surgeons, some of the old codgers there, said they recollected the case ; that was confirmation strong enough, if any had been required.

A lunatic, in attempting to get over a wall, fell, and produced a compound fracture of his right leg ; the bone projected a little through the skin ; the opening was enlarged, and the bone reduced. The fracture was secured, first by a many-tailed bandage, then by splints, the wound carefully closed by plasters, and the surgeons who had the management of the case went away consoling themselves with the nice apposition of the bones. The man paid particular attention to the manner in which they applied the apparatus, and as soon as they were gone, he took it into his head to remove the bandage and splints from the injured leg, and set them on the other. He discovered a hole in the feather bed, on which he was lying, and he thrust his fractured leg into the middle of it. When the surgeons arrived on the following day, they were much pleased with the look of the limb ; they said, how straight it lies ! It appears not to have shifted in the least from

the position in which we placed it; what little swelling there is! indeed there is scarcely any, and so on. They thought it would turn out an excellent case. They came from time to time, to see the patient, and finding every thing going on so favourably they did not disturb the limb. After the usual period had elapsed in which such a fracture is supposed to unite, they took off the splints to examine it. They were quite surprised to see how straight the leg was; and as to the wound in the integuments, it had closed so nicely, that they could not even find the scar. Well, this is very extraordinary! But, said a person present, is this the leg that was broken? Broken! yes, certainly. Could it be supposed that the splints were put on the wrong leg! However, it was thought they might as well look at the other, but it was not to be found. After some search it was discovered buried in the feathers, and when they pulled it out, egad! it was crooked enough, with a large bunch of feathers sticking into the leg, like a cockatoo; yet the wound had healed and the bone united.

Fractures may not only be compound and comminuted, but they may be produced in such a way as to cause extensive injury of the neighbouring soft parts; for instance, the wheel of a heavy carriage passing over the limb, or gun-shot wounds may do so much violence as to render it probable that the parts will slough; under such circumstances, it has been a question of some consideration whether an operation for the removal of the part should be immediately performed or not. Mr. Guthrie, who has written on this subject, recommends immediate operation; but you must take this into account, that his conclusions are drawn from peculiar circumstances. Mr. Guthrie, as a military surgeon, has perhaps seen the necessity of amputating *sur la champ*, because the wounded can only be taken away in heavy, rough-going carriages, which would, to a certainty, injure the parts materially, and very much preclude that chance of success that might otherwise be gained by delay.

Other persons have advocated the same opinion; but I should rather defer operating, if possible, until the next day, if I could, or the next evening. The constitution has sustained a very serious shock by the injury, and the additional shock of the operation, before the first was recovered from, would, in many cases, extinguish life. I know that the practice of imme-

diate operation would not succeed among Londoners, for there are very few of them capable of sustaining such a two-fold injury. I should say, wait; tell the patient that you will leave it until the next day; let him recover a little, and prepare his mind for it. If you find, that the circumstances of the case are not more urgent on the second day, say you will put it off a little longer. It is astonishing to see what little impression a large wound, such as that formed in amputation, will make upon the system when it has been for some time previously suffering from considerable irritation. Amputation of a limb from a man in full health is known, by experience, to be a very dangerous thing, and therefore I should recommend you to wait a little after the receipt of a violent injury before you perform it.

RULE FOR AMPUTATION.

It does not follow, as a matter of necessity, that because a bone is comminuted, and the soft parts contused, that you must operate. There are many such cases that do well. What then are the principles which ought to guide you in determining on the operation? It is quite impossible to lay down any precise rule, founded on this or that appearance of the parts; or to say, that because this artery is torn, or that part is injured, or that the bone is broken in this or that manner, that you must operate. But if there appears to you to be such mechanical injury done to the living structure as could not be ultimately repaired by nature's processes, or, that if reparable, would, from the state of the person's general health, make such demands on the vital powers as they could badly support, then you might be justified in amputating. Rules! there is but one rule in surgery, or there ought only to be one rule—*Do unto others as you would have done to yourself.* If you can say, after asking yourself, if your limb were in the condition of that man's, you would have it removed, then I say, you can with great propriety recommend it.

Operations, generally speaking, do much better in the country than in town; and injuries requiring amputation in town will often be recovered from in the country. Of amputations, I have found that those of the thigh do better in London than those of the leg, especially about the calf of the leg; and those of the upper arm do better than the fore-arm. The question which we have been discussing is one of great importance, and erves your most serious attention.

LECTURE XVIII.

I HAVE, in my former lectures, described to you some of the injuries done to the body by mechanical causes, and I have now to consider such as may be produced in a different way; such causes as cannot be said to be mechanical, but are called *chemical*. Now all kinds of chemical agents, viewed in their operation on the human body, may be reduced to two kinds, such as are irritating, possessing a kind of acrimony, and such as are destructive. These agents may be applied in a sufficient degree of acrimony to destroy parts, to decompose the animal structure; or we may have them applied in a less degree, and produce a great and violent irritation of parts, short of destroying their vitality. Now as fire may be considered the most powerful chemical agent that can be applied to the human body, and as it produces effects in proportion to its degree of intensity, so I think, in short, that the treatment of burns and scalds will include all that can be said in relation to the nature of chemical injuries.

BURNS AND SCALDS.

Now I say that the vitality of a part may be destroyed, or even the animal structure decomposed by fire; but these parts which are so acted on cannot be the subjects of surgical treatment. What is to be said of parts violently irritated? As boiling water cannot decompound a part of the body, but is capable of irritating to a great degree various parts of the body, and as parts actually decompounded cannot be the subject of surgical treatment, so all that can be said about burns, is included in that which will be said respecting the treatment of scalds.

Boiling water does not decompound animal matter; but the heat of boiling water will effect the coagulation of the animal juices which are capable of coagulation; of crisping up and coagulating the fibres of the cellular substance, because they are principally made up of gelatin. It will crisp up the rete mucosum, so as to cause a separation of the cuticle, but it does not decompound or kill the animal matter by its chemical pro-

perties. It is, however, violently irritating, but short of that power which produces the decomposition of the part. Now, sloughing, or the death of the part does sometimes take place from scalds but not always; in some persons more than in others; and when it does take place, it is the effect of the re-action consequent upon the high degree of stimulation which the part has sustained, and not as an immediate effect of the injury.

When I spoke of the injuries done to the body by mechanical violence, I said that they were succeeded by inflammation, and that the most successful mode of treating that inflammation was by the reduction of temperature; as by evaporating washes cooling down the heat of the parts, thereby, checking the progress of inflammation. What should you imagine that a person would do, supposing him to have scalded his fingers, and I suppose there is not a person to be found who, in some part of his life or other, has not done so. You know very well that there is a very unpleasant feeling produced, a sort of pricking and tingling with heat, and a man almost instinctively pops his hand into a basin of cold water, and he finds it very comfortable; but when he takes it out the same sort of feeling returns, and he soaks it in the cold water again; but as soon as he removes his hand there is the feeling of heat still remaining, like an unquenchable fire.

This method of treating scalds sometimes answers very well, and I have heard a story in proof of it which I will tell you. It was said that a man, by some accident, spilt a kettle of boiling water over both his legs, so that they appeared to be scalded equally. His wife brought him immediately a bucket of cold water, and he put one leg into it; it happened that there was only one bucket in the house, and the other leg remained in *statu quo*. The doctor was sent for; when he arrived, he ripped open the stocking on the leg not in the water, and after he had seen it he treated it in his own way, and then wanted to look at the other; but the man said, "No, I thank you, that is very comfortable, and so I shall let it stay where it is. Now I tell you what, doctor, I'll take care of one leg, and you shall take care of the other; and I will make you a bet that the one I take care of shall be well first." The report says, that the patient cured his leg first.

Then again, on the other hand, it has been said, you must not put your hand into cold water, but you must hold it to the fire for the purpose, as the old women say, of drawing out the

fire. That you are to hold your finger sufficiently near the fire to feel the heat of it, and that you are to hold it there until the pain goes away; this may appear rather strange, but there is more plausibility in it than you might imagine, as you will afterwards see. Some people then are for treating scalds with cold applications, and others are for holding them to the fire; contradictory, certainly, and yet each at times succeeds.

TREATMENT OF SCALDS.

It was left, however, to Dr. KENTISH to publish what might be called a rational method of treating these injuries. He said, that the principle that should be adopted in the treatment of scalds should be such as is adopted when a part has been exposed to a very low degree of cold, when it is what is called frost-bitten. Experience has shown, that a sudden transition from extreme cold to heat causes the destruction of the part so circumstanced. When speaking to you on the operation of cold on the vitality of parts, I said that the method adopted was that of the gradual application of a stimulant, until the part was brought to the natural temperature, and that by this practice people saved the tips of their fingers and noses when frost-bitten. Dr. KENTISH proposed treating parts, the temperature of which was raised much beyond the natural standard, on the same principle, and thus gradually to lower the action of the part, by the use of a stimulant weaker than that which produced the scald, until the part was restored to the state of an equilibrium, that is his expression, and I really cannot find a better to express what is meant. A person after having been severely scalded is chilly and cold; his pulse is feeble and faltering; his respiration is rather hurried and weak; the nerves of the part have sustained a great injury, and the whole nervous system suffers a great shock. He therefore took the patient into a room where there was a fire, and brought him near enough to it to feel the warmth of it, yet not so near as to increase the pain, and he applied to the part *oil of turpentine* mixed with *basilicon*. His constitutional treatment corresponded to the local, for he gave the patient warm wine, or some cordials, and a few drops of opium, and afterwards had him removed to a warm bed. After the patient has been in bed a short time the pain goes away, for it generally happens that he falls into a tranquil sleep. When he awakes the pain is gone.

I deem this to be a rational mode of procedure, and I think it has been condemned because it has not been properly understood. Recollect, that this stimulating plan of treatment is not to be continued after the equilibrium of the temperature is restored, and after the patient has a degree of heat and re-action established; oh, hang it! no, no; that was never intended: you are then to dress the scald with mild applications, and leave off giving the patient cordials He may then require a little aperient medicine, and so on. Neither are you to put your stimulating applications on a surface which has been denuded of its cuticle; where the cutis is bare even the mildest application to that will give great pain. You should in that case give the patient a small quantity of opium in a little cordial, and put a little simple dressing on the sore. Dr. KENTISH says that he has found finely-powdered chalk, with which a small quantity of opium has been mixed, the best dressing for such scalds; it appears to allay the irritability and promote the cicatrization of the sore.

But supposing a scalded part to be followed by an inflammatory action and sloughing, there is oftentimes a great demand made upon the vital powers of the system to separate the slough, and even the energies of the parts surrounding the slough become fatigued; they are in that state which Dr. BROWN calls *indirect debility;* and in such cases, a gradual support to the system will be necessary. This is one of the most delicate points to manage in the treatment of scalds and their consequences; the degree of support necessary to afford the system, without the danger of bringing about too much re-action. There is a very great sympathy between the surface of the body and the stomach: when a person has an extensive injury of the skin, he is, as I before said, chilly, and has a feeble pulse, but he has also vomiting very frequently; the stomach is evidently disturbed. The digestive organs suffer, and Dr. KENTISH has recorded a very notable fact to this effect: he observed that many of the injuries of the skin cicatrized rapidly, when a spontaneous purging came on, and this led him to prescribe, in some cases, aperient medicines, with a view to facilitate the healing process, and he says that he succeeded very frequently in accomplishing it.

I can speak from experience and say, that the mode which I have now mentioned of treating scalds is a successful method,

if properly managed. We have had a great many such injuries in this hospital, and since this plan has been pursued, they have generally done very well. There were a great many cases of burns and scalds brought in last Winter, and there was not one of them but did well. The great point to be attended to is, to leave off your stimulating applications, when the parts have recovered their equilibrium of susceptibility; to leave off your internal stimulants at the same time. I have many times, in going round the hospital, asked the patient, How long has it been before you felt ease in the part after you had been drest? and the answer generally is, "In about ten minutes or a quarter of an hour, Sir." Well, supposing that it requires fifteen or twenty minutes; what of that? I know that it requires a much longer time to procure ease by the cold applications. I have known patients brought in two days after they had been scalded; they had used cold applications, but the fire was not out of the part, as they say; and by using this stimulating application alone for about fifteen minutes, the pain has been removed. If the stimulants be continued beyond this time, the patient becomes exceedingly irritable, restless, and delirious; and such cases frequently terminate fatally.

I have been accustomed to say to this class, I should recommend you to try experiments as to the advantages of the two methods of treating scalds, which I have this evening mentioned. I should advise you to satisfy yourselves by experiment, and I do not know a better way of deciding the point, than by dipping two of your fingers in boiling water, let them be fairly scalded, then take them out, put one into a basin of cold water, and dress the other with the turpentine and basilicon, and then you will know in which the pain is removed first. I do not want to try the experiment, because I am decided already as to the superiority of the latter method, and have therefore no occasion to scald my fingers, you know.

When the fire happened at Covent Garden theatre, there was a very great multitude of people assembled; the Volunteers were called out, and a great many persons were burnt. They were taken to the different hospitals, and most of them died. Those who were brought here were treated as I have told you, and were getting on very well indeed for a few days; but the statements made in the newspapers, respecting the unfortunate sufferers, induced many persons to send them various good things, which there is little doubt they continued to dis-

pose of; they had been eating and drinking in this sort of way for a day or two, when the greatest part of them became highly irritable and delirious, then became comatose, and they died with great rapidity. I recollect, also, that one of the Volunteers of the Bloomsbury division was burnt, and was a patient of mine; he went on exceedingly well for some time, nobody could be doing better, and in one night he became so much worse, that they considered it necessary to send for me in the night. I happened to be out of town, and a gentleman being with me at the time, went. When I returned in the morning, he told me, "Ah! Sir, that poor fellow is going in the same way as the rest of them; I am afraid he will go too." I went and saw the patient, found him delirious, and getting comatose. I inquired what he had been eating the day before; he had partridge for his dinner, and had drunk, even from their account, more than he ought. I ordered him a purge with water-gruel, and he recovered.

Sometimes the shock made on the constitution is so great from an extensive scald, that re-action never takes place, and the patient sinks in a state of stupor, this is especially likely to happen in children. I do not know that I can explain to you what I mean on this subject in a better way than relating the following anecdote. A medical man was prosecuted for killing a child by giving it opium, at least that was said. I happened to be in the country at the time, and was strongly solicited by him to attend and give my opinion. I considered that the character of the profession was at stake, and although rather against my inclination, I went. After waiting in a cursed crowded court the greatest part of the day, I was called upon, and placed in the witness box. The lawyers had taken it into their heads that the child had died from maltreatment on the part of the surgeon; the child had been scalded severely, and he had given opium, and they thought they should be able to make the jury think as they did. The first question put to me was, "Mr. Abernethy, will you inform us what is considered the proper treatment for scalds? This was a question broad enough to be sure; I was puzzled a little how to answer it; I did not know but that they would require a lecture on burns and scalds. I considered a minute, and then said, "That which was adopted in the present case." Oh! that was what they did not expect; it was giving a turn to the case which they did not like. "You have heard the evidence, have you not?"

"Yes, but it is contradictory." "But judging from the evidence?" "I have no right to judge, you may judge if you please, or the jury may judge, but I shall not." "But I ask you, for the information of the jury, your opinion respecting the opium, whether you do not consider it too large a quantity for a child?" "The statements respecting the opium have been contradictory, but admitting that the child had, as was said, eight drops immediately after the accident, and ten drops two hours after, I should say that the child had not taken one drop too much." "But are you aware that the child had no pain?" "Yes, perfectly; when the skin, or any other part of the body is severely injured, the nervous system of the part is so affected that the peculiar actions of the nerves on the brain or spinal marrow, by which alone pain can be felt, do not take place. A man may have a serious injury inflicted by a mechanical cause, may have his leg smashed, and nearly torn off in machinery, and yet feel little or no pain; and we are in the habit of giving, in such cases, wine and opium, not to act as a narcotic, but to arouse the energies of the vital powers, and call them again into exercise; the nervous system has sustained a great shock, from which it requires to be roused." The judge said, "that he understood it, he saw the principle on which the treatment was founded, and had no doubt but it was correct." "But, my lord," said the counsel, "the child slept to death." "So he may have appeared to sleep, but he would have done so if no opium had been given; it was the torpid state into which the nervous system had fallen which caused that appearance, and from which the child could not be roused." Here the business rested; the treatment was admitted to be correct, and the character of the gentleman exculpated.

Scalds are very serious injuries; some people have said they are not; but I am sure that I have seen persons die who have been scalded, where you might not expect such a result. The injury done to the skin was not so extensive as would lead you to suppose that death would happen. The function of the skin becomes destroyed, the impression made on the whole nervous system is very great, and they appear to die under the force of that impression. But supposing a man to have fallen into a vat of hot water, up to his middle; egad! I think you will allow that to be no trifle. The one half of the external surface of the body having its function destroyed,

cannot be considered an affair of little moment in the animal economy. Oh! no, it is a very dreadful injury, and often takes away life. When the skin is so seriously injured, its sensibility as well as its function is destroyed, and therefore those are the very worst cases in which the patient has no pain. If a man scalds his finger, he feels that sharp enough, and he jumps about squeaking or bellowing just as he may fancy; but I have seen the poor creatures who have been brought into this hospital with extensive burns, toss themselves about in the bed without complaining of much pain, until they have stripped the cuticle off the burnt parts. Oh! only consider what a dreadful state the nervous system must be in to admit of such injury being done to the skin without producing pain. Well, I believe I have said to you all that I can say on the subject of chemical injuries; I have considered that parts must be either irritated or destroyed by them; the effects of fire on the body admitted of the readiest exemplification, and I have, therefore, told you the best treatment that you can adopt in such cases. I must now proceed to speak of a certain class of agents which are neither chemical nor mechanical. What are they then, you may say? They are what have been called poisons.

OF POISONS.

They are such substances as exert a peculiar influence on the living structure, not referable either to mechanical or chemical actions. I do not know that I can define them better than SHAKSPEARE did; he said that they were things "which hold an enmity with blood of man," so they are; they disturb the vital functions.

I do not now intend to speak of narcotic or vegetable poisons, I shall do so afterwards; but I intend to say something about those, which being generated in an animal body, produce a matter that shall, when it is applied to another body, produce a like matter. There are not only poisons which produce only like poisons in animals, but there appear to be particular classes of animals on which only certain poisons appear to operate. Man, for example, has measles and small pox, yet his dogs and cats have no such diseases, although living in the same house with him. Mr. HUNTER, with a view of ascertaining this point, made some experiments with the matter of

chancre on dogs; he inoculated them with the chancrous matter, but it produced no bad effects in them.

Mr. HUNTER was of opinion, nay, firmly believed that there were certain parts of the body susceptible of particular diseases, and that poisons take their different seats in the body as if they were allotted to them. From observing that some poisons, generated in the human body, were capable of producing like diseases in another body, he was induced to use the term "*morbid animal poisons,*" in order to distinguish them from those poisons with which some animals are furnished for purposes connected with their economy; but I do not see how this can be held with propriety; a poison may be the result of diseased actions, or it may be capable of producing diseased actions, but it is not the disease itself; the term, morbid poison, appears to me to be a solecism in language. You may say that a poison is capable of exciting diseased actions, and therefore morbific would be more correct than morbid. I shall therefore use the term

Morbific Animal Poisons.—Such are the poisons of small pox, measles, syphilis, and so on. Their influence on the body seems to depend on a certain combination of matter possessing a very subtle and peculiar power; just such as constitutes the differences in the colours of the rays of light, or in odours. Vegetables emitting various odours, having an almost endless diversity of colour, are found to be made up of the same elementary parts. It appears to me, that the effect produced by the application of certain poisons to the human body, depend on some peculiar combinations of matter, with which we are at present unacquainted, resembling these minute modifications of matter, which give rise to the differences of odour and colour in vegetables. Dr. ROLLO, of Woolwich, a man very fond of trying experiments, with whose name I dare say you are well acquainted, who wrote on diabetes, had to inoculate some soldiers, and he did it in the following manner. He put some of the variolous matter in a watch glass, dipping the point of his lancet in it, he introduced it under the skin; he inoculated six men in this way. He then exposed the remainder of the matter to the action of oxymuriatic acid gas, as it was formerly called, chlorine now, I suppose, and with this matter he inoculated six other men. Those whom he inoculated first had the small pox completely produced in

them, but the others had not. This will explain to you partly what I mean.

I shall commence with the description of the effects produced on the human body by the application of the matter of *chancre.*

LECTURE XIX.

CHANCRE.

I shall give you the definition of chancre according to Mr. HUNTER, for it would be really almost a profanation to doubt the accuracy of that man's description. He was a very attentive observer, and therefore I say, that I think I cannot do better than give you his definition. He says, that a certain pimple takes place, or a speck of inflammation like what is called a pimple, and produces a sore; this sore seems of a circular form, not perhaps precisely circular, it may be a little oval; but generally circular, for a disease beginning in a point, and spreading equally in all directions, is likely to be circular. There is a thickening round the ulcer, which is the effect of the adhesive inflammation in the surrounding parts, so that it has a thickened base and edge; this is what he says: There is a thickening of the parts round the ulcer, forming what may be called the bowl of the ulcer. There are no granulations—no attempt at reparation; it is an ulcerative disease, and I know that such an ulcer used to go on from bad to worse, until mercury was employed for its counteraction. An ulcer of this kind is now so seldom met with, that many practitioners entertain a doubt whether there ever was such a disease as Mr. HUNTER described. But he detailed a particular disease, and it would be absurd to suppose that he did so from fancy and not from reality. In truth, these ulcers have become very scarce to what they were in the early part of my practice, but as far as my trifling corroboration would go, it would go to support Mr. HUNTER's assertion, as to its formation and progress, and that it continues to increase, unless counteracted by mercury. I have given a case in my book, which in all its

circumstances precisely agreed with the description of the disease given by Mr. HUNTER. It was unchangeable in its character; it regularly and progressively got worse when no mercury was employed; but as soon as it was employed, the further increase of the disease appeared to be checked, but from the insensibility of his constitution it was difficult to excite its specific effect; he went on taking it month after month, rubbing in and taking blue pill, and the disease was at last removed when an adequate mercurial affection of the constitution had taken place.

The question is, whether there are any varieties of this kind of ulceration. But it is really not worth while to discuss this point, because we see so little of that kind of disease now; and all that can be said of any practical import, amounts to this; if you see such an ulcer progressively increasing, until mercury be used, you must use it, but not in such a way as to injure the patient's health.

THE EFFECTS OF MERCURY, AND THE MANNER OF USING IT.

Now with regard to the use of mercury, it was well to note the disease which led to the use of mercury, so also it is right to inquire into the effects and modes of using it. Before Mr. HUNTER's time, it was thought that mercury acted chemically on the poison of syphilis circulating in the blood, and so operated as to destroy it; but he considered it as an antidote to the poison in a different way; he says, a disease may excite one state of action in parts of the body, or of the whole system, and mercury may excite an opposite kind of action, and that the two kinds of action cannot exist at the same time. He explained what he meant in this assertion by a reference to small pox, measles, and other specific diseases. Now I think Mr. HUNTER was perfectly right in supposing that mercury did not act chemically, because a little mercury will change the action of a chancre in one person, whilst a large quantity of mercury will not produce a beneficial action in another. But if it acted chemically, we might suppose that the action it would exert on the sore would be in proportion to the quantity of mercury employed. It is pretty clear that it is owing to the changes produced in the actions of the vascular system that the good effect of mercury is to be traced. But no metal in its reguline state will have any effect on the animal system; it

is necessary that it should be oxydated, and the most simple form of metallic substances is that in which the oxygen is combined with the metal in the lowest degree, or such as is at present called a protoxyde The attraction of adhesion, or affinity, as the chemists say, becomes weakened by trituration with some viscid or unctuous material; the metal combines with the oxygen of the air, and forms a greyish powder, which is called the oxyde; if the metal be rubbed down with hog's lard, it forms the blue ointment; and if rubbed down with conserve of roses, it forms the blue pill; the chemical state of the mercury in both preparations is the same, but a man would not like to bolt lumps of hog's lard. There is also the mercury ground down with chalk, and the same state of oxydation is produced. These are the most simple forms of mercury; but there are various other combinations of mercury, with different acids, which are very useful. There is the combination of it with chlorine, forming calomel, and so on; but as the preparations partake of a saline nature they become more irritating, such as the combinations with the nitric and muriatic acids, but those which I first mentioned are in most common use. Now the object in giving mercury in the syphilitic disease, is to mercurialize the system in the mildest possible form, not to irritate as a saline preparation might do; but I say to produce a mercurial action of the system in a mild way.

It is generally used either in the form of a blue pill or blue ointment, for they are, as regards the mercury, the same thing. In one way or other, then, the mercury gets into the system, and produces a peculiar state of constitution; it increases the pulse; there is a greater excitement of the heart and arteries; after a time it increases the secretions, it affects the mouth, the gums get swollen and tender, there is a copperish taste in the mouth, and the flow of saliva is increased. They have been seeking for quicksilver in the secretions, but they have made no hand in finding it out, although I have no doubt but it is there. A watch gets tarnished in the pocket, and how could this happen unless there was some sensible change produced in the perspiration. Persons afterwards get very weak, languid, and thin, and therefore it is evident that it produces absorption. People get lean whilst using mercury; there is a kind of febrile excitement induced. Now this state of the system, kept up a proper time, was known to cure the chancres completely; granulations would shoot out, and the cicatrizations of the

wound take place. The disease used to be cured outright, and never returned. But we use mercury for other diseases, therefore it is well to know what state of the constitution is induced by the mercury. I should add to the account which I have just given, that there are some whose mouths you can never affect by mercury, and yet have the mercurial diathesis of the system induced. Surgeons, without considering this circumstance, continue to push the use of the mercury until the general health is very much injured indeed, from their supposing that a salivation is necessary.

I recollect the case of a fine young man, who was a clerk in a merchant's counting-house, he wrote beautifully; he was directed to take mercury for a sore on the prepuce, which was supposed to be syphilitic; the mercury did not affect the salivary glands as much as the surgeon wished, and the mercury was pushed further, and so it was continued until he became very thin and languid; the muscular power was so much destroyed, that he wrote like an old man; the muscles of his arm had become quite paralytic. I advised him to give up the mercury immediately, which he did, and he soon recovered.

Sometimes it produces profuse perspiration, and occasionally it affects persons like lead does; it produces a metallic colic; there are gripes upon gripes in the bowels, attended with mucous and sometimes bloody discharges. This sort of metallic colic is relieved by opium. I know that I have seen many such cases occur whilst patients have been rubbing in for the venereal, and I have said, you must give them small quantities of castor oil every day to clear out what mercury they have in their bowels, then give them some opium, and let them leave off mercury. When this has been done they have soon got well. I say, then, that in giving mercury, you should not stimulate by giving the saline preparations, but use the mildest, as the oxyd, and I consider that the ointment is the best form of getting mercury into the system, except the fumigation, which ought not, certainly, to be lost sight of. The mode of affecting the system with mercury by fumigation is rarely employed now, and when it is employed, it is done in a coarse and bad manner. LALONETTE, a physician in Paris, published a book on *mercurial fumigation*, in 1776. The method which he adopted to procure the powder for fumigation was operose and complicated. I consider, however, that it is a mode of procedure which has been too

much neglected. I have succeeded effectually in procuring the effect of mercury on the system through it, and of completely removing the syphilitic disease. The plan which I generally adopt is this: I place the patient in a vapour bath, in a complete suit of under garments, tuck a cloth round his chin, and let him remain in the machine about fifteen or twenty minutes, and the whole surface of the body becomes covered with a whitish powder; he should then go to bed and lie in the same clothes till morning, and then go into a tepid bath. I used to say that it was the most gentlemanly way of curing the disease that I knew. The powder which I used for the fumigation is the calomel deprived of its acid, by being washed in water containing a small quantity of ammonia. A certain quantity of it is put on a heated iron and placed in the machine in which the patient is sitting. It is a very curious thing that two drachms of this grey powder consumed in fumigation daily, is equal to the same quantity of blue ointment rubbed in every night. I have known patients salivated by the absorption of the mercury used in fumigation in forty-eight hours. It is a method which should be borne in mind.

Mr. HUNTER was of opinion, and so was I, that the syphilitic poison might be modified by the diseased propensities of the constitution, so as to produce sores of divers characters. This supposition may be very reasonably doubted indeed; the most dissimilar sores were reputed venereal sores, and that the difference of appearance was owing to the constitution of the patient so attacked. Now this was a very curious thing, and it was a notion which I had a helping hand to subvert. I was induced to consider this point whilst a young man, and I saw there was a great variety of sores that broke out on the genitals, a very great variety indeed, they resembled the old syphilitic chancre in some points, but differed essentially in others, and I will tell you some of the differences.

SORES RESEMBLING CHANCRES, WHICH ARE NOT CHANCRES.

Sores often break out upon the genitals which very much resemble chancres; there is a form of ulcer, beginning with a sort of pimple too, and spreading about a little, it may throw forth exuberant granulations; but it is not so in the true syphilitic chancre, there is no growth of flesh, no reparatory action until the progress of the disease be put a stop to by

mercury. There may be even a number of such sores break out on the prepuce, behind the corona glandis; but in the true syphilis there is not more than one sore, or two at most, but here is a crop, quite a lot of them. Then again, some of these sores will spread in the circumference, and get well in the centre. Some sores indurate, forming a sort of thickened base, like an areola, and yet have little disposition to ulcerate. Some sores spread *herpetically*, healing in one direction and breaking out in another, and spreading from one district to another. Now with regard to all herpetic sores, I affirm that they cannot be syphilitic; for if you meet with a sore healing in one direction and breaking out in another, what is the inference but that the morbid actions of the sore have exhausted themselves, or that the disease had worn itself out, as Mr. HUNTER used to say. Now, if no mercury be employed in herpetic sores, it is evident that they will sometimes get well, and if the sore were syphilitic it would not heal without mercury. It is clear then that herpetic sores cannot be syphilitic.

Some sores spread in such parts as have little life, as in the cellular substance beneath the skin, leaving the skin entire, and forming burrowing sores. Oh, hang it! they were horrible things to manage, were these burrowing sores. I have known them spread beneath the integuments of the penis upwards, towards the abdomen between the muscles, causing extensive suppurations, and doing great mischief. When I saw such sores extending towards the scrotum, I used to say to the patients, it is a lucky thing, for they must come to the bottom soon. It is not probable that such sores are syphilitic.

Some sores spread by sloughing; venereal sores will sometimes suddenly slough, but you must understand that I call such sores venereal as are the results of modifications of animal matter applied to susceptible surfaces by sexual intercourse; but by no means possessing the character of chancres as described by Mr. HUNTER, which I distinguished by the term syphilitic chancres. The old surgeons used to say that there was no occasion to employ mercury for a suddenly sloughing sore, and they attributed the absence of secondary symptoms to the chancre having been removed by the sloughing of the surrounding parts. Some sores slough from the edges, whilst others spread from the whole surface, and the sloughing may

extend in particular directions. Celsus has described eight species of sores as affecting the genitals of the Romans in his time. It is difficult to make out his description exactly from the different terms which he uses; however, he has described some *phagedæna*, ulcerating phagedænic sores; and he has described ulcers as *gangrenosa nigritiæ*, sores spreading by sloughing from their edges.

Many sores seem to break out on the genitals from a kind of irritation on the system, and break out in a kind of crop; they come out suddenly, and, without taking any adequate quantity of mercury to remove them, they, from some change again taking place in the general health, suddenly disappear. They break out in succession, and sometimes after considerable intervals of time, which would render it improbable that they arose from infection of the ulcerated part, since such sores would probably continue. My attention was attracted to such sores when quite a younker. Some medical men took it into their heads to consult me for such sores after they had been salivated for some time, and yet the sores did not heal. They are fretful sores without reparation, the edges of the sore are thickened, and the middle of the sore throws forth an exuberant fungus, and the granulations rise above the edges of the sore, but the edges remain diseased and retard their healing. These sores are slow in healing under any mode of treatment, and they generally get well in the same succession as they broke out. It matters not what applications you make to them in the first instance; the irritation subsides in the prepuce, to which they appear to be generally owing, and they heal. You may touch them, after a time with the argentum nitratum, and I know nothing better, but do not do so at first. You should see that cleanliness is observed; do not suffer any accumulation of the discharge to take place behind the glands, but bathe the parts frequently with a little tepid water; but it sometimes happens that you cannot retract the foreskin, and then you may use a syringe, and inject some tepid water freely behind the glands, not with a little foolish syringe, with which you cannot use any force, but one that will hold about two ounces, and force out all the accumulated morbid and irritating secretions. Do not stuff down pieces of lint, as I have often seen done, for what good can you expect from it! If you were to push lint round the healthy glands, and allow it to remain there, it would irritate and produce sores.

Mechanical injuries done to sores, as must be done with the lint, are very likely to aggravate their irritation. Oh! hang it, these cases go on badly, because people are negligent of these particulars; they put things upon the sores which tease and irritate them, and make the bad worse. When I was speaking to you on the subject of ulcers, I said that you should allay the morbid irritability of the sores, by the application of a stimulus, and induce a new and more vigorous action in them.

I have often seen patients having such sores about the prepuce and glands in this hospital, and I have said to them, go to bed, and, in the name of heaven, keep yourselves quiet; lie on your back, and support the parts by laying them on a little pillow, wash with a little tepid water, and so on.

The object of local treatment is to soothe and to cleanse, and afterwards apply a gently stimulating application; to such sores as you suppose will require the use of mercury internally; you may also apply it to the sore, and there is nothing so good as the *black wash;* it is made by mixing calomel and lime water together, (two drachms of calomel to a pint of lime water) and you have just that powder produced which LALONETTE recommended for the purposes of fumigation. You may dress the sore lightly with a bit of lint, dipped in the wash, and do not stick a boss of lint into the wound. I have said that you may touch the edges of the sore with argentum nitratum, but it does not destroy the nature of the sore, only the hardened state of the edges. And I know of no better treatment for sores with hardened edges, than that of touching them every two or three days with the argentum nitratum; it does not, I repeat, change the nature of the sore, but it counteracts the morbid conditions of the sore. So much then for the local treatment.

THE USE OF MERCURY.

Now, as to mercury, some people say I will not give mercury at all, and I will give sarsaparilla; but this is not what I should do. I believe that mercury, to a certain extent, will counteract the progress of the specific malady, and as some sores are doubtful, as you cannot exactly decide whether they are specific or not, I should advise you to give mercury. Tell them to take the blue pill, but take it mildly. If the sore be

syphilitic, it will heal under the administration of mercury, and if it be not syphilitic it will do no harm, but frequently promote the healing of the sore. People come to me and say, Is this sore syphilitic? how shall it be treated? Why, I tell them to do this, if it be at all doubtful: Take five grains of the blue pill every night, for about a week, and if you do not find the sore improving, take five grains also in the morning; continue it about a week after the mouth becomes affected, and if it be a syphilitic sore it will heal, and if not, the mercury, if you take care of yourself, will do you no harm. The patients are satisfied, and so are you, for, egad! if any thing like constitutional symptoms should appear—although they might not be of a syphilitic character, and that such a state may happen I shall afterwards show—oh! they will be inclined to blame you very much, and say, Ah! you see what a state I am in; if I had taken mercury, I should have been well. And their friends will say, Ah! if you had gone to Mr. such-a-one, he would have given you mercury, and you would have had nothing of this kind happen. Therefore I say, give a mild unirritating mercurial course, and it will do no harm if it does no good.

THE CONSEQUENCES OF SUCH SORES.

They produce abscesses in the glands of the groin, by the absorption of the poison from the sores; at first the gland enlarges by a slow kind of inflammation, it becomes afterwards more active, and suppurates; an ulcer is formed, which any man who was not very sceptical would acknowledge to be very much like chancre. The ulcers which form after the abscesses have broken in the inguinal glands, are as various in their appearance as the primary sores. Now, Mr. HUNTER said, that where the glands in the groin are affected, that the sores, produced by the ulceration, should be dressed with blue ointment, but it appears to me too irritating, the black wash is the best application that I know. He recommended the ointment to be rubbed in on the inside of the thigh, that, as he said, the same vessels which had imbibed the poison might take up the antidote. And then Mr. HUNTER put in a very curious paragraph, in which he says, that he never saw a bubo suppurate where mercury had been in this way employed. Yet the contrary is the case; this is generally observed, but for my own

part, I have seen that mercury rather hastens than retards suppuration in the inguinal glands. Whenever disease is induced in the absorbent glands, that disease will be aggravated by the state of the general health, and therefore you should principally attend to that. If a man consult a doctor when he has enlarged indolent glands, what does he say to him? Why, you had better go to the sea side, and what he means by that is, that he should get in better health.

PHAGEDÆNIC BUBO.

When a gland in the groin has suppurated, it often happens, from the patient being out of health at the time, and irritable, that the disease spreads, perhaps also by sloughing. Now it would be absurd to suppose that you could cure this by mercury; you must quiet the disturbed action in the part, and correct the state of the constitution. Common diseased actions are not curable by mercury, and this is the result of common inflammation in an irritable habit. Now the sores which I have described are local, and yet they produce constitutional symptoms. But if you give the patient mercury, then you are acquitted in his mind, although it might not have been absolutely necessary, and although the constitutional symptoms would have subsided without it, by regulating his general health.

LECTURE XX.

BUBO.

On the last evening I spoke about buboes, but I had not time to finish the subject. There are some buboes that become very indolent, and form burrowing sores; and I do not know that there is any good in cutting all these open. I believe what I said on the subject of fistula, connected with abscesses, is applicable here. The state of the abscess depends upon the state of the general health; if you improve that, the sores will soon heal. Mr. Hunter showed that the syphilitic virus must pass

through various glands before it reached the circulation, yet it only irritated the first glands through which it passed, and I think that this may be referred to the modification which the virus undergoes in its passage through the absorbent glands. It is probable that the poison of syphilis is of so acrid a kind, that it always excites local ulceration of a peculiar and progressive nature, whilst the varieties of morbific poisons, which produce symptoms resembling the constitutional effects of true syphilis, may be absorbed without any evident ulcer. It was formerly considered necessary to continue the use of mercury until the bubo had quite healed; but if the ulceration were specific, the mercury which had been taken to cure the chancre would be sufficient to cure the bubo, or a very little more.

Sores caused by the breaking of buboes are sometimes very indolent and intractable, and form numerous sinuses, and yet all of a sudden, after they have resisted the use of mercury a long time, they get well, clearly showing, that it is to some change in the constitution of the patient that we must look for the cause of the sudden change in the local action. I will tell you the following case, which appears to show that it is so :— A gentleman had an enlargement of the glands in the groin, following a sore behind the corona glandis ; the inflammation in the gland became more active, and ultimately suppurated. When the abscess burst, an indolent sore was formed ; there was no attempt made at reparation, but, on the contrary, sinuses extended in various directions, and, although he put himself under the influence of a full mercurial course, they would not heal. A consultation took place, and it was decided on that the sinuses should be dilated, and I was requested to dilate them ; they extended in different directions ; one toward the pubes, another toward the spine of the ilium, and a third toward the thigh ; but what good was there in doing that ; when they were laid open there they remained in the same indolent inactive state as before. The patient was recommended to go to the sea, and so he did, and he came back just as he went. He came to me again when he returned, and complained very much of the state of the parts, how miserable he was, and so on. I told him not to be out of heart ; that I was sure they would heal, but I could not tell when, but that it would be as soon as his constitution had improved. He went afterwards into the country again, and it was really curious to see what

happened; cicatrization suddenly took place on the surfaces of the wound, and there were flaps of skin hanging about in different directions, which had not united to the parts beneath, yet the surfaces had cicatrized.

VENEREAL SORE THROAT.

One of the consequences of the absorption of the syphilitic poison is a sore throat; it is then called the venereal sore throat, and Mr. HUNTER has described the kind of sore that forms there, and I believe that he must have seen a disease which he described so accurately. He says, that an ulcer formed on one or both of the tonsils, of a circular or oval form, having a thickened base and edges, the surface of which became covered with matter, and that it gradually increased, until it acquired about the size of a sixpence, and then remained stationary, and that it would not heal unless mercury were employed. This is the venereal sore throat as it used to appear, and which was removed when the antidote was employed; but I had seen such a variety of sores upon the tonsils, attended with eruptions of a livid colour on the skin, and which were not curable by mercury, that I began to doubt whether they were syphilitic or not. It used to perplex me very much when I heard the surgeons say, oh! this sore throat is decidedly venereal—these eruptions are decidedly venereal, and he must take mercury; and yet I found that the rubbing in did not cure the ulcer or remove the eruptions. I say, I was exceedingly perplexed at it; I could not make it out to save my life; but when Mr. HUNTER's book came out, oh! then I saw it clear enough. Oh! said I, it appears then that there are some constitutional affections, generally considered to be syphilitic, which are not so; this explains why some sores should not heal under the use of mercury, and others, after mercury had been used extensively, get well apparently spontaneously. It was a point which I never afterwards lost sight of, as I shall hereafter have to tell you.

It usually commences with a little stiffness about the neck, and some pain in swallowing, and this induces the patient to look into his throat, and there he sees an ulcer, sometimes such as I have just described, at other times more irregular in shape, and it would be almost impossible to say, from the appearance of that ulcer only, whether it was venereal or not

But you must trace the history of the case, and if you find the sore to be extending under the use of ordinary methods, you had better give a moderate quantity of mercury and watch its effects. Sometimes the soft palate becomes the seat of a foul intractable ulcer, which often destroys the velum palati, causing a distressing difficulty of swallowing and imperfection of voice.

VENEREAL ERUPTIONS.

These eruptions, which take place on the skin consecutively to an absorption of morbific poison on the genitials, are exceedingly varied in their appearance, just as much as the sores which first appeared on the genitals or in the throat. The characters usually given to such sores are, patches first appearing on the skin, of a darkish colour, somewhat like copper; after a time there is a secretion from the surface, and a scab forms, which often degenerates into a foul intractable ulcer. Many eruptions take place on the skin which are not at all syphilitic, and which are owing to a disordered state of the general health, especially a weakened and debilitated state of the system; but there is one distinction that may be observed in such sores as are the result of a morbific poison, and such as are attendant on a weakened state of the system. The first appear more generally in such parts as are nearest the centre of the circulation, as about the chest, neck, &c., whilst the others occur in the extremities, and appear to extend toward the trunk. These are frequently removed without the use of mercury, and are, in the legitimate disease, only to be relieved by mercury. The nose is often attacked by ulceration, which proves very destructive. I have been accustomed to assign the syphilitic affections of the nose an intermediate place between such diseases as are placed on the surface of the body, (and I consider the throat as being lined with cuticle, and part of the surface of the body,) and such affections as are seated more internally, as the bones and joints. I do so, because it is difficult to determine whether the ulceration commences first in the lining membrane of the nose, or in the periosteum, and thence proceeding to the Schneidderian membrane.

The patient complains first of having some uneasy sensation or tickling in his nose, and if you have been accustomed to see cases,

you can immediately detect what is the matter by the patient's voice; he snuffles when he speaks: if you say to him, Do you blow your nose? "Oh! yes, I calculate, I guess that I do." Do you find any white or greenish matter discharged? "Yes, very frequently." Does the matter discharged smell bad? "Yes, very bad indeed; and I can scarcely bear my own breath." Indeed you seldom need to ask the question, for you may smell the breath fetid enough at a pretty considerable distance. It often happens that the disease is very much aggravated if you push mercury far; you must administer it in moderate quantities if the patient have other symptoms that induce you to think that the disease is syphilitic, and if he have not taken a proper mercurial course. But if he has done so, and the primary sores have healed under the course, and the use of the medicine has been continued some little time after the sores have got well, then you had better not give any more, but attend to the general health, and treat the inflammation in the nose as you would any other, on general principles.

NODES.

When inflammation attacks the periosteum, it causes an effusion of a fluid matter between it and the bone, or a thickening of that membrane merely; a swelling forms on the bone, and is called a node. If the inflammation which caused the swelling be removed early, the node will again disappear, but if allowed to remain a short time, an incrustation of bone takes place, and the swelling becomes permanent. Syphilitic inflammation generally attacks the bones nearest the surface, bones that are but slightly covered, as the tibia, cranium, sternum. The characteristics of this inflammation of the periosteum are, first, an aching, gnawing pain, confined to some particular part, and that pain having intermissions; the pain in the part comes on about ten at night, and continues until three or four in the morning, and throughout the day it is very much easier, or disappears. This pain is relieved by mercury and opium; the mercury removes the syphilitic disease from the bones as it does from the soft parts, but not so readily. It is then an aching, gnawing pain, increased during the night, and abating through the day. The patient should wear warm clothing, rub in the blue ointment every night, and take a moderate dose of opium; attend at the same time to the state of his bowels, so

as to keep them soluble, and the pains and also the node will go away. But I have often known patients expose themselves to cold, and sometimes to wet, during the time they have been taking mercury very plentifully, and they have had pains in their limbs again, even worse than before. I have heard patients often complain in the hospital, when they have imprudently exposed themselves, and gone out and caught cold, "Ah! Sir, I am worse than ever; the pains are all returned; and instead of having them in one part only, I have got them in twenty places; such pains in my thighs, shoulders, knees, and so on, that I cannot sleep for them." Well, you know by their description that this is not a syphilitic pain; it is too wandering, it attacks too many parts, and is not followed by the distinct remission of pain during the day, as in syphilis. Oh! no; it is a rheumatic pain. Rheumatism attacks the parts, and you try to remove it by sudorific medicines, by the warm bath, and by alterative doses of mercury, and although the complaint is very obstinate, you may sometimes succeed. I have known pains in the limbs, and nodes, form on the periosteum of the different bones, which appear to result from some modification of animal matter acting as poison on the constitution.

Many such cases have I seen, of diseases resembling the syphilitic, which were neither regular in their progress or cure. The following case in particular, of a medical student of this hospital, made a great impression on my mind. He had, in opening a bubo, scratched his finger, and infected it with the matter. The scratch fretted out into a sore of about the size of a sixpence; it was very intractable and slow in healing. He became rather anxious, and consulted me. I said that I did not think it a syphilitic sore, for it had not the thickened edge and base, and other characters of a venereal chancre. I therefore recommended him to try the effect of local means, and not to take mercury. It went on very indifferently for about a month, and then began to heal a little, and contract in its circumference. About this time a swelling took place in the arm, just on the inner side of the biceps muscle, where the absorbent vessels pass, and he came to me with a doleful countenance: Oh! Sir, I am infected to a certainty; see, here is a bubo forming in my arm. I examined the sore in the finger, and as I saw it was disposed to heal, and that it had no surrounding induration, I said to him I will not practise my pro-

fession any longer, if that bubo is syphilitic, for I'll be hanged if it's any use. Do not take mercury, but apply a spirituous lotion to the part, or the Goulard's wash : under this treatment the tumour in the absorbents subsided, and the sore healed. About three weeks after he called on me, and said that he had ulcerations in his throat, and requested me to look at them. I did, and I saw ulcers deeply excavated, with irregular edges, with a surface covered with matter, and just such as would induce any man to say, who had not considered the subject, that they were venereal. Shortly after, some copper-coloured eruptions appeared on his face and breast. Ah ! it is all over with me now, said he, one day ; I wish I had begun to take mercury at first. I would not take mercury if it were my case, I told him ; but advised him to have the advice of some other surgeon, and hear what they would say. They told him it was a decided venereal eruption, a decided venereal sore throat, and so on. They advised him immediately to go into lodgings, and begin to rub in, for that the mercurial course had been very improperly delayed.

Whilst the patient was looking out for lodgings, in order that he might go through the mercurial course, a circumscribed thickening and elevation of the pericranium appeared over the frontal bone, nearly as large as half a crown : it was as fine a corona veneris as any man could wish to see. I shall never forget the agitation and the alarm in which he called on me one morning. I am ruined, I am ruined ; see here, see here, putting his hand to the swelling on his forehead, is not this syphilis ? What do you think of it now, Sir ? Think of it, why I am more convinced than ever that it is not syphilitic ; for I never knew in my life the occurrence of a true venereal node simultaneously with sore throat, and so on. Just as he was about to commence his inunctions, he was required to go into the country on some very important business, and away he went, considering himself as the most unfortunate fellow on earth, a ruined man, and so on. I advised him not to be out of heart about it, for I was sure he would get better. But, oh ! there was no consolation to be afforded him, and away he went with a batch of the blue ointment, determined to rub in on the first opportunity. In about a fortnight after, I heard from him : he said he had been very much benefited by his journey ; that he had been prevented from beginning the use of the mercury : that he did not think he should now use it at

all, for that his throat was getting well very fast. Soon after the node disappeared also, and he perfectly recovered, without any mercury having been used.

PSEUDO-SYPHILIS.

There are so many instances in which certain diseases appear, resembling the constitutional effects of syphilis, in which it is morally certain that no syphilitic virus has been absorbed, that I have called such appearances the mock-pox, or more classically, pseudo-syphilis. I showed that there was a very great number of sores resembling chancres that were not chancres, and I mean to show also that there are a great many constitutional diseases produced, in some cases from the absorption of morbific animal matter, and in other cases spontaneously, from a disordered state of the constitution itself.

First, of these *appearances resembling the syphilitic*, which may be traced to a *constitutional origin*.

In the earlier part of the Lectures, I attempted to show the importance of correcting disorders of the digestive organs in endeavouring to cure local diseases. In the cases which I am now considering, a disorder of these organs constantly exists, and produces, or at least aggravates and protracts a state of weakness and irritability of constitution, to which the origin of these diseased appearances must be undoubtedly referred.

Ulceration of the throat, copper-coloured eruptions, nodes on the bones, have all taken place in persons to whom no venereal poison had been communicated. Men of undoubted veracity, married men, clergymen, men and women advanced in life, who would have no inducement to conceal, nay, whose interest it was to communicate the information of any illicit or suspicious intercourse. There is a very striking case that I will mention to you; it is the case of a student who was at the school of Windmill street; and it came under the observation of the late Dr. Baillie. This pupil came from the West Indies, and was observed by the others to look very much out of health; they inquired of him what was the matter, and he told them, that he had nodes upon his shins. Nodes upon your shins! said they, there is no doubt then as to what you ought to do, you must undergo a mercurial course. He declined doing this, and, continuing to look unwell, they teased him again, and said, we will speak to Mr. Hunter about you;

you had better take mercury. He agreed to see Mr. HUNTER, who examined the swelling; he thought it was a venereal one, and advised him to take a mild course of mercury. But on asking him if he never had any other symptoms, a sore throat, a chancre, or eruption, he said no, he never had, but that he recollected having such a swelling on his leg when quite a boy. This made them hesitate, and, on being questioned as to whether he ever had any sores on the genitals, he said no, he never had; this was doubted by some of them, and he acknowledged, what he appeared reluctant to do, that he never had any sexual connexion in his life. He took sarsaparilla, and the nodes got well without his having taken a grain of mercury. Now this account shows, if any man's word can be relied on, that diseases resembling syphilis can arise from disorder of the health, even without any sexual intercourse.

There is no doubt, on the other hand, that there are certain modifications of animal matter, that will produce appearances resembling the venereal, in constitutions having certain morbid propensities. Even the irritation of a morbid secretion in one person applied to the susceptible surface of another person, is capable of producing these results. How common is it for a morbid irritation about the nipple of the nurse to produce an ulcer in the lips of the child, spreading to the cheeks, and even afterwards of producing eruptions of a suspicious character. Many law suits have I known instigated upon such accidents, and the parents have been inclined to anathematise the nurse, and say, Oh, what a vile hussey that nurse must be to take our heirs to suckle, to ruin the hopes of our family with such a loathsome disease. Nurses have often made similar complaints, and said, what a shame it was to send me their child to nurse, and give me such a horrible disease. I remember a case very confirmatory of this proposition. A young married couple came up to town to spend their honey-moon or moons, and see all the fine sights of London. They took up their residence at a splendid hotel, and they discovered, about the end of a fortnight or three weeks, that they had each numerous sores about the genitals. There was a great kick up about it; they called in a great number of medical men to consult what was to be done, and sent for me. There was a fine crop of warty excrescences about the genitals of each, yet there was not the least reason to suppose that either had any gonorrhœa, or any other complaint previously. What was to

be done? Oh! it was very clear; I told the husband, what must first be done; one of the parties must return to the country, and that they must observe strict cleanliness, use a little astringent wash, and take a little medicine to set their digestive organs to rights. They adopted this plan, and soon got quite well. Mr. HUNTER has devoted a chapter of his book on the venereal disease to this subject, the appearance of constitutional symptoms from the infection of morbific animal matter, in which he mentions the evils which sometimes follow from the transplantation of teeth. It used to be the fashion, if a lady or gentleman wanted a front tooth, to apply to a dentist, who found some poor girl of the town, or chimney sweep, who had a front tooth of about the same size, and, by offering them money, got them to consent to part with it. The tooth was drawn from the poor person's mouth and put into the socket of the rich; the gum would adhere very firmly for a time to the surface of the tooth, and appear to do very well, but after a certain time, the gum would separate, the tooth get loose, and drop out, an ill-conditioned ulcer form in the part, followed by sore throat, eruptions, and nodes, symptoms of pseudo-syphilis, as I call them. This soon put a stop to the transplantation, and glad am I that such iniquitous practices were punished. It is a subject well worthy of attention, and I hope that you will not only receive it because I tell you of it, but investigate the matter strictly and impartially for yourselves.

LECTURE XXI.

ON THE TREATMENT OF DISEASES RESEMBLING SYPHILIS; OR PSEUDO-SYPHILITIC DISEASES.

I HAVE already shown you that there are certain states of the constitution, and certain local diseases produced by the absorption of an animal poison, over which we have no immediate control; for which we have no specific remedy. What, then, you may ask, is to be done? In truth there is very little that need be done; the principal thing to be attended to is, to re-

move the morbid propensities of the system by improving the general health, and the manner of doing this I shall presently have to show you. Improve the general health! but this is no easy thing in some persons. The nervous system is going on wrong, and the digestive organs are out of order. As I have told you before, if I could strengthen weak nerves, quiet irritable nerves, and set the stomach and bowels to rights, I should be able to cure all the diseases that you could bring me.

Now if these diseases, which I have called pseudo-syphilitic, were curable by mercury, "we might make short work of it," put the patients under a mercurial course for a given time, and all the symptoms would disappear, never more to return. This used to be the case formerly with the disease which Mr. HUNTER described, and which was the true syphilis. But now we have a host of diseases very much like syphilis, but not syphilis, because they are not curable by mercury, but are sometimes aggravated by it, and frequently return after a full course of mercury, sufficient to remove any form of true syphilis, has been given. But if we have no specific remedy for such diseases, we must try to remove them by attending to the state of the general health, and I will tell you what I direct the persons who come to me to do.

I tell them that they must pay strict attention to diet; nothing can be done without this, they must not overload their stomachs with such quantities of vegetable and animal matter as the stomach cannot digest. By remaining there, it undergoes decomposition it is true, but not such as can be converted into the nutritive material of the body, but proves a source of irritation and disturbance to the system, and I maintain, that if a man were to do as Cornaro did, put no more food into his stomach than his stomach could digest, he would be rewarded for it by a long and happy life. In addition to a strict attention to diet, I advise them to drink sarsaparilla, which appears to have a good effect in allaying the irritable state of the system. They should drink a pint of the *compound decoction of sarsaparilla* during the day; instead of drinking with dinner a large quantity of fluid, as is generally done, let them drink a half pint of the sarsaparilla about four hours afterwards; another half pint should be taken in the evening. The medicinal virtue of the decoction will be increased if some of the extract of sarsaparilla, containing the active properties of the plant, be added to the decoction, say a drachm

to the pint. Gentle aperient medicine should, at the same time, be taken to keep the bowels regular, and there is nothing better for the purpose of improving the secretion from the liver than a few doses of the blue pill. Of this I shall speak more at large presently.

Now I affirm that I have known many cases of sore throat, eruptions and nodes, disappear under this treatment, where no more mercury than an occasional dose as an aperient had been used. If you have not had the opportuuity of seeing the primary sore, you must watch for a short time the progress of the constitutional symptoms before you decide upon adopting a full mercurial course. People very often come to me and say, "Is my sore throat syphilitic, must I take mercury?" I tell him candidly that I cannot always say at first sight whether a sore is syphilitic or not, but my opinion is very much influenced by the history of the progress of the constitutional symptoms; and that if I may be allowed to watch the progress of the disease itself I shall be able to decide. In the mean time I put them upon the plan of treatment, as to regimen and medicine, which I have just mentioned.

But on the other hand, if the disease should be seated in an important part, and if the disease appears progressive, so as to threaten to destroy the part, even although it is probable that the disease is not syphilitic, then the most effectual mode of stopping the increase of it, is to exhibit an *alterative course* of mercury, with the compound decoction of sarsaparilla. But it is extremely difficult to find out that quantity of mercury which may be given as an alterative without producing such a mercurial influence on the system as to irritate and disturb it. The quantity that would act very slightly on one person would be quite sufficient to salivate another. I have in many cases observed the progress of the symptoms for a sufficient length of time to assure myself that they were not syphilitic, yet I have found them yield as if by a charm, on exhibiting mercury in alterative doses, short of that of producing salivation. Whereas a long continued course of mercury, in which that medicine may be employed very freely, will not only fail to remove these symptoms, but, contrarywise, aggravate and increase them. To show you that this is the fact, I do not think I can do better than relate the following case:—

An officer, who had been in that division of the army of the Peninsula, under the command of Sir John Moore, had endured

great fatigue and privations, by which his health had been very much broken up. He embarked for England from Corunna, and a short time after he had been in this country, he consulted me for an ulceration in his throat. There was a large oval ulcer in one of the tonsils, deep, and covered with a white matter. He had also eruptions of a copperish colour, and was very much out of health. I could not obtain a clear account of the history of the case, and it was such an ulcer as would have induced any man to suppose that it was syphilitic. Whether it was syphilitic or not, I told him that the state of irritability in which the system then was, rendered it very improper to commence a mercurial course, and I advised him to delay it a little, until his digestive organs were set to rights. I told him to regulate his diet, to act gently on his bowels, and to retire to rest early, and so on, and to call on me again at the end of ten days. When he came again, I found the ulceration had very much lessened, and that the sore was healing. I was convinced from this circumstance that it was not venereal and recommended him not to use mercury, but to go on with the plan I had proposed, and take sarsaparilla. That part of the regiment to which he belonged was afterwards ordered to Scotland, and not being quite well, he obtained leave of absence from the Army Medical Board for three weeks. The ulcer continued to heal, and just as he was going away, from some irregularities of conduct, it broke out again, and was as bad as before. He presented himself again at the Board, and was examined by the medical officers there, who said that it was a syphilitic ulcer, and that he must take mercury. He came and told me this; when I said, now although I am convinced that your disease is not syphilitic, your army surgeons will say so, and induce you to take mercury, when you may not have such a convenient opportunity of doing so as you now have. I will therefore put you under a mild course of mercury; such a course as shall be adequate to the removal of the disease, provided it be syphilitic. I ordered him to rub in every night, which he did for five or six weeks; the throat healed during this time, and he thought himself perfectly well. He joined the regiment at Edinburgh, and whilst there, the sore throat returned, followed soon after with nodes on his shins, and Mr. JOHN BELL had eloquence enough to persuade him that the disease was venereal, and that although he had taken mercury, yet that he had not taken

enough of it. He went through another mercurial course, longer than the first, and took sarsaparilla, when his throat again healed, and the other symptoms disappeared. He returned, in the course of the Summer, to London, and called upon me; he told me that he had been obliged to sell out of the army, that he thought he never should get well; his eyes were very much inflamed, and he was almost blind. He said he would submit to whatever I should tell him, for he was determined to get rid of his complaints or perish in the attempt. He took sarsaparilla freely through the day, he took five grains of blue pill every night for about three weeks, paid strict attention to his regimen, and got perfectly well.

It may appear strange that the administration of mercury should be attended with so many different effects, but the effects produced by it depend on the manner of using it. Small doses of this medicine do not seem to produce any constitutional effect, but only to act on the digestive organs, and in this way are productive of great benefit, as I showed you when speaking of the importance of paying attention to the state of the digestive organs in the management of local diseases. Mercury, when exhibited in larger doses, exerts an influence on the constitution in general, though its effect on the vascular system is not to be detected in the pulse, or in the state of the secretions; but it affects the nervous system in a peculiar manner, and by inducing a specific state of the constitution, counteracting that morbid irritation which before existed as the effect of the morbific poison. It is on this account that mercury relieves many other disorders besides syphilis. The quantity of mercury necessary to do this, I have before said, must vary in different individuals, but, by attention, it may be ascertained. Such a state of mercurial excitement which I should call the effect of the highest alterative course, and such as is necessary to produce this impression on the nervous system, may be kept up without inducing debility; on the contrary, I have known patients become stronger, and increase in bulk under its use, because it controls the irritation dependant on the disease. But if you carry the mercury further than this, you produce a decided increase in the heart's action; the secretions become increased, the gums get spongy, the patient is continually slobbering and spitting, the constitution becomes irritable and weak, it produces emaciation, and you have considerable disorder of particular organs. Such a mer-

curialization of the system appears to cure completely and radically syphilitic diseases, whilst others which had been checked and cured by a slight touch of the mercurial action, often increase, and even break out again under that degree of the mercurial influence which I have just described.

In the treatment of pseudo-syphilitic diseases, it is the second degree of the mercurial action on the system which is alone to be produced, therefore you must give the medicine in small and gradually increased doses until you have done this, but go no further. I said that people frequently got stronger under its action, and I have seen many hundreds of instances where this has been the case. I am in the habit of asking patients, "Do you feel yourself getting stronger under the plan which you are now pursuing? If they say, yes, I desire them to continue that plan until the local disease is quite removed. If, on the contrary, they say they are weaker, I desire them to discontinue the use of mercury. I can relate a case to you in which such a course of mercury was attended with a most wonderful result. A young gentleman from the country, the heir to some large estates, had a bubo and excoriation on the penis, followed by ulceration of his throat, which were considered by the surgeons who attended him to be syphilitic, and as they were very anxious to secure him from any ill consequences of the disease, mercury was administered very freely, but the symptoms did not yield. So much mercury had been used as induced his medical attendants to think that the disease could not be venereal. He came to London and consulted me, and on asking whether he had taken mercury, it was answered that he had taken mercury enough to cure twenty persons. His throat, they informed me, had frequently got better and again worse during the continuance, and after the mercury had been left off. I said that I considered the complaint to be pseudo-syphilitic, and that I had generally found such complaints to yield to alterative doses of mercury, and the decoction of sarsaparilla. This was about a year and a half from the commencement of the disease. Whether they did not carry into effect the directions I gave them, or from any other cause, of which I am ignorant, I do not know; but he returned to town in the following autumn, and when he came to me he was pale and emaciated to an extreme, with great debility, in fact, he looked like a poor chlorotic girl, so emaciated and feeble was his appearance. He snuffled so

much when he spoke, that it was difficult to understand him; he said, however, that he was very ill, that he had considerable pain in his nose, that when he blew it, mucus and pus, sometimes mixed with blood, came away. He said that he was worried excessively by the contradictory opinions which had been given, and that he was determined to remain in town and abide by my opinion, let the consequences be what they might. I told him that he should drink a pint of sarsaparilla every day, "Zarzaparilla! Zounds, I ave drank gallonz of it." Well, you must take sarsaparilla, although you may have drunk hogsheads of it. You must take also five grains of blue pill every night. "Blue pill again!" He growled out something about blue pill, which I could not understand, and was going away. Well, Sir, you are to do these things, regulate your diet, drink half a pint of sarsaparilla four hours after dinner, and another half pint in the evening, and if you like to drink more of it, you may. These directions will suffice you for a week, at the end of which time I shall expect to see you. He came at the time appointed; "Well, Sir, are you better?" "No, nothing better." "Are you worse?" "No, I don't know that I am worse." "Very well, you are to go on another week in the same way." He came again, "Are you better, Sir? "No, nothing better." "Are you worse?" "No, I don't know that I am worse." Finding the mercurial influence was not in that dose sufficient to control the disease, I ordered him to take for the third week five grains of the blue pill night and morning. When he called on me the third week, he said he was quite well, and spoke as distinctly as I could, but that he had a little discharge from the nose when he blew it forcibly. He had increased in strength very much since he had been taking the mercury, but it had not in the least affected his gums. I ordered him the next week to take only one pill at night, and to observe whether it increased or diminished during that time. The discharge from the nose had then completely ceased, and in three weeks more he had become so stout and fat that every body was astonished to see such a change effected within such a short time. I am certain that he increased as much as fourteen pounds in weight every week. He stayed a short time in town afterwards, long enough, however, to get a gonorrhœa, which prevented his going into the country for a time. He got completely well, and was able to eat, after he went into the country, a pound of beef steaks at a lunch.

Other preparations of mercury have been found useful, and the oxymuriate, or chlorate of mercury is frequently given as an alterative, combined with the sarsaparilla; but I generally prefer the blue pill; I think it is the mildest, and therefore the best preparation of mercury. I hope I have said enough to show you, that mercury must be given in the cure of all pseudo syphilitic diseases, to a sufficient extent to produce a gradual impression on the nervous system, and that this is to be accomplished by giving it in that way which I have named, the *highest alterative course*, but short of that which would disturb and irritate the system, by giving it to that extent which is usually necessary to cure true syphilis, and this is the proposition with which we set out.

GONORRHŒA.

I have next to speak of a disease that was formerly considered to be only a modification of syphilis. This was an opinion which prevailed until Mr. HUNTER's time, and it was considered by the old surgeons to be necessary to give mercury for its cure. But mercury only aggravates it. It is undoubtedly a distinct affection; it is a mere local disease, not followed by any constitutional symptoms. Nay, it is decided to be a separate disease from the old lues, or venereal disease, by Act of Parliament; for, in the days of the good Queen Bess, an act was passed for the regulation of certain brothels, which were licensed by the government; and it was "further enacted, for the benefit of her Majesty's *loving* subjects," that certain classes of females in these houses should be kept separate from others, lest her Majesty's *loving* subjects should burn their ———.

What is the gonorrhœa? I dare say most of you know that it is an inflammation of the membrane lining the urinary passage; it is attended with great pain in making water, aye, violent and excruciating pain, and a profuse purulent discharge follows. It is, when severe, attended with violent and painful erections, and oftentimes with great constitutional disturbance. This is what the old clap used to be; but, upon my life, it is as rare to meet with a disease like the old gonorrhœa, as it is to meet with a sore resembling the old chancre. It appears that the disease is become very much milder in the present times, but why it should be I do not know; but people come to me and say, "I have such a confounded running, what shall

I do with it?" I tell them to reflect a little; that the running is not the disease, it is only the symptom of the real disease, which is an inflammation of the membrane of the urethra; that they must therefore direct their attention to that, and never mind the running.

TREATMENT OF GONORRHŒA.

When I meet with a case of gonorrhœa, I advise the parts to be sponged with a little tepid water, and to apply a warm sponge to the perinæum, or to use a hip bath; to cleanse away all the secretions from the part, and not allow any accumulation to take place, which might become a source of irritation. This is the first thing then, to cleanse and to soothe the parts. The prepuce, or fore skin, should also be drawn forwards, so as to protect the mouth of the urethra from the irritation of any foreign body. The patient should rest a few days, adopt a spare diet, and take a little gentle aperient medicine, not such as would irritate and disturb the bowels, for if he did, the complaint would be made worse rather than better. He should drink plentifully of some mild tepid drinks, as toast and water; or some mucilaginous drink, as linseed tea, and so on. By doing so, the urine becomes very much diluted, does not prove so irritating to the susceptible surface of the urethra, over which it must pass, consequently the *scalding* will be very much lessened. As to the running, when you have removed the inflammation and set the digestive organs right, it will cure of itself. But some persons are very anxious to have the running stopt, but it is not what I should advise; it is the relief of the disease, and it will prove its cure. I am satisfied that many serious diseases are caused by the methods used, from an undue anxiety to stop the discharge. Injections, bougies, and so on, often prove sources of very great mischief, therefore it is paying for the removal of the running at too dear a rate. I should rather advise the attention to diet; the occasional use of some mild aperient medicine, so as to set the digestive organs to rights; keep them so, and the disease will soon disappear. There is no necessity to keep a man upon a very low and spare diet for a long time, for by doing so you weaken the patient too much, and the discharge will continue. I have often known persons who had been very solicitous about the removal of the complaint, and who, therefore, after the vio-

lence of the inflammation had been removed, kept themselves upon a very spare diet, go into a party, and there, forgetting their claps, drink more wine than they intended. They have come to me the following day, and, with considerable consternation, informed me that the discharge had quite ceased. It is not necessary to keep a person so low and feeble, but let him have a regular nutritious diet, and such as will not disorder the digestive organs which you have been trying to relieve.

There are various remedies which have been employed for the purpose of removing the chronic kind of discharge which, from certain irregularities of conduct, has been kept up. *Cubebs* have been strongly recommended, the balsam of *copaiba*, and injections; but I cannot say that I like them. I believe there is no doubt that, if used carefully, they will succeed; but the way in which they are generally used is highly objectionable, and frequently lays the foundation for far more serious diseases.

LECTURE XXII.

THE CONSEQUENCES OF GONORRHŒA.

The discharge of gonorrhœa will sometimes leave the internal surface of the urethra and attack the parts about the corona glandis, and it is then called the gonorrhœa of the prepuce. It is a circumstance worthy of notice, since it serves to show the sympathy of continuous surfaces, and prepares the mind to believe, which is indeed forced upon it by the occurrence of cases, that sores will break out upon the prepuce without their being contaminating sores—sores which break out at a considerable period after any impure connexion, from the irritation of the prepuce which the gonorrhœa may occasion. It is not at all unfrequent to see such sores, but they ought not to be treated as syphilitic, because they are not curable by mercury; but the treatment of them I shall afterwards have to refer to.

SWELLED TESTICLE.

If I were to mention the concomitants, or consequences of gonorrhœa, in the order of their frequency, I should say, that a swelled testicle was the most frequent. It rarely occurs, however, while the clap is in full force, but when the disease is going off; and they say that it is owing to the stopping of the running of the clap, and that the running being again established the disease gets well. There are many urgent reasons against stopping the discharge of a clap, and this should be one of them; indeed, DESSAULT, who lived almost all his lifetime at the Hôtel Dieu, at Paris, and perhaps saw more of this disease than any other man living, (for he must have seen thousands of such cases there,) has an expression to this effect, "that it is sinful to stop the progress of the clap." This is a strong expression, certainly, but there is great truth in it. The irritation towards the orifice of the urethra sometimes suddenly subsides, and attacks the prostatic portion. A swelled testicle has often been called a sympathetic inflammation, but it is no such thing; it is not sympathetic, it is the result of a propagation of the inflammation by the *vas deferens* to the testicle. The inflammation which is set up in the prostatic portion of the urethra, communicates a like action to the orifices of the seminal ducts, which terminate there, and that inflammation is extended to the testicles. It seems to be curious, that this irritable state of the testicles endures a certain length of time and then subsides; it continues just as long as the inflammatory stage of the gonorrhœa, namely, about three weeks. There is generally some swelling of the testicle produced, but the degree of it very much depends upon the management of the case. It is of no use to fight against it, or go about with such a complaint; the patient must lay himself up for a time and take means to reduce the irritable state of the testicles; and although the testicle may become a little harder and the scrotum inflamed, yet if the patient keep himself in the horizontal position and soothe the testicle, I have known such cases to go off with very little swelling. The inflammation of the testicle may abate, yet if the patient go about before it is quite gone, it is very liable to return again, and be accompanied with shivering and great constitutional disturbance. It is of no use, I say, to fight against the disease, and cases are

liable to relapse within a certain period of time, that is to say, about three weeks. The discharge then again returns from the fore part of the passage. Sometimes it happens that both testicles become affected ; sometimes the inflammation appears to shift alternately from one to another.

TREATMENT OF SWELLED TESTICLE.

There is irritation seated in the back part of the urethra, and there is nothing better for the purpose of relieving it than tepid bathing of the perinæum and genitals, as by flannels wrung out of warm water, or the local tepid bath, and I consider there is nothing better than the bread and water poultice. The weight of the part should be taken off, by affording it a comfortable degree of support, and a proper bag truss is a most excellent thing ; but I can rarely see a good suspension bandage now-a-days, they are for introducing new fashions upon all occasions, and they have introduced new shaped trusses for this purpose. The bag of the truss ought to be attached to a circular belt by two straps, with buckles, so that they may be raised to a comfortable height, and no farther. There is generally some little febrile state induced, and then you give some James's powder, and take care to keep the bowels regular ; let the patient remain in an horizontal position and he will soon get well. It is of great importance to keep the horizontal position when any of the lower part of the body is inflamed, for example, the leg; you must have noticed the relief almost instantly afforded by laying the patient on his back. As to leeches, I say nothing of them ; I do not think that they do much good ; the disease appears to continue for a determinate time, and then goes away if you attend to the circumstances I have just named. This is all that I am inclined to do ; and I say, that although people get better, they should not go about under a certain time, for if they do the disease will return.

IRRITATION OF THE GLANDS IN THE GROIN.

There is no person who has a gonorrhœa, that has not more or less enlargement of some of the glands in the groin; therefore, in point of precedence, I ought perhaps to have named this first. It appears that the absorbent vessels are irritated by the inflammation which is going on in the mucous membrane of the urethra, and the glands to which these vessels

lead, become inflamed. When you soothe the inflammation in the urethra, the tumid glands generally subside very rapidly, provided the patient only observes rest and takes measures to soothe the parts. They do not require to be treated as venereal buboes, but the treatment employed to cure the gonorrhœa will generally remove them. You may apply tepid water to the part; linen dipped in tepid water, and allowed to evaporate. I repeat, that the means adopted for the cure of the one will remove the other.

IRRITABLE BLADDER.

As another concomitant, or consequence of a gonorrhœa, I may mention an irritable bladder. The patient makes water very frequently, and has an unconquerable inclination to do so; the patient has, perhaps, to make water three or four times in an hour, and only a few drops may escape, when the irritation is violent, yet he feels as if his bladder were quite full; there is, at the same time, a great irritation felt at the extremity of the urethra, and the patient often pinches the part as if there was something to squeeze out. When I was young, patients used to take castor oil and mucilaginous drinks and things of that kind, and without much benefit; they used to remain a considerable time suffering from the disease; but had I known then as much as I know about it now, I am sure, such cases would have been cut short by bathing the perinæum with tepid water and applying leeches there; by keeping the bowels regularly open, by mild aperients, and regulating the diet. This is the treatment that should be adopted in such cases, and the complaint subsides in a short time. There are sometimes knobs which form round the urethra, about the size of horse beans, and sometimes they suppurate; they form in those parts of the urethra in which there is the most mucous secretion—as behind the frænum glandis, just in front of the bulb of the urethra, and in the perinæum behind the bulb, just where Cowper's glands are situated. I have seen such swellings increase to the size of a pigeon's egg and gradually subside again. Sometimes they burst into the urethra only, or burst outwards only, and at other times they burst both inward and outward, establishing a communication with the external parts and the urethra, forming what has been called a *fistula in perinæo*. When I was a young man, if the surgeons

felt any fixed hardness in these parts of the urethra, they used to prescribe mercury; but there is no necessity for this, indeed it is wrong. It is not often that the communication is established so as to form a fistula, even although the swelling should be laid open. All I have to tell you is, that if you poultice them and treat them kindly they will do very well.

CHORDEE.

The inflammation from the membranous lining of the urethra is sometimes propagated to the corpora spongiosa, agglutinating their cells together and producing a kind of *chordee*. This occurs sometimes in the early stages of a gonorrhœa, and produces great pain. It is the distention of the irritable urethra, by a powerful and violent erection of the penis. It occurs often at night when the patient is warm in bed, and is best relieved by tepid applications to the part and an opiate at bedtime. But this chordee, of which I am at present speaking, is rather the result of the acute stage of the gonorrhœa; there is a thickening and agglutination of some of the cells of the corpora cavernosa, so that it is imperfectly distended and the erection is imperfect, the penis bent downwards and attended with great pain. This state is best removed by applying a tepid bread and water poultice to the parts, by avoiding all causes of irritation, by keeping the bowels regular, and by attention to the diet, so that the digestive organs may be set to rights. A man should not take horse exercise, as that would increase the irritation of the parts. Some small doses of aperients, as a few grains of calomel or blue pill, with a little rhubarb and jalap, and a little castor oil, are the best aperients; the bowels must be kept open, but by no means irritate the intestines by teasing them with coarse cathartics.

STRICTURES.

Gonorrhœas are very liable, if they be improperly treated, to lay the foundation for strictures. I have spoken of the inflammation shifting from the urethra to the external part, producing a discharge from beneath the glans; I have said that it sometimes leaves the fore part of the urethra, and attacks the back part of it, or that portion inclosed by the prostate gland, and therefore called the prostatic portion of the urethra. It

may leave that part and again attack the extremity; but the inflammation sometimes establishes itself there and causes a thickening of the parietes, and consequently a diminution of the calibre of the canal. Indeed the whole of the urethra is so contractile and irritable, that strictures are likely to occur in any part of it, as in all other mucous tubes surrounded by a muscular structure, as in the œsophagus, the biliary ducts or the intestines, especially the rectum. So irritable and powerful is the contraction of the urethra, that, egad! persons can hardly hold their water a minute, who have an irritable urethra, or when stricture begins to form. You have a proof of the resisting or contractile power of the urethra when irritated, if you attempt to pass a bougie under these circumstances. You must have seen this happen, that if you attempt to introduce a bougie when the urethra is irritable, that it will be grasped firmly, held so tight, that it cannot be pushed on to the bladder, but be twisted and indented in various places.

It very often happens that there is a sort of spasmodic action of the urethra excited, and a temporary retention of urine is the consequence. If this should happen some little time after a man has had a clap, he goes in a great fright to a surgeon, and he proceeds to pass a bougie, but he finds that he can get but a little way into the urethra, and when he takes it out he sees that it is very much contorted, and has several depressions on its surface. He informs the patient that he has a stricture, and a very bad one too; intimates to him that he must have neglected himself very much, and so on. The irritation subsides spontaneously, and he makes water as well as before. There could not have been a stricture, because the patient passed the urine very well a short time before, and a stricture could not have been formed so suddenly as all that. Sir Everard Home has observed, that such a state of the urethra may arise from the application of a blister, and he has given a case where a bougie was attempted to be passed, and the result was similar to that which I have described to you. It has often happened, that patients have gone to some surgeon in the country, and he has done what I tell you. The patient is convinced that he must have a stricture, and a bougie is passed, perhaps every day, until the urethra is made excessively irritable indeed. They sometimes come to London for further advice, and how they stumble upon me I cannot tell; certain it is that they do so. A man comes to me and says with a very long face, "I

am very bad, Sir; very bad indeed." "What is the matter?" "Oh, Sir, I have got such a stricture as I fear I shall never get rid of: I can only make water in such a small stream, and whenever I feel the least inclination to evacuate the bladder, I must do so directly," and so on. Well then, I hear the history of the case, and I find out that the bougie has been passed much too often, and that the evil is owing to an irritable state of the urethra. I do not, under these circumstances, set about passing a bougie, but I tell them to bathe the perinæum with tepid water, to take some medicine to open their bowels, to keep them regular, to attend to their diet, to take no spirits, and so on; and to keep themselves quiet, and call upon me again at the end of a week. The truth is, that many such persons never call on me again; they find that they are so much better, that they can make water so well, that they put off the evil day, as they think; they have suffered so much from having a bougie passed that they are afraid of it. Well, those who do return, come saying that they are very much better, but they should like to have a bougie passed. Well, I say, to satisfy you I will pass a bougie: now what size would you like me to pass? and I show them a bundle and let them take their choice. They will be sure to choose one of the smallest, and say, "Mr. —— could only pass one of that size, Sir." If I take up one twice the size, and ask them if I shall try that: "Oh, Lord, Sir, you frighten me; you could not, I am sure." "We will try, if you please:" and after dipping the end of it in a little oil, I introduce it very gently; it goes into the bladder without any difficulty, and they are perfectly astonished.

If this irritation become established in any one part of the urethra, it lays the foundation for a stricture. The irritability being established, a kind of inflammatory action attends it, and there is a thickening of the membrane of the urethra, causing a diminution of the calibre of the canal, and producing a *permanent stricture*. As Sir Everard Home has told you, you may take a soft wax bougie, and passing it on to the stricture you may, by continuing the pressure a little, take exactly the model of the stricture. The contraction is more upon the lower than the upper part of the urethra; the strictures are indeed half moons rather than circles: you should, therefore, when you are passing a bougie, always conduct the point of it along the upper part of the urethra; it is a point of some importance to be attended to, especially when you reach that part of the

urethra which passes between the arch of the pubes. I know of no book that contains such good directions for passing a bougie as that written by Mr. Whateley. Every young man ought to accustom himself to the passing of bougies; for I can assure you that there is a great reputation either won or lost, according to the degree of skill which a man possesses in this respect. Mr. Whateley has told you, that you should nip up the end of the bougie a little between your finger and thumb, and having thus rounded its extremity, take it as you would a pen in your hand, and carry it on gently, keeping the extremity of it against the upper surface of the urethra: when you reach that part of the urethra which passes beneath the pubes, depress the handle of the bougie, by which you elevate its point, and it will find its way into the bladder without difficulty.

I can never impress upon your memories a thing half so well in any other way as by the recital of cases: A surgeon called upon me from the country, to request me to accompany him to a patient who had very bad strictures, and for whom he could not pass a bougie; yet he said that the patient could make water in a stream, so that the stricture could not have been very bad, you know; however, he became very much alarmed because he could not pass a bougie and wished me to go with him. I learned from him what sized bougie had been last passed, and I took up one about the same size, and without letting him see, just nipped up the end of it a little, and by attending to the rule which I have told you, keeping the point against the upper surface of the urethra, I passed it into the bladder without any difficulty; I could carry the handle of the bougie close down upon the pubes, and that was a good proof that the other end had gone into the bladder. The patient and surgeon were quite surprised, and I believe the patient thought me a very clever fellow, when, in truth, there was not much credit due to me for passing it; well, I did not wish to make a secret of the mode of passing it, so I told the surgeon how it was to be done. I remember another case, of a very queer little man, from the country, who had had stricture for some considerable time, and he came to me, complaining of the very distressing state in which he was left by the stricture; that he could only pass such a very small bougie, and that there was great difficulty even in passing that. I understood from him that his surgeon had been passing a bougie every second day, and that his urethra was in a very irritable state, so I told

him that I should not pass a bougie then, but that he should bathe his perinæum with tepid water, apply some leeches to the part, and come to me again at the end of the week. He came, and I passed a bougie nearly twice the size which had been before passed, without any difficulty. The man could hardly believe that it was done, and the expression he made use of was —" Bless my soul, Sir, I am sure you must have had a pick-lock key at the end of it."

Now, the opening of a stricture may lie to one side of the urethra or the other. In passing a bougie you have the point sometimes turned upwards, or to one side or the other; and what is the inference, but that the stricture lies in the opposite direction to that from which the bougie is directed. From the misapplication of remedies to cure a gonorrhœa, or from the too frequent introduction of bougies, to cure either imaginary or real strictures, an irritable, lurking disease is established, which gets worse by degrees; the urethra becomes very much contracted, until the stricture is very bad indeed.

TREATMENT OF STRICTURES.

The treatment of strictures may be divided into two parts, Medical and Surgical, and I know that the medical treatment is of great importance. The patient should use the tepid hip bath, apply leeches to the perinæum, sponge it often with tepid water; he should also use some mild aperient medicine, not such as would irritate the lower bowels. Set his stomach and bowels right by a proper regulation of diet; for while there is any thing wrong about the stomach and bowels, the urethra will never get right. You next proceed to examine the urethra, and for this purpose you should use a soft bougie of a small size, and having done what I before recommended, introduce it very gently, and carry it on till you come to the stricture; keep the point of the bougie directed towards the upper part of the urethra, and if you find you cannot pass it on in that direction, turn it a little to one side, and then to another, and you may generally get it on. But if you take a large bougie, and pass it roughly, you will lay the patient up with a retention of urine. Pass it in the most careful way that you may, you will be sure to excite some irritation; and never attempt to pass another until you have again soothed the urethra. Indeed, I have made it a rule never to do so; I never

will pass a bougie for a patient until I have soothed the urethra by tepid bathing, and so on. I have lived long enough in town to know the result of my own practice in this respect, and to contrast it with that of others, and I know that this is the best practice. The reason why strictures get so bad as they sometimes do, is the following:—A man who has an irritable urethra or a slight stricture, applies to a surgeon, he passes a bougie roughly, and brings on retention of urine. Means are taken to soothe the parts, and allay the irritation; he gets better for a time, can make water better than he did before; but the complaint returns, and the patient has such a horror of a bougie, that he keeps himself away from the surgeon until the disease gets very bad indeed. I say, that if a bougie be passed ever so gently, and withdrawn as gently, it will produce an increase of the irritation for a time.

The principles upon which bougies should be employed are like those which I mentioned, ought to guide us in the treatment of susceptible surfaces. Now I would put as a parallel case the action of a chemical stimulus. What do we do when we have an inflamed eye to treat? First, subdue the inflammatory action, and then wash the eye with an eye water. The first application may be horribly painful, but after a time the effect of the stimulus will subside, and the eye feels much more comfortable than before. You desire him to apply the stimulus again and again, until at last he thinks no more of using the wash than if it were common water. Now a bougie is a mechanical stimulus, it gives some pain at the time it is first used, but the urethra is less morbidly sensible afterwards. When you first pass a bougie for a patient, Oh! he will cringe about and complain very much indeed; but after a time he thinks no more of having a bougie passed than of having his hair combed. You are not to repeat the stimulus too much; if you do, you will cause an increased irritation. As much harm as good is often done by bougies being passed too frequently. By introducing bougies too often, you have not only to counteract the irritation which previously existed, but the irritation which you have yourself created. I set it down as a rule to pass a bougie no more than *once a week*. It produces perfect astonishment in the minds of some persons to find the stricture subside under the use of such apparently trivial measures. If the irritation appears to be increased by the bougie you must not introduce it again until that irritation shall have

subsided, and you must be guided by this entirely. I grant you that the stimulus of the bougie will increase the irritation at first, but if you treat the case in the manner I have pointed out, you will find that it will have a different effect; it is used with a view of diminishing the morbid irritability. They talk of dilatation, that bougies are used to cure strictures by dilatation; as if they could stretch such sensible stuff as the urethra, as if it were a piece of leather. Now, I say, let a stricture get well without stretching, and that it will do so is proved most pointedly, by the enlargement of a stricture round the varnished catheter. This is what they do with strictures in France, they introduce a varnished catheter, and the urethra enlarges around it. I will tell you an instance of this enlargement of the urethra.

I saw a man who was making water through six or seven openings in the perinæum. They were fistulæ, and he was very bad indeed. I had brought with me some of these varnished catheters from Paris, and they really were wonderful things, as small as the smallest size bougie, yet perfect tubes. With considerable difficulty I succeeded in getting one of these tubes into the bladder, and the stream of urine that passed through it was as fine as any hair; indeed it could hardly be seen, yet the fluid accumulated in the vessel into which it was conducted. He never made water through it again, for it became plugged up by some means, but he made water by the sides of it. It was left in the urethra for about a week, and one twice its size was introduced. At the end of another week, a catheter twice the size of the last was introduced, and so we continued to increase them in size, until the largest size could be introduced, and the sinus got perfectly well. It is not therefore the stretching of the stricture, but the giving way of the stricture about the tube. I know that here it may be said, that the pressure of the instrument promotes absorption, but I do not believe that it does so. This is the French method of treating stricture, but it is not a permanent cure for stricture.

IRRITATION OF THE URETHRA.

Dysury, or irritation of the urethra, depends sometimes on a constitutional malady, and, in such cases, passing bougies will do very little good; you must remove the constitutional derangement by attending to the state of the digestive organs.

Strictures are so very irritable, and grasp the bougies in such a manner, that when you attempt to force them on, they become reflected and twisted about, and if you persist, you must do a great deal of mischief. In such cases the passing of metallic sounds is better than the bougies. If a sound be introduced gently, it produces all the good of the bougie; the canal enlarges until the urethra becomes of its ordinary calibre. But you will find that it is not an absolute cure for the disease. I have known patients who have gone on from No. 1 to No. 20, and yet a little fit of illness has produced a contraction again. However, this is what they call the cure by dilatation: I am sure that the cure does not depend upon the frequency of the introduction of the bougies. It was the custom to direct patients to keep in bougies an hour a day, and many old men have I seen who have had a diseased state of the urethra thus kept up for a long time, and which has subsided when this practice was set aside. One of the most striking cases of this kind occurred in an old nobleman, who had very bad strictures; he had bougies introduced too frequently, by which the disease was kept up, and the urethra irritated so much that at last he could only make water in drops. His medical attendants said that he must apply to Sir Everard Home, and get the caustic bougie passed: he did not like the thought of a caustic bougie, and he determined to consult some more eminent surgeon; how he stumbled upon me I do not know. However, I discovered that his urethra was in a very irritable state, and told him that I should advise him to bathe the parts frequently with tepid water, and take some mild aperients, to regulate his diet and set his digestive organs to rights; to allow a little time for the irritation of the canal to subside, and that if he would do me the favour of waiting on me again in a week, or if I waited on him, I would then pass a bougie. He did not appear to think much of it, and said, "he thought it was a very nugatory practice." And will your Lordship permit me to say, I replied, that you are no judge of it. He was so much better at the end of a week, that I was able to pass a larger bougie than could have been passed for a long time before. I told him to continue the plan I had laid down for him for another week, and that then I would pass a bougie again. Why, said he, my surgeon passed a bougie three times a week, and again observed that it was a "nugatory practice." To which I made the same reply as at first. The state of the

canal was so much improved at the end of *three* weeks, that I could pass a larger bougie than had been passed for fifty years. It was not a large bougie certainly, but it was as large as the largest crow quill, and he could empty his bladder with great facility. This is the practice which I should recommend: first soothe and allay the irritability of the urethra, and then adopt the use of bougies, upon the principle of treating a morbidly sensible surface, as I have before explained to you, and which I have called the Surgery of susceptible Surfaces.

LECTURE XXIII.

In the last lecture I spoke of the medical treatment of strictures; I pointed out the importance of soothing and allaying the irritability of the part, and of attending to the state of the alimentary organs, with the regulation of diet. I also spoke of the surgical treatment of strictures, in as much as they depended upon an irritable state of the urethra, with the manner of passing bougies, and the principle which should guide us in our practice, viz., that of allaying the morbid sensibility of the part, by the application of a mechanical stimulus, short of that which would produce re-action.

It sometimes happens that strictures become very intractable; they do not subside upon the introduction of bougies, and they appear to increase and to become so much thickened, and the structure of the part so much altered, that it becomes necessary to apply some chemical stimulus. Mr. Hunter tried what could be done by escharotics for the removal of stricture, and he used red precipitate, but it did not answer; it produced great inflammation in the sides of the urethra, and did not destroy the stricture. He next tried what could be done by passing a canula down to the strictured part, and introducing through the canula a port-crayon, having a small bit of caustic inclosed in its extremity; but this did not answer very well, except in the straight part of the urethra. Mr. Hunter acknowledged that it was difficult to get at some strictures with the canula, and he afterwards devised a better mode of

using the caustic, namely, putting it into a bougie, and carrying the bougie down to the stricture: this method, however, was more extensively introduced into practice by Sir EVERARD HOME. Caustic bougies have been used for the purpose of applying this chemical stimulus to the stricture; and Sir E. HOME has written a description of the mode of using a bougie, having a small quantity of the argentum nitratum, or the lunar caustic, stuck into its extremity. The urethra is first dilated by a large sized bougie being passed down to the strictured part, and then the other bougie, or armed bougie, is introduced, and held firmly against the stricture for a few seconds. The part which the caustic has touched loses its vitality, and then it is of no use to make any farther application until it separates; for it is the living surface only that can be benefited by treatment. That slough separates, and then he recommended the caustic to be applied again. But this is not the way in which I should be disposed to treat stricture: it not only lessens the morbid action, but it produces a destruction of the vitality of the part, and that is rather more than is wanted. I had an opportunity of observing the effects of this argentum nitratum, *par hazard*, on my own person; for, while I was scraping a pencil of it to put into the end of a bougie, some bits flew off and got into my eye; I dipped my eye immediately into a basin of water, and washed it out; but I had three spots on my eye. The cornea fortunately escaped; but, if I may be allowed to say it, I had three black specks on the white of my eye: they were on the conjunctival covering of the sclerotic coat, and they had rather a curious appearance; they became surrounded by a margin of inflammation; but the conjunctiva did not become greatly inflamed, and in a few days the black spots desquamated. We see that the practice of touching ulcers, which are morbidly irritable, with the caustic, produces very good effects; healthy actions are excited, and they get well; but it was found that the practice did not succeed in curing stricture, and this induced Mr. WHATELEY to try another mode of applying the caustic: he powdered the caustic, dipped the end of a small bougie in mucilage of gum, and then in the powdered caustic; having passed in a large sized bougie first, he next introduced the smaller one, and carried it into the stricture, so as to apply it to the whole circumference of the stricture: he succeeded, in this way, in curing a stricture which had been given up by many other surgeons, and, strange to say, he published a book

on this mode of treating strictures, with only this single fact to support his opinion.

These are very sharp medicines; they succeeded sometimes, and were consequently eulogized in the highest manner. The result of such rhapsodical praises was, that the practice fell into utter disrepute; so that when the caustic bougies were talked of, people said, Oh, it is absurd, it is nonsense to think of using them; and many useful medicines are from this very cause quite disregarded. The argentum nitratum fell into disuse, and Mr. Whateley, in his publication on strictures, has endeavoured to show that there is a strong disposition in the strictured part still further to contract, and that the diseased portion of membrane lining the canal requires a stimulus to do away with the morbid susceptibility, but short of destroying the vitality of the part. He introduced the use of the caustic potash for this purpose, as being a much milder and as effectual a remedy as the argentum nitratum, having the superiority over that escharotic on this account, that it does not cause the part to slough, but excites that degree of action in the part which destroys the morbid susceptibility. This potassa, or potassa fusa, is broken into very small pieces, and a small speck of it, not larger than a pin's head, is to be used at a time. A little hole should be made in the end of a small bougie, and this speck of caustic be put into it, and it is to be held in its place by a bit of hog's-lard stuck round it. Having ascertained the precise part of the urethra in which the stricture is situated, you must first pass down a full sized bougie to it, the stretcher, as Sir Everard Home calls it; when that is withdrawn, you introduce the caustic bougie, and make gentle pressure with it on the stricture; let it rest there for a few seconds, and then press it on gently from the front to the rear, until it goes through the stricture, and then immediately withdraw it. The principles which Mr. Whateley has inculcated in his book appear to me very correct, and such as cannot fail to be useful. He allows the irritability which the bougie may have occasioned to subside before he again introduces it, and this is the only proper way of curing strictures; in about a week he introduces the caustic again, and uses rather a larger bougie than at first, still using the same small quantity of caustic as before, and when this irritation has subsided, perhaps at the end of another week, he introduces another bougie. The practice has proved very successful in Mr. Whateley's hands, and with

others, and I deem it a very rational method of curing those strictures that you cannot remove by milder measures. Recollect that I am now speaking of strictures in which the use of caustic is supposed to be necessary. You have tried the mild measures; you have tried to introduce the smallest size bougie, and you have failed; the stricture has proved very stubborn, as it will sometimes do, and you are obliged to resort to this mode of treating it. No surgeon would think of using such severe measures until he had tried the milder ones. You should read Mr. WHATELEY's book on this subject, for the rules and precise directions which he has given I consider very useful. I remember a remark made in one of the flippant reviews of the day, upon Mr. WHATELEY's book; it was written by some inexperienced young man, I am sure; some one very clever in the use of his pen, and you know there are such young men; the reviewer's remark was this, after having spoken of the argentum nitratum as a caustic, and so on, "but it was left for Mr. WHATELEY to discover a method for the cure of strictures, and for the manufacture of *soap*." The truth is, that the potash does combine with the lard and the mucus of the urethra and form a kind of soap, which modifies the action of the caustic, and has a very good effect.

I have known a great number of cases succeed very well when the caustic has been used. I have used the argentum nitratum and found it to answer very well, but, upon the whole, I prefer the kali, I think it is a safer thing to use. This is the way in which we manage strictures when we cannot succeed by any other; but the French say the English surgeons treat strictures very badly, indeed they have gone so far as to say that we do not know how to treat them at all. They are in the habit of using flexible varnished catheters, and allowing the urethra to dilate about them, which it often will do, in a surprising manner. A gentleman told me that he called when in Paris, on a French surgeon, to examine his stricture, which had continued for a long time, and was very bad; he asked him what he should do, and if he would pass a bougie, but all he could get in reply was, a long tirade against the English surgeons. I do not advise you blindly to adopt any particular treatment, but when you go into practice judge for yourselves which method is the most likely to be successful. The treatment of diseases of the urethra, like every other part, should be considered scientifically, and the same surgical principles

which have influenced you in the treatment of other parts of the body should guide you here. You must, by the application of a stimulant, try to remove the morbid susceptibility of the part. Bougies are mechanical stimulants, but if these fail, you must have recourse to a chemical stimulant.

The *ne plus ultra* of surgery, as regards stricture is this, that you should pass a full sized bougie through the urethra with little hindrance or molestation, without any impediment from the remains of the stricture, or without the molestation of again rendering the canal irritable. This is the *ne plus ultra*, I say, but, egad! you cannot reach this in all cases; you are very glad to be content with passing a moderate size bougie with little hindrance or molestation; if the patient can pass his water in a moderate stream, that is as much as you can expect to do in very obstinate cases. Well, I do not know that I have any thing more to say to you on the subject of strictures, and I shall next speak of

DISEASES OF THAT PART OF THE URETHRA ENCLOSED BY THE PROSTATE GLAND.

Now, there are certain parts of the urethra more disposed to contract than others, but it appears that there is one part of it not susceptible of contraction, it is that part enclosed by the prostate gland; the urethra adheres to the gland, and cannot contract like the other parts of the canal, but it often becomes the seat of irritation; the lining membrane becomes thickened, and prevents the free discharge of the urine. I shall be able to present you with a piece of information, which, to say the truth, I stumbled upon; it was not the result of intelligence or reasoning, but I say I stumbled upon it, and it was in the following way: Cases were very often occurring in my private practice which I did not at all understand; persons who had strictures could pass large bougies, and would yet complain of difficulty in emptying the bladder; I could not at all make out the cause of it; I used to think that there must be a stricture at the neck of the bladder; however, the following case gave me a great insight as to the cause of the difficulty which I had before experienced. A man came to me for advice under the following circumstances; he said that he had a stricture, that he had been under the care of many surgeons, and that they had successively passed bougies until they could

now pass a full sized one, yet he could not make water but in drops, or in a very small stream. I thought there must be considerable mischief going on somewhere, and I proposed that he should take the opinion of some other surgeon, and we would attend to the case jointly; he said, "No, I have come for your opinion, and I shall abide by it; if you succeed, well; but if you do not I shall never blame you." There appeared to be an honest frankness in the man, and I said, that since that was his opinion, I should advise him to bathe the perinæum, to apply a few leeches there, to take some opening medicine, and come to me in four days, and that I would then pass a bougie for him. He came, and I passed a bougie, it went on very well until it reached that part of the urethra which passes through the gland, and then it produced very great pain indeed, a sense of heat, and a vehement desire to make water. "Oh! what are you doing?" said he, "Are you withdrawing the bougie?" at the time I was carrying it onwards. The sensations produced are so confused as to make patients sometimes suppose that they have been making water; there is great pain, with a feeling of heat, and urgent desire to make water. The passing of the bougie for this patient was attended with great relief, inasmuch as he afterwards voided his urine more freely than before. The second time the bougie was passed, which was done after a few days, it gave much less pain, and he said so each succeeding time that it was passed; the uneasy sensations in the part subsided, and he got quite well.

Irritation existing in the prostate gland may operate in two ways, it may tend to keep up the irritation in the bladder, and especially about its neck, and cause dysury, even when no mechanical impediment exists to the discharge of the urine, as I think the case I have just mentioned shows. But it may also produce an irritation in other parts of the canal, so as to give rise to the formation of strictures. A gentleman, more than seventy years of age, had experienced great difficulty in voiding his urine for some time, which gradually increased until the stream became small, attended with a frequent propensity to empty the bladder. It happened, that when in the country, a complete retention took place, and a surgeon attempted to introduce a catheter, but could not get it any farther than six inches. The gentleman ordered his carriage and immediately came to London, and called upon me. I directed him to bathe the perinæum with warm water frequently,

and to take some castor oil to clear out his bowels ; after some time the urine flowed, and he was relieved from the urgency of the symptoms. After he had continued to soothe the parts by warm bathing and so on, I examined the urethra, and found a stricture at six inches from the orifice, through which I could not pass even a small bougie. I touched this with the argentum nitratum, for it was then in vogue, and in three days, I passed a bougie on to a second stricture, about seven inches from the orifice, which was also touched with the caustic. I afterwards passed the bougie on to the prostate, but as soon as it entered that part of the canal I was immediately obliged to withdraw it, from the sudden and violent pain which it occasioned. I told the patient that it was a great point to introduce an instrument into the bladder, but from the small size of the stream of urine, I was afraid it would be difficult to do so. But that it was a thing of so much importance in the management of his case, that I wished that he would allow some other surgeon to be called in, (any person of whom he might have a good opinion,) and that we would together try what could be done. Another surgeon was called, and it was agreed that we should try to get a tube of some kind or other into the bladder. We tried the flexible catheter, but could get it no farther than the prostate. I should not say *we* neither, because the other surgeon tried, but I did not. After making several attempts, but without using much force, it was considered unnecessary to presevere longer at that time, and it was withdrawn. A slight retention of urine followed, but, after a few days the patient was again *in statu quo.* When I examined the urethra, about four days after, I found that I could not pass the smallest bougie any further than six inches, so that it appeared that there was a continuance of the irritation in the back part of the stricture, or that our last attempt had reproduced the stricture. I touched this again with the caustic, and in a few days more I passed a bougie into the prostate, and wished very much to ascertain if I could not pass an instrument into the bladder.

The patient made some objections, because the former attempt had failed, and he said, "I tell you what, Sir, I am an old man now, I must die soon, and I do not care when ; but I will have no more attempts made to pass an instrument into my bladder." I told him that it would be making his afterlife miserable if he did not submit to another attempt, and that

if he would allow me to try, I would engage that I would do no harm, if I did not do good. Well, at last he consented, and as the other surgeon had tried to pass the bougie in the right way, I tried what could be done by passing it the wrong way. So I took a very soft bougie, and having got it into the prostate, I depressed the point of it, and it unexpectedly passed into the bladder. When it was withdrawn, the patient passed a full stream, as large as he had ever done, full half a pint of horribly fetid urine, mixed with mucus, and perhaps pus, and he felt as if his bladder had been completely emptied. From this time he passed his urine in a full stream, and not oftener than natural. A bougie was introduced every third day, which was at first covered with a little blood, but on the subsequent introductions it showed none, and the parts perfectly healed.

The irritation, and even ulceration of the prostate portion of the urethra, may exist without the gland itself becoming much implicated. The irritation set up here produces symptoms of irritable bladder, and excites contractions in the urethra. This morbid sensibility of that portion of the urethra which passes through the prostate gland, may arise from an irritable bladder; or it may arise from the continuity of the membrane and the mitation of the last stricture; but I think it exists far more frequently as an original and independent disease. This irritable state of the prostatic urethra is sometimes produced by the too frequent introduction of bougies; some young men who have had gonorrhœa, but for reasons best known to themselves, have taken it into their heads to pass bougies, and the pain which they have described when they passed over that portion of the urethra, is such as I have before mentioned. The treatment should be such as is calculated to diminish the morbid as well as the natural sensibility, by the application of a stimulus, so as to produce a kind of re-action in the part short of that which increases the after irritation, and I think that great advantage may be derived from the use of bougies, but they should not be used at first of such a size as would stretch the irritable urethra. Tepid bathing, with the occasional application of leeches, will remove the irritation which is often occasioned. So much then for irritation seated in the prostatic part of the urethra.

Next I come to the consideration of the occasional ill consequences of a disordered state of the urethra. You will re-

collect the arrangement I made, first of gonorrhœa, and the occasional consequences of gonorrhœa; next I shall mention

THE OCCASIONAL CONSEQUENCES OF AN IRRITABLE OR DISEASED URETHRA.

The consequences of a disordered state of the urethra are numerous, and it is a thing of great importance in the practice of surgery to investigate the causes of diseases. Irritation and diseases may be induced in parts which have a sympathetic connexion with the urethra, even where the original affection may have been of too slight a nature to attract the attention of the patient. It appears to me to be of the utmost importance to investigate as far as possible, the causes of disease, and we may well say,

"Felix, qui potuit rerum cognoscere causus."

People complain of the effects of disease, they cannot tell the exact nature of their complaints, but they complain of certain effects, or rather defects, and how can we expect to cure these while the cause which produced them continues? We must investigate the causes of disease. It has been said, the great end of practice is experience; but I should say it is otherwise, or, in the language of the poet, that we should

"Of effects defective trace the cause."

I mentioned, when speaking of the consequences of gonorrhœa, that inflammation of the testicle was a frequent result of irritation excited in the prostatic part of the urethra, where the seminal ducts terminate, and that the inflammation was propagated by the vasa deferentia to the testicles. Now if a high irritation in the urethra, can produce a corresponding degree of inflammation of the testicle; then I say it is reasonable to suppose that a less degree of disorder in the urethra may produce a milder irritation of the testis, which may terminate in a low or chronic disease of that gland.

That the existence of irritation in the urethra may first cause and afterwards maintain a diseased state of the testicle, I think the following case will show. The subject of this case was a medical man, and he thought proper to consult me. He had an uneasy sensation from time to time in both testicles, but the left became harder than the right. He was not under the necessity of voiding his urine very frequently, but had occa-

sionally a feeling of tightness about the perinæum, and a dull pain in the glans penis. About a fortnight after he received a slight blow on the part, which caused it to increase in size, but not immediately; he applied leeches once a week, wore a suspensory truss, put on a poultice; the swelling was abated by this treatment, but the hardness remained. These means were persevered in for nearly four months, and by that time the testis was about three times its natural size, and the scrotum had adhered to the upper part of the epidydimis, and was so much inflamed, as almost to induce you to suppose that an abscess was forming. I passed a small bougie, and found a stricture about six inches from the orifice of the urethra, and a second stricture a little farther on; the bougie passed without much difficulty through the strictures, but when it was passed through the prostate, it gave great uneasiness, with a sense of heat, and strong inclination to make water, and the bougie was firmly grasped by the urethra. Finding the canal in this state I ordered the perinæum to be bathed in warm water, and a bread and water poultice to be applied to the testicle for another week; the bougie was passed with much greater ease, and it did not produce the uneasy sensations in the prostate which I before mentioned. The same plan was continued, and each time the bougie gave less pain. At the end of the third week the strictures were touched with the kali purum, and in the following week a much larger bougie could be passed; the large bougie afterwards passed with great ease, till at last the uneasiness before felt by its passing the urethra entirely subsided. The testicle, at the end of this time (seven weeks from the first introduction of the bougie), was reduced to its natural size, and you could scarcely distinguish that it had ever been diseased.

Many testicles, that I have esen condemned to extirpation, have been saved by removing the irritation which existed in the urethra. The old surgeons used to say, when they were going round the hospital, "Now if it had happened that one testicle only should have been as bad as they both now are, we should remove it." So that because both the testicles were diseased, they never thought of removing them! and it generally turned out that they recovered: and why did they recover? because they were allowed time; the irritable state of the canal was removed, and the disease disappeared.

LECTURE XXIV.

As gonorrhœa produces bad consequences, so irritation of the urethra produces bad consequences. Chronic diseases of the urethra will produce chronic diseases of the testicle, and certainly of the glands of the groin ; but I do not want to dwell on that subject. I have seen many irritable ulcers in the groin which have healed by passing bougies, and relieving the irritable state of the urethra. I mentioned that ulcers would very often break out ; there may be a cessation of the discharge from the urethra ; it may attack the prepuce, and ulcers may break out from the irritation of the common running of the urethra.

Now the irritation of the urethra leads to the breaking out of ulcers about the prepuce and glans.

This is what I call a notorious fact, and a fact of very great importance for you to attend to, because you may be very much puzzled about the whimsicalites of the sores, which very much depend on this cause, and can only be explained on such a supposition. Now to show you, as well as I can, what I mean, I must give you a case. These sores may happen when the patients themselves are not conscious that they have any disease in the urethra, and to shew you this fact I shall resort to a case.

There was a gentleman who, coming to this country from abroad, had connexion with a female, after which a crop of sores broke out—irritable, bad sores. I treated them as I should do such sores. I used measures to soothe the parts, and gave a mild alterative course of mercury, although the sores were not syphilitic. I gave him five grains of the blue pill night and morning, which was likely to cure them if they had been syphilitic, and used a little astringent wash, and in the course of a few weeks the sores got quite well. He made a solemn vow not to have any further connexion with any females of this country. He went to the play, and in one of the boxes there, a girl of the town caught him by the penis. This fresh irritation of the part was followed by a second crop of sores. I knew they were not venereal, and that it was of no

use to prescribe mercury. I suspected that there must be strictures, but he said the stream of water was natural, and he would not allow a bougie to be passed. He considered that they must be venereal, and determined to rub in, and he did so till he salivated himself, but the ulcers remained as bad as ever. He afterwards consented to have a bougie passed, and I found two strictures in the urethra, which being relieved, the sores got well without any further treatment at all. This appears to be a point of considerable importance, and I wish to impress it on your minds: I know no better method of doing so than by the recital of a case.

A medical man, in the navy, had frequent crops of sores breaking out about the glands and prepuce, for which he was advised by his friends to use mercury: the sores healed during the salivation, and broke out again afterwards. There was no court-martial to be held over the disease, and he came to the hospital one day to consult me. I told him that I suspected he had strictures: he smiled, and said, "Mr. ABERNETHY, that may do to tell your patients, but it won't do with me. Why, Sir, I can make water as well as you can." "That may be, and better for aught I know." However, a bougie was passed, and two strictures were found; these were cured, the urethra got well, and he never had any breaking out of the sores afterwards.

I can tell you another case, of a married man, who was said to be a very eccentric character, and that amongst other peculiarities, he used to absent himself from the nuptial bed, although it was admitted he had a very beautiful wife; and the sequel of this case shows how careful we ought to be in repeating any ill-natured observations, for he had good reason, as you will presently see, for doing so. This gentleman said to me, "I have crops of sores breaking out every now and then, and they are, at times, so much like venereal sores, that they have said to me, you must take mercury, for they must be venereal sores." He had a great reluctance to do so, and he came to take my opinion. Not believing that he would tell a lie, when it was his interest to speak the truth, he told me their history, and I said I do not believe that they are venereal, but there must be some disease of the urethra. He said no, that it was as well as need be. When he came again, I asked him to allow me to pass a bougie, and there were strictures in the urethra, which being relieved the sores got well.

Now, in this man's case, a fact occurred worthy of your attention ; he has sat down to dinner with his family, and no sooner had he taken a mouthful or two of meat, but it has produced so much irritation of the glans and urethra, that he has been obliged to leave the table to run into another room, and mixing some hot water with cold, he has made a warm bath of it. and sat down on the bidet, and allayed in this way the irritability of the urethra. On the other hand, there are some things which, being taken into the stomach, afford so much relief to the urethra, that some people have imagined that there must be a short cut to the urethra, or that certain things passed from the stomach to the bladder. I have known many persons who, after having taken food, have complained very much of palpitating hearts, and they have said " Oh, Lord ! how my heart beats." It shows the sympathetic connexion of the stomach with divers parts of the body, and divers parts of the body with the stomach. One day I touched the stricture with the caustic potash, and he complained of extreme pain in his sores ; there was a little fungus on one of them, slight ulceration about the surrounding parts, and when I touched that he complained of great pain in his fork, as he called it.

I have seen many such cases ; people who have had great thickening and whitening of the prepuce, and who have had phymosis ; I have seen them get well by removing the irritation of the urethra, which is not only adequate to the production of sores, and various other diseases of the neighbouring parts, but even sores arising from other causes ; it may render the progress of such sores peculiar ; therefore, whenever you see any whimsicalities of the sores about the prepuce, you may immediately suspect that there is some irritation about the urethra, and the sores get well, in a way that is perfectly astonishing, by relieving the disease of the urethra. This may serve sometimes as a clue by which you may go through the intricacies of practice.

Persons having disease of the urethra are likely to have bursts of discharges like claps, the parties with whom they have been connected having no disease. I have known numbers of cases of this kind, but it is perhaps necessary to mention one. This is the case of a medical man—a married man too, an elderly man, an old sinner—and he thought of strengthening his connexion with a family in the neighbourhood by connecting himself with the wife of another man. He was pu-

nished for it by one of the most vexatious claps, he said, that he ever had in the course of his life, and which held on the best part of a year. He wrote to have my advice, and I answered him saying, that I believed he had strictures in his urethra; he examined the urethra and found he had; he introduced a bougie now and then; the running ceased, and he did perfectly well.

You may have enlargements of the mucous parts of the urethra, from irritation of the urethra, as in claps; and lastly, you may have fistulæ in perinæo, induced as the consequence of this kind of disease of the urethra. Of fistula in perinæo—or, I may as well first speak of

RETENTION OF URINE.

This is a subject of too great importance to be discussed under one head. When I was young, it used to be a question at Surgeons' Hall, "What would you do, Sir, in a case of retention of urine?" If the respondent answered, "Sir, I would bleed, and bathe with tepid water, and so on, and allay irritation." "But would you introduce a catheter?" If the other were to say, "No, Sir, I would rely upon the other measures;"—they were considered good answers, and these questions were often put by Mr. Pott; this was the sum total of his experience, and it is the sum total of mine. I say no, do not do it; that is, where the retention of urine proceeds from irritation, inflammation, or spasm of the urethra; and you may have all these without stricture. I have related a case in the anatomical lectures, in which retention of urine followed an accident. A man, having a compound dislocation of his ankle, had retention of urine; the catheter was obliged to be introduced every day for a week, and he could not, at the end of that time, make water, so that the pupils thought there must be some terrible mischief done to the parts. I ordered leeches to the perinæum, advised the parts to be bathed afterwards with tepid water, gave the patient some castor oil to empty the bowels, and in the night he made water, and never required the catheter to be introduced afterwards. I do not wish to dwell upon this point now, I just remind you of it.

Now retention commonly arises from *spasm* or from *stricture*, but there are other causes, as I shall presently show. What is to be done when there are strictures? It is in vain to attempt to pass any instruments *a fortiori*; you ought to with-

hold them; but what you do is to ask the patient this, "Did you make water in a stream before you had this retention?" If he should say, "Oh yes, Sir, I did, in a small stream;" why you know that there is a passage, and that the retention is owing to spasm, irritation, or inflammation, and what you have to do is to subdue that irritation, and the patient will make water again. But I say that retention of urine may arise from divers causes, from enlargement of the prostate gland, from atony of the bladder; and when a person is called to a case of retention, he does not know what may be the cause of it, and this he is obliged to find out. And what inquiries would you make so as to enable you to guess at the cause of the retention? Why, you should ask some such questions as these: "How did you make water before this retention occurred? did you make water in a full stream, or were you a long time doing so?" If the patient says, "Oh, no, I could not make it in a full stream; I was a long time making it, and sometimes it would only come by drops,"—you know then that the man has strictures. If you should ask him again, "Have you any uneasiness in the trunk of the urinary passage?" and if he should say, "Yes, considerable uneasiness," then there is spasm and inflammation. Well, then, I say, you had better not in such cases introduce bougies or other instruments; if you do, you only keep up the morbid irritability and increase the mischief; and what are you to do, is the question? Now many things are to be done, and there is some method to be observed in doing them. It is a most distressing state for a patient to be in, and it is a very distressing situation for the surgeon to be in; for the patient is every now and then calling upon him, "For God's sake, Sir, relieve me, I can bear this pain no longer;" and it requires some fortitude on the part of the surgeon, to abstain from doing what is wrong, under such urgent solicitations. I say to the patient, get to bed, you must get to bed, sponge the perinæum with tepid water, and foment the lower part of the abdomen with flannels wrung out of hot water, as warm as the patient can comfortably bear; let them remain on for five or six minutes at a time, and they will be found very soothing to the patient, it is a mode too of passing away the time very usefully; put leeches to the perinæum, that is an excellent practice; bleed from the arm, if the patient be plethoric; and there is this general rule for blood-letting in such cases, if there be

any general febrile action, blood should be taken from the system ; but where there is not that general febrile action, blood should be taken locally. But *first* give the person a dose of castor oil to clean out the bowels, that is a great thing ; give this the first of all ; you should next apply the leeches and use the tepid bathing. By and bye the castor oil operates, the bowels are freely relieved, but still the retention continues. I say again to the patient, go to bed, and continue the fomentations over the lower part of the belly and genitals, for that is what I call local tepid bathing, and do not be out of heart, you will be relieved ; after a little, some urine dribbles away in the flannels, and this for a time removes the urgency of the symptoms, and relieves him for a time. If the urine dribbles, and dribbles, and dribbles, you may depend upon it he will come about ; but where there is no discharge of urine are the cases which cause the most anxiety. Well, then, supposing there is no dribbling, then you should give an opiate glyster ; this composes him for a time, and, if he gets fidgetty, give him another. Some say you should give opium by the mouth, but if I gave opium I would combine it with calomel, for the calomel appears to have a different effect from that of a mere aperient, but I say I should rather combine them.

You have heard, no doubt, of specifics employed for retention of urine ; there is the *tinctura ferri muriatis,* and I know that it does good sometimes, by producing nausea, but rarely do I see it answer. They say again you should give tobacco glysters to produce relaxation ; but I rarely find that any such things are necessary, for I can generally accomplish the whole by the simple measures which I first explained to you. Patients are afraid that their bladders will burst, and the surgeon is apprehensive and alarmed, and at the request of the patient sets about passing the instrument ; but the bladder does not burst, it will distend itself until it reaches the navel, and yet there is no harm done. As to the introduction of instruments into the bladder, it is a practice not to be decided upon in four and twenty hours. In answer to this it may be said, " Well, but, Sir, I have seen bougies introduced and relief obtained ;" to which I should reply, " Well, so have I, and done it myself in many instances." I have passed down a soft bougie to the strictures, and having held it against the obstructed point for a time, and on withdrawing it the patient has voided his urine

partly, but he has not been relieved of the retention; on the contrary, it has been rather prolonged by it. I have done so, and afterwards the patient could not make water, his bladder is distended as much as before, and I question if the retention has not been continued longer than if the bougie had never been introduced. The introduction of the varnished catheter, they may also say, gives great relief; but I know that relief would also have been obtained as well without it, and, as far as I know, patients who have had retention arising from such causes as I am at present considering, have made water better after the suffering and the medical discipline which the retention has called for.

I have next to advert to other causes of *retention* of urine which I have before mentioned, and first of the *enlargement* of the *prostate gland*, and you should see the history of some cases of this disease which have been published. It is the enlargement of that portion of the gland, called by Mr. HUNTER, the "valvular portion;" it is "the third lobe" of Sir EVERARD HOME, and the *trigone* of other authors.

The enlargement of this part is attended with some uneasiness, and there is a frequent desire to make water, especially when the bladder has reached a certain degree of distention, but if they neglect to evacuate the bladder at that time, however anxious they may be to make water, they find they cannot do it. It often happens that men, being engaged in business, or sitting too long at a time after dinner, may have retention take place in this way. There is no tenderness in the track of the urethra; they will tell you they made water a short time before; and it generally occurs in old people. You take a full sized bougie, or catheter, and having introduced it to the prostate gland, you depress the handle, which of course elevates the point, and it passes into the bladder. Many such pensioners have I had in my time, who have had occasion to send for me to relieve them when they have neglected to empty the bladder at the proper time.

After the bladder has been over-dilated, it is incompetent to expel the urine for a certain time after, and the time which it requires to recover its power is, on an average, about three weeks. I hold that the profession is very much indebted to the late Mr. HEY, of Leeds, for his observations upon this point, as well as many others. He thought that it was an object to determine, whether the bladder would soonest gain the

power of expelling the urine by leaving the catheter in all day, or by the common mode of introducing it only night and morning; and after many comparative trials which he made, he found that those patients did best, and recovered the power of expelling the urine first, who had the catheter introduced only night and morning. After he had gone on with this point to a sufficient extent, he set about another, which was, whether it was better to continue to introduce the catheter during the day, when the patient could void a portion of his urine, or whether it should be passed until he could pass it completely. Now the bladder may have recovered its power to a certain degree, but not thoroughly, and Sir EVERARD HOME has advised you to do this: Tell the patient to try to empty his bladder, and when he thinks he has emptied it, you are to introduce the catheter, and you may find half a pint of urine there, and in that case the catheter should be daily introduced; but if it be completely emptied you need not introduce it again.

Again, you may have *retention* from *want of power in the bladder*. The detrusor muscle of the bladder not being able to act, the bladder becomes very much distended, and a dribbling takes place, which is very apt to be mistaken for incontinence. Mr. HUNTER says, that the bladder being distended, the abdominal muscles press on it, and a dribbling takes place, which is apt to be mistaken for an incontinence of urine. Well, but you may judge of this being the case, by making pressure with your hand above the pubis, and you feel the bladder very much distended; you must introduce a catheter daily, as in the case of enlarged prostate; the bladder is weak from being distended, and it is not likely to recover if it be kept distended. So much for retention of urine; and now I go on to the subject of

FISTULA IN PERINÆO.

Now as to fistula in perinæo, there is some considerable variety in the cause of this disease; as far as I have read in surgical books, and even in Mr. HUNTER'S book it is accounted for in this way: the stricture being very complete, and opposing the passage of the urinary discharge, the bladder at the same time continuing to act, and distending powerfully the urethra behind the stricture; the urethra gets into a fidgetty

state, and either by sloughing or ulcerating about the stricture it gives way, and the urine gets diffused into the surrounding cellular substance. Many cases of fistula in perinæo occur in this way undoubtedly, but they occur in other ways. These accidents, however, frequently occur, and I shall tell you a case. A gentleman living near London, having retired from business, and about seventy years of age, was attended for some malady by a physician; this physician called upon me one day and said, I wish you would bring a bougie or two with you, and visit this gentleman, for he has got a swelled testicle. I did as I was bid and went; but as soon as I saw the swelling, I said, Oh! there is urine here; this is an extravasation of urine. I pointed out the nature of the case to the patient, and advised him by all means to allow me to make an opening for the discharge of the fluid; but he thought I was a young man, and would allow no operation to be performed but in consultation. I wrote, at his request, to some surgeons, desiring them to meet me there as early as possible, but I knew they would not go till the morning. I set off again, and took a varnished catheter, thinking I might be able to introduce it, but nothing of the kind was to be done, and at that time the penis was quite distended, and although I did all in my power to advise him to let me make an opening, he would not consent, therefore he had to await the consequences. Next morning, when the consultation met, the man was dying, he was comatose; there were patches of mortification on the penis and scrotum; and they considered that nothing could be done—they had nothing to recommend. I knew that the man had a good constitution, and that some attempt should be made to relieve him. I drew his legs out over the bed, and there they hung without any power. I made an incision about an inch and a half in length, just where the incision for lithotomy is made, between the crus penis and the corpus spongiosum urethræ, and carried it backwards in a line parallel to the course of the urethra, and, putting my finger into the wound, I moved it about freely till I found the opening in the urethra, and when I withdrew my finger, out splashed about three pints of stinking ammoniacal urine, and presently after the old man crept into bed. A small sized varnished catheter was introduced, which was gradually increased in size every week; the sloughs separated, and the patient recovered. Now I mention this case for another reason than that of showing what the treatment of

fistulæ should be. This man had been in the habit of passing bougies all his life-time nearly, and seemed to be perfectly well before this occurrence took place; it therefore shows that it is not the enlarging of strictures that will cure them, but correcting that morbid irritation in the urethra, which is the essence of the disease. This is one case of fistula in perinæo, in which the business of the surgeon is to make a large gap or opening in the urethra, to let out the extravasated fluid. You must also recollect that the fascia of the perinæum very much influences the escape, on the first appearance of the swelling caused by the extravasated urine; and it is a point of some importance to attend to, when about to make your incision, to ascertain, if possible, on which side the urethra has given way, so as to direct your incision to that part. I was called to an Irish baronet, who had a tumour appear by the anus, on the nates, which, as soon as I saw, I said was caused by extravasated urine; it had never been suspected that he had any disease of the urethra by his medical attendants. I made an incision, let out the urine, afterwards introduced a varnished catheter, and he recovered. I have known the urine travel even forwards and upwards, and cause a swelling in the groin, from which, when it was opened, matter and urine were discharged, and, which is very singular, there was a fistulous communication behind with the rectum, so that flatus escaped by the wound in the groin from the rectum.

LECTURE XXV.

I HAVE been speaking of irritation of the urethra, as the consequence of gonorrhœa, and of the diseases of the urethra and neighbouring parts, which appeared to be kept up by this irritation; but I have now to advert to another cause of irritability and disorder of that canal, namely, a disordered state of the digestive organs, and therefore it may be considered as a constitutional malady.

THE CONSTITUTIONAL ORIGIN OF DISEASES OF THE URINARY ORGANS.

I say that this irritable state of the urinary organs, and of the urethra particularly, may be often traced to a constitutional cause. It is a fact that brothers, generally, have strictures; fathers and sons have them frequently, and therefore I think it is right that you should consider the constitutional origin of diseases of the urethra.

I may mention that dysury is very likely to occur, even in children; and I have known irritation continue and produce strictures, and even abscesses, so that it has led to diseases of the affected organs. So again, dysury has occurred in married men, who never had any gonorrhœa or strictures; yet they have had affections of the urethra, irritation set up, producing dysury, stricture, and even effusion of urine into the cellular substance. Now I feel very well convinced, in my own mind, that it is a disordered state of the digestive organs which produces the state of constitution which leads to dysury of this description. I think that I have a fully sufficient number of facts to warrant the inferences which I have drawn from them, and I have related these facts. I have seen children who have been searched for stone, being supposed to have calculus from the urgency of the symptoms of the dysury.

There was a boy, who lived at Islington, who suffered so much from this complaint that he was supposed to have a stone in his bladder, and was searched by many eminent surgeons; one of them said, at last, that he felt a stone, but advised the operation to be delayed, because the stone was not large enough to extract, and there the poor child was kept living in torment. I was afterwards requested to see the child, by a gentleman who had been a pupil here, and I did see him, and in great suffering the child was. I looked at his tongue, and it was very dirty and furred; his bowels were very much out of order indeed, and there was great constitutional disturbance. I said to the gentleman I went with, now I think you ought to be whipped, for allowing the child to remain in this state, when so much might be done to relieve him; for whether there is stone or no stone, you may palliate, very much, the sufferings of the patient by setting his digestive organs to rights, regularly relieving the bowels, and regulating the diet. The

bowels were kept open without teazing them, alterative doses of blue pill were given to increase the secretions of the liver a little, the diet was regulated, and in six weeks the child was perfectly well.

I say that dysury in women is a very common occurrence, as well as in young persons, arising from the same cause. I have known females so tormented by irritation and pain about the urethra and bladder, that they have even consented to have an examination made, under the supposition of their having calculus; and a woman will suffer a long time before she will consent to that. By putting the bowels and stomach right they have lost all their uneasiness.

If the stomach cannot digest the food which it receives, there is a great deal of unassimilated aliment circulating in the blood: it cannot get out but by the kidneys, which get overworked; they have double duty to perform to get rid of this unassimilated aliment. Besides, the kidneys are supplied by the same nerves as supply the bowels, namely, by the splanchnic ganglia, and therefore there must be a great mucous communication between these organs. I have told you cases in which disorder of the digestive organs would produce great variety in the quality of the urinary secretion, and that this result appeared to happen sympathetically from the disturbance of the digestive organs. I told you an instance of a person whose stomach was put wrong by an operation, and for three days no discharge from the bowels could be produced, and during this time there was no secretion of urine, but as soon as the bowels began to secrete, the kidneys secreted also. It was strange sort of stuff as urine, but still it was the secretion of the kidneys.

It is not the kidney only which is disordered, but the bladder, urethra, and all are disordered, and I can describe to you the state in which they are, by telling you what the patients themselves say of it. A patient says, "There must be, certainly, something very wrong, Sir, in my water-spout, for every half hour through the morning I am obliged to make water, and I am very much disturbed by this; but the strangest thing of all is, that after dinner I can sit, if it is until twelve or one o'clock, without feeling any inclination to do so." Well, I sometimes say this: "The urine which you pass in the morning is limpid and clear as water, and if you were to try you would find it as tasteless; but that which you make at

night is very foul. "That is exactly the case," they say. Is it not strange that the bladder should be so anxious to get rid of such a mild fluid? But the reason is this, the same degree of nervous irritation which affects the kidney and causes it to secrete such a fluid, affects the bladder also, with the urethra and all the organs, and causes it to get rid of that fluid, although in itself unirritating. After dinner, however, the nervous irritation ceasing, the kidneys separate more animal substance, and the urine being fully loaded with various salts, is nevertheless retained in the bladder. Then I say, that in the case of irritation of the urinary organs we should soothe the parts by local tepid bathing, but the lower bowels should be particularly cleared out. There is great sympathy between the lower bowels and the bladder; when the rectum becomes irritable, the bladder is made irritable, and vice versa. Well, in addition to the regulation of the bowels, it will be necessary to excite the healthy actions of the liver; endeavour, in short, to put the digestive organs to rights, for that is the great thing to be attended to. I have often spoken of diseases of the urethra, and their consequences, but there is another affection which I may as well mention here, which will finish the subject of diseases of the urinary organs, and therefore I come now to the irritable bladder.

IRRITABLE BLADDER.

The bladder is often irritable, capricious in its actions, and tender in its lining membrane, so that it will not bear distention; capricious in its actions, acting partially, not emptying itself entirely, and therefore liable soon to become distended again; not bearing distention, and tender in its lining membrane, and on that account irritable. The lining of the bladder may go on to ulceration; there may be a puriform secretion, and blood may be discharged with the urine; yet all these cases go under the title of irritable bladder. I have known blood and pus discharged in the urine without ulceration; but when the bladder does go into a state of ulceration there is but little hope of recovery. The function of the bladder is such as almost to preclude the possibility of an ulcer healing; acrid urine is continually applied to the sores, the ulcer is stretched by the distention, and even injured by the contraction of the bladder. You can hardly expect an ulcer of the bladder to

get well, and, generally speaking, it is a lost case. I know not what is to be done; there are no remedies, but those of steadily pursuing the plan of soothing the local irritation, and putting the digestive organs to rights; and I have known instances in which people have got well, which you will hardly credit; but I will tell you the facts of one case.

A man, having his bladder in a state of ulceration, after having tried various measures, was given over by the surgeons in the country, and he resolved, as a last resource, to come to London, and consult some of the surgeons here. How he stumbled upon me I do not know; there might have been something whimsical in the name which struck him; however, he sent for me. I found the man in a dreadful state, extremely emaciated, in great pain, voiding blood and pus in his urine, and discharging it very frequently; he told me how long he had been labouring under these sufferings (I do not now exactly recollect); he had been given over by his surgeons, and he was determined to adhere to any directions which might be given. I did not entertain any hopes of the case, but I told him this: "If I were in your situation I should adopt the following plan: I should pay the strictest attention to my diet, buy a pair of scales, and weigh every bit of food that I ate, for I think that a rigid attention to your diet will afford you the best chance of recovery." I told him that it was unnecessary for him to remain in town, and he again went into the country. I am sure that I never expected to hear from the man again, and every person who saw him thought he was dying. About five years after this, I was sent for to the house of a gentleman at the west end of the town, a Member of Parliament, and was shown into the drawing-room, where there were several persons assembled, talking what I call very fast and loosely on divers subjects, and amongst other subjects, a gentleman there was talking about an excellent trotting mare which he had bought; "and, by the bye," said he, "Mr. ABERNETHY, I bought her of a patient of your's. Every body was surprised to see at what speed she went, and how well the man rode, but when he left the country for London, no one ever expected to see him again." He might as well have been talking Hebrew, for any thing I could understand of all that. At last he told me that he was member for such a place, and that from that part of the country a man had come to London and consulted me, having a diseased bladder, and asked me if I did not re-

collect the case. I said I did not; but when he told me some further particulars, about weighing his food, and so on, it brought a recollection of the case to my mind, and I was as much astonished as the gentleman was to hear that he had been trotting horses. It appears, however, that the man departed from his good way of living, and about twelve years after I first saw him, he came to town again for advice, having some affection of the heart, of which he ultimately died, but the urinary organs were not in the least disturbed. The secretion of the kidneys, of the bladder, and all the urinary organs, and although there is a very undefinable state of the nervous functions, yet there is a very definable state of disorder of the digestive functions.

I know of no specific remedies for this complaint, none worth talking of. The *uva ursi*, I know is recommended, and it unquestionably does good sometimes, but I suppose this to be by its action on the stomach as a light tonic. It has been recommended in affections of the mucous membrane of the lungs. The uva ursi seems to do well, when there is a little discharge of mucus from the bladder, with a degree of languor of the patient's constitution, but not in the irritable bladder.

Mild doses of *alkalies* do good. Ever since I have given lectures, I have made this observation respecting alkalies—our power of giving alkalies is limited; small doses of alkalies render the urine less acrid, and seem to diminish the irritation of the urinary organs; but when we increase the dose of the alkali, we increase the irritation of the urinary organs, and therefore our power of using them is limited; you cannot push their use beyond a certain dose. The teribinthinates have been recommended, as the *copaiba* balsam, the oil of *turpentine*, and they do good in some cases; they seem to impregnate the urine with something which is warm and comfortable, but we know not what that is, only it seems to do good. I have seen, when people have been voiding offensive foul urine, that taking turpentine and the copaiba balsam has produced a considerable change in the qualities of the urine. The compound spirit of sulphuric æther, *Hoffman's anodyne liquid*, does assuage irritation in the urinary organs; it does good in one case, but not in another. Concerning all these sorts of measures, I say I can tell you nothing about them, we try them, but I state that they are all upon this sort of footing, that they are things which have nothing to do with the general principles of treatment, for we

do not know the principles upon which they do good. I say we must put the digestive organs to rights, and all these remedies may be termed empirical. What we call the *juvantia* and the *lædentia* in the treatment of disease, must be ascertained by experience; we must try them.

Well, now, here I finish the subject, and only think what a way I have been led astray from the arrangement which I had made of the lectures. I was talking of diseases induced by external causes, which were arranged under such as were mechanical, chemical, and poisonous; the last division led me to speak of the venereal poison, and next of clap, as being something analagous to that poison; and that led me to speak of the consequences of claps, namely, diseases of the urethra, and, lastly, the consequences of diseases of the urethra. This was the arrangement which I had made, and I now return to the consideration of diseases induced by poisons. I need not speak of the variolous poison, or the vaccine, or measles, you are already very well acquainted with them; but I shall go to the subject of the

POISONS OF RABID ANIMALS.—HYDROPHOBIA.

I beg you to consider what is to be done in the case of the canine poison, communicated by the bite of a mad dog, or it may be of a cat; unquestionably it may be communicated by the bite of a cat, but I have never known this disease appear in a cat, unless previously bitten by a dog. I do not think it ever originates in the feline species, and whether it ever originates in the canine species without contamination is a doubt; in short, the origin of this peculiar poison is very uncertain. How such poisons ever originated I do not know, for they do not seem to be generated afresh.

Mr. Hunter used to say in his lectures, that dogs would not get mad spontaneously, and in proof of that assertion he told the following story: In one of the Islands of the West Indies, (which I do not know, but any one must be hot enough, God knows,) where every negro kept a dog, this disease never appeared in the dogs of the island, except where other dogs were brought in a ship from America, from that part of America from which the run to the island was short. If a dog had the disease in Europe, he would have shown signs of it before he got to the West Indies, and would have been chucked

over-board in spite of the hydrophobia. But this is a very curious poison, for it affects all animals; now many poisons do not; you may inoculate animals with the venereal poison or small-pox, but it will never be produced in another; but all sorts of animals are affected by this disease—horses, cows, pigs, and even poultry. But whether they generate the poison or not is unknown, because it is not the habit of all these animals to bite, and therefore it is uncertain.

This poison differs from morbid poisons in general, inasmuch as the wounded parts into which the poison may be introduced do not inflame; it has so little acrimony that the parts heal like a common wound; but when it is introduced into the circulation, there is a very great and peculiar disturbance of the system indeed. This disease, however, does not make its appearance until some considerable interval has occurred after the bite, very frequently in about three weeks after the inflammation in the wound has subsided; in some cases it has appeared at a much longer period, but I do not know that it has ever happened at a much earlier period. And this has led to the inquiry whether the poison lodges in the part for a time, and being afterwards absorbed and carried into the circulation produces its peculiar effects; or whether the poison, being at once absorbed, requires a certain time to elapse in order to produce its specific effects?

Now, I recollect that during my apprenticeship at this hospital, for a considerable time we were never without some cases of hydrophobia in the house, and then again for almost twenty years there was no instance of disease occurring; then it became prevalent again. During this interval I always said, that I thought it very reasonable to suppose, that the poison lodged in the part, and afterwards absorbed, and that as soon as it got into the constitution it produced its specific symptoms; and that it was a subject extremely worthy of investigation, for if it could be ascertained by facts, then might we at any time cut away the affected part and there would be no necessity of doing so immediately. As far as the experiments have gone which have been made to determine this point, it is probably true; individuals who have been bitten, have had the parts cut away several days after the accident has happened, and have recovered. However I do not think there is any thing certain about it. I will tell you a case which occurred in the hospital when I was young, before that long interval

of which I have just spoken A boy was bitten by a mad dog in the hand; the parts were cut out, and caustic was afterwards applied before he was sent into the house; poultices were applied, the sloughs separated, and the wound was healing very well. Going one day through the ward, I asked this boy how he did, he said, he was pretty well, but that he had a little uneasiness creeping up his arm, and on turning up his garments, I observed two red lines passing up the fore-arm; this was absorbent inflammation; I appeared to take no notice of it, said that it is not of much consequence—you will be better to morrow, you must put on a poultice and go to bed, and take some medicine to open the bowels. I was anxious about the boy, and went early into the ward in the morning, and, as I came in, I stopped with one of the other patients, as if my visit was to him, and, as I was going, I appeared accidentally to see the lad, and asked him how his arm was; he said it was better, and on looking at it, the inflammation had subsided. But said he, "Sir, I have been very unwell all night, I have been very uncomfortable; and have felt some stiffness about my neck." I asked him to put out his tongue, and I found it was furred; his pulse went quicker than usual, so I told him that he was a little feverish, and asked him if he were thirsty; he said he was not very thirsty, but he would take something to drink. So I called the nurse, and said you should not leave this boy without something to drink —you should have brought him some toast and water, he is a little feverish; bring him some while I am here. She brought it, but, immediately he saw the water, he turned away his head with great agitation, and said he could not drink it. It was not the mere sight of the water, but the consciousness of extreme difficulty of deglutition, which caused him to avoid it. In twenty-four hours the boy was dead. Now, it appears, that although the parts had been cut out, the disease continued, but we cannot say how it was done, and the majority of cases have been in favour of excision. I say, that we need not be so much in a hurry to cut out the parts immediately, they may be removed in a day or two, and, if removed properly, it seems very probable that the poison will also be removed. If a dog bites a man through his garments, he wipes his teeth of the poison, and there is less chance of a communication of the infection, then when naked and exposed parts have been bitten.

Caustic has been strongly recommended to be applied: but by using the caustic we redouble the injury and destroy unnecessarily the neighbouring parts. FONTAINE, who made so many experiments with poisons, tried what could be done by rubbing caustic freely into the wounds, but it did not prevent the operation of the poison. Caustic will not do: excision is a mild thing to the application of caustic; it is a mild thing in comparison to the mischief done by the application of the caustic. And when I say excision is a mild thing, I mean when it is rightly applied. Every tooth does not penetrate, the small front teeth do not penetrate, only the tusks; the others may scratch the skin a little, but they not go deeply into the flesh, and yet it is often cut away from them as if they had penetrated the parts deeply. Wash the parts with a little soap and water, and you will wash away the poison from the wound. But as to the tusks, the fangs make deep wounds, and then what you should do is, to introduce a skewer into the wound, and carry it as far as the tusk has gone; having done this, you cut through the parts around the skewer, and completely cut it out, examine what you cut out, see if the cyst, which should be found, is imperfect in any part, and if it should be, remove that part. You cut out the skewer and the cavity which contained it by an elliptical incision, by which all that animal substance with which the fang came into contact is removed.

Caustic wont do; and I think the case which Mr. HUNTER has recorded will show it to be inefficient. Mr. HUNTER was called to a young lady who had been bitten in the lip by a favourite lap-dog. Mr. HUNTER thought that a prompt application of caustic to the part would destroy the poison; he applied caustic, and you may suppose that he used it liberally; however, the young lady died of hydrophobia. This gave great cause for triumph to Mr. JESSE FOOT, of whom I suppose you may have heard; he said, why not have cut out the parts as is done in the operation for hare lip, instead of applying caustic, and the patient would have had much better chance of recovering. True, she might have recovered, but it was a very ungentlemanly, illiberal observation, it was not doing as he would be done by. It is bad enough for a man to have the mortification in his own mind of being unsuccessful, without being taunted about it afterwards. I say it is not doing as we would be done by; but Mr. JESSE FOOT was always opposing Mr. HUNTER, and cavilling at what he did, and I suppose

he is now almost forgotten. What incensed me more against Jesse Foot than any thing else, was, that he spoke very disrespectfully of Mr. Hunter after he was dead. There was something very malicious in this, and unmanly too; for I would say with Zanga: —

> "Were I a lion, I would not feed
> Upon carcases: I war not with the dead."

In hydrophobia there does not appear to be any great febrile action, but the nerves are principally affected; there is great mental anxiety; irritated by trifling circumstances; they have no actual dread of water, but a horror of deglutition. Mr. Hunter relates a case in which the patient was raised into a violent fit of anger, and he could not tell why, but as soon as his passion had cooled down again he said it was because a gentleman near him did not offer him a pinch of snuff. It is impossible to describe the state of mind which these poor creatures endure, and yet there is not any great febrile affection, but a severe affection of the muscles, over which the mind has the greatest influence, such as the muscles of deglutition and speech. How much these are influenced by the mind may be known by a man being almost choked in the performance of a tragedy. The diaphragm too is very much affected. The œsophagus and lining membrane of the stomach have been found somewhat inflamed and spotted; and when you find spotted appearances in the stomach and bowels you are led to suppose that the dog was mad. A dog that is mad runs about with his tail between his legs, and dropping his head, he looks dull and miserable, but at the same time is very irritable; he bites hay, wood, or what comes in his way; and is snappish and irritable.

TREATMENT OF HYDROPHOBIA.

Now as to the treatment of hydrophobia I have nothing satisfactory to offer. Bleeding has been recommended; copious bleeding; but when I heard of it, I said, they might bleed me if they liked, and thank them too, for if I was conscious of labouring under the disease, I should have so little hope of recovery that I should be inclined to do as Seneca did, open my veins, and sit in warm water; although I suppose from the dread of water, I should not be allowed the luxury of dying in a

warm bath. There have been other remedies proposed, but none of these appear sufficiently established to enable me to recommend them to you—you must read of them and judge for yourselves. Indeed, what can we give when a man cannot swallow, even if we had remedies. It is of some use to keep the bowels open, and keep the patient quiet; and there is that Croton oil which is useful to procure purgation, which we could not do before, for a single drop of it put upon the tongue acts upon the bowels. A pamphlet has been published about the appearance of pustules under the tongue, and that cutting them out will prevent the disease; I do not know any thing further about that—you will no doubt read it for yourselves. Now as to the

BITES OF VIPERS.

They occasion considerable inflammation and discoloration of the bitten part, but the bites of vipers of this country are seldom fatal; they do not appear to produce any constitutional effects; but the constitutional disturbance which attends these bites appears to be in proportion to the degree of the local injury. It has been said, that applying a ligature about the bitten part will prevent the progress of the inflammation, and it may be tried. You have to soothe the local inflammation, when it is severe, by tepid bathing and poultices, and anointing the part with sweet oil has been recommended. This appears to be the general practice, to anoint the part with sweet oil, and to use the volatile alkali as a cordial to the stomach. but it should not be continued after a certain time, when the patient gets feverish and irritable. A man was bitten by a rattlesnake and taken to St. George's Hospital; the case has been published in the Medico-Chirurgical Transactions, which I dare say you will all read; but what the treatment was I do not now recollect.

LECTURE XXVI.

CONNECTED WITH THE ANATOMICAL COURSE*.

DISEASES OF THE NASAL DUCT, ANTRUM, &c.

In the inside of the nasal process I perceive a groove, which is for the *lachrymal bag*, and continues downward, forming a sort of canal; not a complete canal, indeed, but pretty much of a complete canal. It goes down the bone, and is called the *ductus nasalis*. It is the canal for the tears to run into the nose. And it is of great importance that you should understand this; because, if you are to lay open the lachrymal bag for the purpose of clearing away any obstruction in it, and if you are to apply force to it, it is of the utmost importance that you should use that force in a right direction; for if you used it in a wrong direction, you would be going against the bone. But, luckily, the ductus nasi runs nearly perpendicularly; not exactly so, but nearly perpendicularly, having a slight inclination backwards.

Then I see, at the right of this nasal process, a sort of ridge; a ridge across which the *inferior os spongiosum* is extended. It is a bone as light as possible, convex on that side next the lamella, and concave on the other side. I dont know that we have any right to consider it a separate bone; I may say, it is soldered to this ridge; and therefore the ductus nasi terminates below the inferior os spongiosum. Now I may tell you with a view to impress this anatomical fact on your memories, that on the continent, they dont adopt the same mode of clearing away the obstruction that we do; they clear it away by putting an instrument into the nose, and turning it up into the ductus nasi. If you go into an hospital abroad, you will see many instruments lying, which are used for that purpose. To me it seems a bad mode of treatment, for it requires great talent, and a great deal of knack and ingenuity, safely to in-

* These Observations are carried on in distinct Lectures, for the convenience of the reader, and for the purpose of giving a complete and connected view of the physiological and pathological opinions of this eminent surgeon.

troduce the instrument. And where you are obliged to use a great deal of force, it is very likely that you may use it in a wrong place.

THE ANTRUM.

Disease in the antrum is a very, very important subject indeed; and I always take occasion to mention diseases, when I think they will impress anatomical facts on your memories, and where the importance of those facts may be shewn. The disease of the antrum, then, I say, makes a great subject in surgery. A man may have a kind of chronic inflammation in the cavities of his antrum—a kind of morbid secretion of stuff in his antrum. It might have originated from the toothache affecting the lining of the antrum; and a very pretty predicament he is then in. Egad, when he is in bed, on turning round, a quantity of stuff will come into his nose, most disgusting to himself, and most horribly disagreeable to his bedfellow, if he has one. An aching pain in the cheek, and a dreadful discharging of matter, will torment a man in that way for years and years. What is to be done? Draw one of his teeth, to be sure; and from the socket penetrate into the antrum. But which tooth are you to draw? is the question. There is an important question. It luckily so happens, that all the grinding teeth of the jaw are below the cavity of the antrum. There are two small grinders, three larger ones, and all those grinding teeth are situated below the cavity of the antrum; and if you put the probe through the socket of the first grinder, it goes into the cavity of the antrum. Turning the head far back, and putting your instrument in, where does it go? Why, into the cavity of the antrum; and, luckily for the benefit of memory, it happens that all the sockets of the grinding teeth are placed below the cavity of the antrum. Which tooth then should I draw? Why, I would rather draw a rotten one than a sound one. You may draw which you please. Then putting a gimblet up into the socket, you pierce the cavity of the antrum; put in a piece of bougie, and having pierced the socket, it then lies snug between the teeth in the jaw. What is the patient to do? Why, when he washes out the antrum, he has only to pull out the bougie, clear away all the matter, and there is a subsidence of the disease. And many, many persons have I seen relieved from a state of great discomfort by an operation of this sort. But this is a trifle.

A morbid state of the membrane may be induced, and it may throw forth a fungus; that fungus growing, fills the whole cavity of the antrum, sprouting forth in all directions till the bone becomes absorbed, till that bone which forms the strong part of the cheek becomes soft, the orbitar plate becomes soft; until the fungus will make its way into the socket of the eye, shove the very eye out of its socket, and the fungus itself will actually shoot out of the mouth, get into the nostrils, and fill up the nostrils, and what then? Why, just see a man in the dreadful state of having his eye turned out of its socket, and the fungus shooting out of his mouth, and growing up into his nostrils; and you say this is horrible—I'll have nothing to do with it. And in many cases you would be right. You had better have nothing to do with it. But though many of these cases are the results of morbid actions which nothing can cure, if you neglect to do any thing at all times, in the interim you would neglect to serve many persons who might be most materially comforted. What should be done then? Why, when you see the fungus growing there, it is your duty to take it out of the mouth. Trefine the bottom of the antrum; put your finger into it, and scoop it out. A most bloody scene it is! A most desperate hæmorrhage ensues! I have seen a quart of blood lost when this was performing. But this does not frighten a surgeon who understands his profession, for he knows it is the discharge of blood from a multitude of small vessels; and if he saw the same discharge from a large one he would be horribly afraid. He would say, if this was a discharge from one vessel the patient would die; but it being from a great many, when he becomes faint, they will stop bleeding. But you do not give him time to faint: scoop it out, put in a bit of sponge, wrench it in; tell the patient to bite it, bathe his face with cold water, and what becomes of him then? There is no more bleeding; after a little time the sponge becomes loose, you draw it out, and any thing collected in the antrum comes into the mouth, and thus, if I may so say, you prevent the walls of the antrum giving way. Now I have seen several people made comfortable in this way. It is true the fungus may grow again, but then you may scoop it out with your finger; you may put your finger into the antrum and scoop it entirely out. Still I am aware that all this is of a cancerous nature; it is a morbid action, which will go on and extend beyond the antrum, even between the different sinuses of the head. I have

seen some osseous matter formed sometimes in the different sinuses of the head. I remember one case particularly, as showing the morbid nature of it, and yet showing that all the cases were not of that horrible nature which would prohibit a surgeon from interfering with them.

I shall relate one instance of a poor man who came into this hospital—a poor fellow who had this morbid growth in his antrum. He had his cheek in a dreadful state, and he applied to a quack, who put a large caustic on the tumid cheek; a large and sharp caustic. It made a slough; the slough came off, and it healed, leaving a hole by which a probe could be put into the cavity of his antrum. He was relieved so far, but there were morbid actions set up; the bone was affected as well as the membrane, and the bone gradually threw forth a fungus. There was a little aperture, and that little aperture increased in circumference. There was a bony substance thrown out from the aperture, which at first seemed like a little cup on his cheek, and it gradually increased until it became like a basin—like a large breakfast basin, the bottom of which laid on the antrum, covered over with a thick skin; it had gone to such a degree, that unless you had sent M. Le Dran to it, there was no other surgeon who would undertake it; none here, I can tell you; and to be sure there are many operations set about which do not seem to me to be vindicable; you had better let the poor creatures die. In looking at the French reports, I can read of taking away the whole of the *maxillaria superiora* bones, and leaving only the hollow jaw! Why, to have shot the man with a bullet would have been much more humane than that. To have let him die, as he would have done, would have been, what I should call, only christianity; to have done that would have been doing to others as you would be done unto; and a much better way, than making a person such an object as long as he lived. Well, this poor chap used to go to a quack, of the name of *Doctor Bussey*, and he bound it up for him from time to time. He also often came here, and I always told him, if you like to have it treated so, be it so; but I would let it alone. I would neither endure the pain, for it is altogether unavailing, nor would I throw away my money in such a way; but he went again and again to this quack, until he could bear it no longer. Well, I was going to say, that many of these actions extend beyond the limits of the antrum; but if you can take away the distressing cause, if you can take the

fungus out of the mouth, you may render very effectual service to many people.

The *ossa nasi* are two in number, forming an *arch* for the protection of the nasal lamella, which supports the ethmoid bone. He then said: this arch is a very strong arch indeed, protecting the nasal lamella, and how important is this! If it were not so, a man having a blow on the nose might have his *crista galli* driven in upon his brain. What is to prevent it? Many a man has his *crista galli* driven in upon his brain, producing cerebral inflammation, and the man dies in consequence; I have seen accidents of that kind. Now what is to be done if a man gets a blow on his nose, and has his *crista galli* driven in upon the brain? What's to be done? why you must try to pull it out again. You have to raise up the ossa nasi, introduce an instrument lined with sponge, raise up the depression, and then pull it out. But, luckily, this is a very rare accident, and I do not believe that every broken nose is followed with such frightful consequences. You see the nose is excessively well constructed to elude the effects of violence; the blow lights on the elastic part of the nose, which breaks the effect of it, and the blow may operate on the nose without driving in the nasal lamella. 'Faith, if every one had their crista galli driven in, when they have their nasal lamella broken, I myself would have had it, because, on one occasion, when I was riding, my horse's head and my own came pretty near together. I was riding, and, on putting the spur to him and pulling the bridle, he threw up his head and struck me with it right on the nose. The blood flowed from it, just as if it had been streaming from an arm after you had introduced the lancet. I got off; got into a stable near at hand; washed my face, and squeezed the bones into their proper situation as well as I could. The people were certainly very kind, and wished to send for a surgeon to me; but I told them I would rather they sent for a hackney coach, which they did, and I went home in it. I then perceived, for the first time in my life, an imperfection in my sight. I could not see more than two-thirds of an object. First of all, however, I should tell you, my vision was indistinct, but I found it arose from the eclipse of the *third* of every object on the right hand. I ascertained this particularly as I went home, because if I saw such a long name as my own, for instance, *A-ber-ne-thy*, in a bookseller's shop window, or any such place, I could see *A-ber-knee*, but I could not see the

thigh at all. Well, I looked with one eye, then I looked with the other, and I looked with both, but still I perceived that the third of every object was eclipsed, on what I may call my right side. Now this sort of case is alluded to by Dr. Woollaston, and he contends that it might be a defect in the optic nerves. Well, I was telling all this to a medical friend of mine—a very clever man, and he said, it was *impossible*. I said, well, I don't know whether it is impossible or not, but I know that what I tell you is true. It afterwards happened that he had a fall from his horse, I believe, or something of that kind, and he had the same imperfection of sight, the eclipse of the objects being on the opposite side. I said to him, there was only one thing I regretted, which was, that when I was in that state I had not *squinted*, to have seen how the things would have looked then. He told me he was convinced it arose from the nerve. But I said, did you squint? O gad, no, said he, I never thought of squinting. But since that time I have been entertained with it often, and often without having had any blow; and I have on those occasions squinted too, and it's just the same. And let those who can account for it as arising from a decussation of the nerve, do it; my own opinion is, that it arises from the *irregular actions of the retina*. You know there are people who see *ghosts*, and *goblins*, and so on; not *blue-devils*; I suppose they don't see any of those, but they absolutely see men and women; you know all *that*, I dare say.

There is a very curious case related, of a man who was a well known character, and a man of sense,—where it was said, he used to see a number of people in the room with him. Now, he himself has described the whole of the phenomenon, and all the adjuncts to it. He has said, after taking a cup of coffee, or tea, or so on, they came into his room in great numbers; and as he got better, and less nervous, he has only seen the arms or legs of the persons, without seeing any other part of them. Now this is all an irregular action of the retina. A gentleman sitting in his library one day, reading or writing, on turning round his head, saw, sitting in a chair, a woman in a red cloak. And he said, how came you here good woman? The woman said nothing. What is the meaning of your being here, woman? No answer was made. You have no right to be here:—Go out of the room. She took no notice of him. He got up and rang the bell for the servant. The servant

came in. Turn this woman out. What woman, Sir? Why the woman in a red cloak. There's no woman, nor any red cloak, Sir. Well, go fetch the doctor for me; tell him I am ill, and I wish to speak to him. The man, however, was not to be frightened by this, because he knew it was a delusion of his sight. Now I have had it so often, that it has been a matter rather of amusement to me, than any thing else. I have stood before a glass, and seen the upper part of my head, and eyes, and nose, very distinctly; but I never saw that I had any mouth or jaw; and I have seen my shoulders very well, but all was blank between my nose and shoulders. Why, now I say, what can you make of this, but that it is *errors of action, or inactivity in parts of the retina?* Now this is a digression; this is no part of the lecture; but I am speaking about the fractures of the ossa nasi, and I am telling you what seems to be the surgery of these cases.

When I finished the bones of the skull. I had a word to say about the *sutures;* and having now finished the bones of the upper part of the face, I have still a word to say on that point, and this is it: All the bones are united together by sutures; and if there was to be an elaborate account of the sutures given, I don't know to what length it would run. But anatomists have contrived to make one suture out of a number of sutures. They say there is a suture which is called the *transverse suture,* running between the bones of the head and the bones of the face; and I have seen dreadful things occur here. There was a poor waggoner, whose head was crushed between two cart wheels going in different directions, on Clapham Common, and his face was literally torn off from the skull. Part of his face hung by the optic nerves. There was no jaw; all was vacant. No raw, half-dissected head, could ever equal it. To give you some idea of the horror the scene presented, I may tell you, the nurses of a hospital fainted when they saw the man. He went to the hospital; went to bed; got up to void his urine; went to bed again; and, in about five or six hours, he died. Now it might form a good subject of inquiry, in a medical society, why he died. He did not die from loss of blood; torn arteries will not bleed. He could not have been starved to death in so short a time. Whatever blood he might lose before, he lost none in the hospital to cause his death. Then why did he die? Now it does so happen, and we have seen, that it is a most wise and benevolent

circumstance, where such an injury occurs as is actually irreparable, the vital powers decline and cease to act, and people die without any seeming necessity for doing so. It is really very curious, but true, that the most extensive injuries are generally unproductive of pain. I say, it seems to be wise and benevolent that it should be so ordered. There are plenty of facts which you will collect in the pursuit of your profession, that seem to warrant metaphorical phrases, such as JOHN HUNTER would have introduced: Nature takes the alarm from something having occurred, apparently without any necessity; or Nature, conscious of the injury, relinquishes the contest, and the patient sinks and dies. But however much this may be a digression, the case goes to induce you to remember the sort of connexion that subsists between the bones of the face and the bones of the cranium.

In speaking of the *Bones of the Vertebræ*, Mr. ABERNETHY said, "There is such an immense quantity of elastic matter in the column of bones supporting the head, that the head rides upon it as if placed on a spring machine; and it is very important that this should be so. If it were not, the head would be subject to perpetual jars. There is a very important case related by Mr. POTT—a case of concussion: An officer standing on the deck of a ship, after some sleet and rain had fallen, began to walk upon it; he slipped, and upon feeling his feet go from under him, he put all his muscles on the stretch to save himself. He came down on his bottom—merely on his bottom, nothing more; but his head was jarred; there was a complete concussion; he was stunned; an inflammation came on, and he became delirious. Now that's a very capital case. The head received no outward hurt at all."

LECTURE XXVII.

DISLOCATIONS, FRACTURES, DISEASES, AND OPERATIONS OF THE JAW.

HAVING described the anatomy of the joint of the lower jaw, I shall next speak of the injuries to which it is liable: such are dislocations, and diseases which occur in it.

And I ask, in what direction you could suppose this joint to be dislocated? Could you suppose it to be dislocated backwards? O no; the *vaginal process* prevents that. Could you suppose it to be dislocated forwards? No, not directly forwards, because you could not pull the *condyles* over in such a way as to do it in that manner. Then, considering the circumstances in which the bones are placed, there is but one possible dislocation can take place; when the condyles are upon the *emminentiæ articulares*, it is possible they may burst the *capsules* and slip over them, and this is the only dislocation that can take place in the jaw. Then the question is, how it is to be reduced? It is commonly caused by an irregular and spasmodic action of the muscles; for persons, either yawning, or laughing, sometimes dislocate their jaw. Then how is it to be reduced? When in this situation, all the strong muscles tend to hold it down. Now the reduction of this dislocation is done upon a principle which I shall explain hereafter. You make a lever of the dislocated bone, raise the chin, and depress the condyles simultaneously. The principle I shall dwell upon and explain hereafter; therefore, I shall not say much just now upon it. The mode of reducing it is simple; you put your fingers into the mouth, and depress the condyles over the emminentiæ articulares, raising the chin at the same time. There is no other way of reducing a dislocated jaw; it must be done by depressing the condyles, and, at the same time, applying a force to the chin, which closes the mouth; and in doing this, by putting your finger into the mouth, you may make the lever. If you attempt to close the patient's mouth, the condyles still lodging where they were, you never will reduce it; but if you depress the condyles, and then close the mouth, then the dislocation is reduced. They say you should wrap a handkerchief round your thumbs, put them into the patient's mouth, and then with all your force press down the last grinding tooth. It may be done in various ways; and there are some who say you had better put a piece of transverse wood into the patient's mouth, then depress, and so on. For my own part, I never found any difficulty in reducing a dislocated jaw in the whole course of my life, and I have reduced a vast number. I have reduced one condyle and not the other, and things of that kind, but I never found any difficulty, so as to require the use of complex measures; never.

However, whatever measure you do adopt, it is reducible

to the simple principle I have stated. With respect to this, I have been in the habit, when lecturing upon this joint, of telling a story I heard related by Dr. M'Laurie, at the first lecture I attended of his. He was an old Scotch doctor; and I do it for this reason, that after hearing the story, I never forgot the principle of reducing a dislocation of the jaw; and the story runs thus. But be it known to you, people who have once dislocated the jaw, are very often likely to do it again; and this I take to arise from the gap in the ligaments not healing, and also from an irregular action of the muscles, which first caused the dislocation, becoming as it were, habitual to it; but I think principally from the gap of the ligaments not healing. We think we have done a great deal when we have put the jaw into its proper place; but we have done only a very little part of the surgery. You should keep the jaw still until the injury is repaired; that is the point in the surgery. But who is it that will keep their jaw still? People wont do it, and therefore the ligaments wont unite and heal. This happens also with respect to the shoulder continually slipping out, and I suppose from the same reason, that the gap of the ligaments has not healed; that there has not been that stillness necessary for its healing. But to the story: there was a major in the army who had the misfortune of frequently dislocating his jaw, and it was an infirmity he cared very little about, for he was generally moving about with his regiment, and when he put it out, the regimental surgeon put in again. But it happened that on one occasion he was fourteen or fifteen miles from where the regiment was quartered, dining with a gentleman, and being rather merry after dinner, laughing heartily, his jaw slipped out; his mouth, of course, remained wide open, and it was impossible to close it, while the condyles remained out of their sockets. His mouth remained open, and no one can articulate with the mouth in that situation. Well, but he made an inarticulate noise, and the host being very much surprised, considered that there was something wrong with him, and sent for a medical man residing in the neighbourhood, whom, if you please, we will for the present call the apothecary. He said, when he saw the major, that there certainly was something wrong with him, that there was something wrong with the jaw, and that in fact, it was dislocated; and he began to *pull* the jaw for the purpose of putting it into its proper place. Now that would take it just

farther from its proper socket. The officer knowing the simplicity of the case, and how it ought to be done, was so enraged that a man should be so presumptuous as to put up a pestle and mortar above his door, and yet not know how to put in a dislocated jaw, that he vented his rage in a most furious manner, but in a very inarticulate way. And, egad, the apothecary took it into his head, that the man was mad; and, 'faith, it was very near verified; for Mr. Pestle's suggestion put the major into a terrible rage, which actually confirmed the apothecary's opinion. He therefore threw him down, put a straight waistcoat on him, left him lying on his back, and then sent him some cooling draughts, and some lotion for the jaw, which was to be applied in due season. The major then found, that there was nothing for him but submission. And after some time had elapsed, he made signs for pen, ink, and paper; and as these were not instruments which it was supposed he could very much injure himself with, they were furnished to him; and when he got them, he wrote on the paper, just these words: "For God Almighty's sake send, with all possible speed, to Mr. So-and-so, surgeon of the regiment." Well, that was considered a very reasonable request, and therefore they sent off a man on horseback for the surgeon immediately. The surgeon came, took off the blister, put in the jaw; and there's an end of my story.

The case is often attended with the most distressing degree of thirst. I remember a maid servant, who had those foolish attempts made to reduce her jaw, by pulling at it, and who waited from four or five in the evening till eleven o'clock at night before a surgeon was got for her, and who immediately reduced the jaw. There was a pailful of dirty water placed in the kitchen, which they had been scouring the house with, and as this happened in the kitchen, the girl put her head right into the pail, drank the water, and turning round on her knees, blessed the surgeon who had reduced the jaw. But they cannot articulate a word during the dislocation. Now it is a simple operation. But you may reduce one condyle and not the other; and I have even seen one condyle dislocated and not the other. But there is a case that looks like it, and yet not of the same nature; nor can I tell you what the nature of the case is: but a person by some action of the muscles, remains with the mouth wide open, and a little awry; the *symphysis* of either jaw is not in the same line. Yet though it is

wide open, it is not so far open but that they can articulate a little; and they can swallow a little. What the plain nature of the case is, I really cannot tell you; but I have seen this case, many, many times in my life, and I have seen people attempt to reduce it by the common process by which they would reduce a dislocated jaw, and without any avail.

Now with regard to this accident, I have partly communicated this to you by way of letting you know what is in my own mind regarding this subject; and I will further tell you of the case of a patient who came out of Essex. The surgeon in the country had tried to reduce the jaw, but had not succeeded; and the man said he would take his wife (for the patient was a female) up to London; and I am pretty sure the surgeon in the country must have understood his profession well, for when the man and his wife were coming to London, he said, "I'll be hanged if any of the London surgeons will ever get that jaw in." I take it, therefore, for granted, that he knew his profession well. However, the man and his wife came up to London, and they applied to a surgeon here—a hospital surgeon, and that surgeon took it for a dislocated jaw, and attempted to reduce it, but did not succeed; and he was excessively irritated at this, and desired to have a consultation as to what should be done. A consultation was agreed upon, and he did me the honour of calling me in to assist him. But as soon as I saw the case, I knew it was not a dislocated jaw, and I said so to the surgeon. He said to me, "Why, what can it be?" "Why sir," said I, "upon my honour I don't know; but you may satisfy yourself it is not a dislocation. Trace the jaw up to its proper socket; you may do it in your own head, and you will find, from the condyle of the jaw, that it is not a dislocation." "You assuredly are right; it can't then be a dislocation?" "No, it is not." "But what can it be? or rather can you tell me how we can put it right?" "Oh yes, I can do that readily." So I put my hand upon the woman's head, placed my other hand under her chin, and simply shut her mouth, without at all depressing the hind part of the bone, for that was not necessary; I tried to bring the symphysis of one jaw to accord with the other, and to shut her mouth; it required no great force to do it, and when I did that she talked with the utmost fluency, and indeed, in her way, with great gratitude. It was all right then.

DISEASES OF THE JAW.

Now, with regard to diseases of the jaw, I have seen many, many cases of diseases of the jaw; and those diseases go on, because people won't keep their jaw quiet. I remember telling a lady so. The ligaments get diseased, and the jaw slips about, cracks and bulges out on one side. Now all diseases that I have seen appeared to me to be constitutional diseases; and I have been surprised to see how readily they have recovered by confining the jaw to the greatest extent the patient would admit of; that is, by putting a *splint* under the jaw, tying it up, and telling them to drink, out of the spout of a tea-pot, strong broth, and so on, urging them upon the necessity of keeping the jaw quiet, and taking at the same time, a little medicine, which would keep their bowels right, for they were all wrong too; and it seemed to me they soon recovered. By having been careful of the jaw, they have had no slipping of the condyle again, which was a great annoyance to them before this plan of treatment.

FRACTURES OF THE JAW.

Then the jaw is liable to fractures, and the fractures are of two kinds, perpendicular and horizontal. Perpendicular fractures are continually happening; horizontal ones are of a much more rare occurrence. A person receives a blow on the jaw, which breaks it. He may have another blow on it, and therefore it may be broken in two places perpendicularly, the intermediate part being loose; or it may be broken, with one blow, into two pieces. Now what's to be done? There have been instances where the gum has been lacerated, and there has been a great deal of bleeding from the artery, which runs in the gum. Well, what is to be done in such a case? Why, nothing but what the common principles of surgery demand; to put it right; set it again, and keep it still. You must put the bone into its old situation, and then you must keep the jaw still. But here is the difficulty: what splints would you use? what splints have you for the fracture of the jaw? You must make them for yourselves; there are none sold at the instrument makers. Then how would you set about doing that? Get some thick, strong pasteboard; put it into water till it is soft, then shape it to the jaw; notch it, cut it, make it

into the shape of a case for the jaw; make a bandage, compressing the case and the front of the jaw together, and keep them so, steadily compressed; the jaw being entirely motionless, the pasteboard gets dry; and then it forms a proper splint for the jaw. Now there is the greatest comfort in this. I have seen a fracture put up merely with a bandage, and the patient has been very much distressed indeed; but when I have put him on one of those pasteboard cases, he has expressed his comfort in the strongest way possible. It is sometimes necessary to varnish the pasteboard, for there may be a cut, or a wound in it, and then it will be no longer a splint; therefore it is sometimes necessary to varnish it. I have to tell you what a comfort it is; I think, that however irregular the jaw may be, even where you cannot get it into its situation, the case will turn out far more creditable than you expect. The action of the muscles, the gentle pressure of the patient's tongue, and one thing and another of that kind, will bring the bone more into its place than you could imagine. I will tell you of one of the worst cases of fracture, I ever knew; it was in the case of a very large man, called *George the Brewer*, who fought with a little man, but a man of infinitely more science than himself. The little man's conduct appeared excessively strange to all the *amateurs*, for it was evident that the big man laid himself open very often to the little man's blows, and yet he did not strike him; at last, however, he struck him in the jaw, and broke it. The big man was a man of great courage, and he still went on fighting; and the little one danced round him for some time, then struck him again on the jaw, and broke it again, and every time the little man struck the big one, he hit him just in the very same place, until he broke it in a most horrible manner, until this big fellow was gulping down large quantities of blood, and at last those who had bet in favour of the big man obliged him to give in. Well, the bones could not be got in their places! inflammation ensued, and matter came from the mouth; but still the case did well at last, and healed, leaving very little deformity; not at all the deformity that might have been expected.

Now as to the *horizontal* fractures; Oh, these are excessively vexatious. When you see the *alveolar process* broken, and the teeth falling in, what then is to be done? I have seen contrivances of a gold wire being used for the teeth; but surely a

dentist would be the man to give relief, by putting in a wire of gold and fixing it to the teeth behind. You know this is the way in which they endeavour to bring children's teeth straight, which are awry; they endeavour to lessen the wry, by bringing round a circle of gold, and fixing it to two standard teeth behind, and I think that that would be the best way of managing the horizontal fracture of the jaw. Now I was educated at a time when *mercury* was very much used for the *venereal disease*; when excessive salivation was considered necessary, and it was thought a great wonder how it should happen that part of a man's jaw should come away, and part should remain. The *periosteum* quitted the dead bone, the dead bone came away, and still there was no vacancy, no loss of osseous matter in the mouth. A surgeon once told me what he thought a very curious thing, that all the front of the jaw came away, and yet that the teeth did not. It curiously happened that the teeth sunk down into the new formed jaw, so that the patient lost a great part of his jaw without losing his teeth.

OPERATIONS OF THE JAW.

The jaw is liable to peculiar diseases, there being a canal running through it; there may be depositions or morbid growths in that canal, and the jaw will expand in a very considerable degree. You will find, too, that there have been cases of diseased jaw, in which a portion of the jaw has been removed—a very considerable portion of the jaw removed. This has been done by a gentleman in America. A very difficult operation it is—taking away a considerable portion of the jaw-bone, which, if it had been allowed to remain, would have spoiled the rest. However, these relate to new operations of surgery, which I have no personal knowledge of.

ON THE DISEASES OF THE SPINE.

It is said there is such a thing as a *concussion* of the spine, the same as a concussion of the brain; a genteel name used to conceal the nature of the injury. But, if it be a question, whether there ever was such a shock given as to impair the functions of the spine, I can only say, I believe it is possible. I have seen people jarred very considerably in their spines, and unable to move their lower extremities, and in a great state

of nervousness; but who have recovered so quickly and so perfectly, that I believed there was no other injury but jarring. These are very rare cases, however. In general, when a man meets with an injury in his back-bone, depriving him of the use of the parts beneath it, the back is usually broken. But, can it be dislocated? That's the question. And the answer to this question must be—it can scarcely be possible that one *vertebra* can be parted from the other, on account of the *articular processes* being fitted as they are. There is one part of the back, however, where a dislocation may take place; that is where the articular processes are very oblique, in the neck. A force applied to the head, propelling it forward, and another to the lower part, propelling it backward, may cause such an effect, as that, possibly, the articular processes may slip off one another. But I have examined a great number of those cases, and, in every one of them, fracture was found; either fracture of the rim of the vertebræ, or fracture of the articular processes, or fracture of the body of the vertebræ. If a person meets with a blow which occasions an irregularity in the *spinous processes*, and, after the accident, the limbs below become paralytic, I say you may be assured the vertebra is broken. And here I have to mention, that the part which projects is in its natural situation; it projects, because the other part has been driven from its natural situation. There was formerly said to be fractures of the rising end of the bone. Now there is no such thing, excepting in one case. Wherever one part juts out, that part is in its natural situation, and only juts out from the other part being out of its natural situation. Well, granted; a fracture or dislocation. What is to be done in such a case? Can you set it, if it be a fracture; or can you reduce it, if it be a dislocation? How? Here are the *viscera* before you. Where can you make the force? Dare you pull it? Dare you make extension to straighten it? Egad, I don't know what *you* might dare to do, but I should be afraid of it. What, with all the muscles of the back restraining your force! you may jerk the *medulla spinalis* asunder. I do not know that you can do any thing more than keep the parts still; for if you were to suffer the parts to move about, then pain and inflammation would follow, and I have actually seen matter formed in such a case where the parts have been in motion and moveable. I do not know, therefore, that you can do more than keep it still, in the expectation of its healing, but leaving

such a defect in the canal for the *medulla spinalis*, as that very probably it would never act properly again. Most of these cases are dangerous cases. I have, however, seen several persons recover; yet they are cases so generally fatal, that I am convinced an assembly of old surgeons would doubt that there was a possibility of recovery in any case; and yet I have seen many cases of recovery, even when the fractures were in the neck.

I remember a gentleman in that situation, who was paralytic in his arms, and in the whole of his body; but we kept the head in a proper situation, kept it steady, and treated the case according to the common principles of surgery, and the patient recovered and afterwards went about. You may say there was no fracture. I can only tell you, that he fell from his horse with his neck under him, and that there was a seeming projection, though not very distinctly felt, as of course it could not be in the vertebræ of the neck; and that he had no use of his arms or legs. I was sent for into the country, a very considerable distance, to see the case. I put him into such a posture, as that his neck was supported, and never permitted to move from one place. By keeping him confined for a certain time in that way, the bones were knit together; we then got him up, and by stimulating the limbs, and rubbing them, and so on, he came about by degrees. Now I do not know that you can do any thing more in those cases. In the majority of them the depression is so great, that the medulla spinalis never recovers. And here I have been in the habit of telling a story of a case of this kind, where the case was properly treated, very properly treated, by a gentleman who was a student at this hospital. He was a wild young fellow, and was riding with another wild young fellow like himself, at a furious rate on Epsom downs, at Epsom races. After they had been riding about for some time, the one said to the other—for God Almighty's sake stop your horse, or you will be dashed into eternity, or some words to that effect; and suiting the words to the action, he pulled in the reins of his horse, for the fact was, he had seen a gravel pit just before him; the other having less fear, instead of stopping his horse, spurred him on, made him leap, and cleared the pit. The horse, however, that had been pulled in, tumbled into the pit, threw his rider, and broke his back. Well, the surgeon returning to his companion, got a shutter, upon which he laid him, had him conveyed to an inn

in the neighbourhood, and attended him very carefully. And after a confinement of about two months, when it was presumable the broken parts had recovered, and the torn ligaments had been re-united, he was brought to town. The accident was about the middle of the back, about the middle of the *dorsal vertebræ*. There was a projection. It was curious to see this young man, who was very gay and volatile, playing at cards, and entering into conversation with any body who called to see him; but he was living with the upper part of his body, without being conscious that he had the other part of it. And it was proposed as a question, could any thing be done in this case? I said I cannot tell you, except taking out the rings of the vertebræ. But that was considered so hard an operation, that it was not at all contemplated, nor does it at all follow that it would have been successful. Yet such a thing has been done, and done in London, and at the time too, this very accident happened; at which time I would not undertake it. I would draw a parallel between it and the fracture of the skull, and I would say, let it alone until all the inflammatory feeling is gone, until you were sure you would have no more inflammation than what would result from the operation itself—I say I should not be inclined to undertake such an operation, just at the time at which the accident happened; and it by no means follows that it would be a successful operation even afterwards; because it very often happens, that the medulla spinalis is broken, jammed, and crushed to pieces. Nay, sometimes torn asunder. They are most horrible cases. In some instances, where the injury is of such a nature as that the patient has to lie on his back, the buttocks mortify and become all over sores. However, the treatment is, to put it as right as you can, and to keep it steady. The first part of the treatment is, to guard against inflammation, and to allow time for the reunion of the parts; then the next object of the treatment is, after allowing time for the repair of the vertebræ as much as possible, what we may call stimulating rubbing and electrifying the limbs, to endeavour to give life to them. But they are very, very hopeless cases. There are fractures even of the vertebræ of the neck. There was one case where the vertebræ of the neck was broken, and it was a curious one · it happened in a child, and no persuasion whatever could induce that child to take away its hands from its head; it held the head steadily, with one hand on each side of it, as if conscious of its injury

The child died, and the vertebræ appeared broken. But I should tell you that fractures may take place there, which is the most important part, and yet the patient may recover.

DISTORTION OF THE VERTEBRAL COLUMN.

These are not diseases, they are deformities. It is customary to attribute them as arising from the same state of health which rickets arise from; but I don't think that that is a fair way of viewing the subject. I call upon you to observe what slight things will produce a deviation from tbe proper direction of the vertebral column. You never saw any one who had a complaint in the hip who was not distorted in the back. The standing upon one leg will occasion a distortion in the back; I cannot stand upon my left leg, for instance, upon any other terms than those of inclining to the right; and if I am to continue in this posture, such is the constructure of the intervertebral substance, as to occasion a springing out, an augmenting of bulk on the one side; so that this is a cause rendering a temporary distortion, and in continuing to recline in that way, the distortion becomes permanent. Now, therefore, the habit of standing on one leg will occasion young people to grow awry. If I were to lean over, as it were, upon my right shoulder, what would be the consequence? Why to balance the weight of my body, I must twist my spine, and incline the loins to the left side, and therefore cause a deviation to the left side. Now, suppose a curvature to take place, and that curvature was on the right side; if the weight of the head were put on it, is it possible that the head could be supported in a straight line from the *pelvis?* It is not possible; and the first curvature induces another curvature, and the next another, and so they go on, and they are all consequences of one original curve. Now, I say, it is very curious to attend to what slight causes will sometimes lead to the original curve, and causes which we do not easily perceive. I have been in the habit of telling a case, when lecturing on this subject, that occurred in my own family: a child, young and active—and I do not see those curvatures happening except where there is some constitutional disorder—I say a girl of this kind became awry; I saw the child when I had an opportunity, and I observed that she had got one shoulder-strap very often down, which she was continually pulling up, and I said to her mother,

"If you allow that to go on, that shoulder will become warped, as sure as you are alive. Let the gown be made in another way, and do not let her always be twisting herself to keep up that shoulder-strap." The mother said, "O, don't take any notice of it; and let it pass on for a time." Then I began to swear about the fashions, that had been the cause of those shoulder-straps being made in such a way. But, in the course of a month, the reason appeared why the shoulder-strap did not stop on that shoulder; it appeared that that shoulder had sunk down about an inch lower than the other. I then told her to walk before me, and then to stop, and I observed her particularly, and I found that she was in the habit of standing and resting always on one leg. I then began to ask her if there were any sores about her feet, any sores about her toes, any pains in her leg, or any thing wrong with it, and she said no. I said, "I should like to see you hop; hop round the room, and then stand;" she did it, and did it very well. "Now," said I, "hop round the room on the other leg;" she attempted it, and she took a few hops, and then she was obliged to walk, because that leg was not capable of supporting her. She had been clearly in the habit of standing on the one leg, and by having disused the other, it became a weak limb. Now, by setting her frequently to hop round the room on the weak leg, very frequently, until she could hop on it as well as the other, both shoulders became of the same height, and the shoulder-strap of her gown never slipped off again. But I am quite sure she would have been awry if she had not lived in a surgeon's house.

Now, you will say, what's the use of bothering us about these things, they are not diseases? No; it is very true, they are not diseases: but I can tell you that they are very important and that you will be more bothered about them than about any thing else you may meet with. O, there are young ladies, whose parents would give any thing to have them set straight. Now I tell parents, that I have any concern with, that they must avoid all the causes which have produced the original curvature; I try to suggest what those curvatures are, and then tell them that they are to lie down as much as possible, and to take the weight off the pillar which has yielded, because if the column has yielded, whatever weight is placed upon it will considerably increase the curve. I hear medical men say, you had better put weights on your head and carry them about

with you, because they say, people who carry large weights on their head or shoulders, such as milk-pails, have straight backs. But I say, weight on the head must be very injurious, therefore I tell them to lie down, to avoid the causes which increase it, and to lie in a horizontal way. Many say you should lie in a manner so as to extend the vertebral column. That can never be done. Lie down, therefore, in a horizontal way, and you take off the weight from the curve. But I would by no means deprive the patient from taking that degree of active exercise which is conducive to health, because I say there is something wrong about those patients; there is something about them that induces muscular rancour, or muscular irritability. It is an object to give all possible energy to their muscles; it is an object, that they should have active exercise, and an object to give them all possible strength, that they may have the power of supporting the head and weight. Then I say, lie down. But people will ask me, "Can't you do any thing more, Sir?" and I must say, "No, I do not know that I can, unless you choose to be gibbeted. That is sometimes done; it is a fashionable way of going to work, and is what I call gibbeting. This was first proposed by Mr. Vacher, and the plan is that of taking the weight of the body from the pillar that supports it. A most horrible annoyance it is to the patient. O, the pressure against the chin, and the under part of the jaw is dreadful; it produces a thickening and ulcerations of the ligament, where it is carried on, as according to principle it ought to be.

Now there is a gentleman, Mr. Cheshire, of Hinckley, in Leicestershire, I think it is, who perhaps understands the principle on which those machines should be constructed better than any body else; yet I have seen patients who have been there, and really there has been no such good done to them as I should boast of; but he certainly does support the principle, and that principle supports the weight of the body, but greatly to the annoyance of the patient, and is productive of the effects I have been describing, occasioning abscesses and deformities, and thickenings, and so on; but he does it effectually; and the effect too of taking the weight from its proper place is, that by using those machines for years, which they have to do, they cannot afterwards do without them; they lose the power of their muscles, so that they cannot do without them; and therefore, if they lay them aside, they have to lie

until they recover the power of their muscles, until they can properly support their weight. But I cannot say I like this system at all, therefore I don't give my mind to it, but I advise all my patients to avoid all causes which might effect the original curvatures; to take off the weight by lying down, and so on; but the child should not lie down in any constrained attitude. In a boarding-school, you will see the mistress having all the girls lying, like a pile of soldiers, on boards turned horizontally, and there they lie for about half an hour; that is a short time; but they can do nothing when they are lying in that position, like corpses. Now I maintain that they may do this on a rug or carpet; and I say, why cannot your child lie down in that way, which is the ancient fashion; but, to be sure, the other is the more modern. But I know that weight on the upper parts of the body must tend greatly to increase the curves; I know also that people will become straight if the cause of the curves is removed; because I have found it to be so in the cases of distortion of the vertebræ from wry necks, as well as in other cases, by such treatment as I have been endeavouring to explain to you.

I remember that Mr. HUNTER used to say a great deal on this subject in his lectures. He used to say, he was convinced that people got awry by the endeavours of parents to keep them straight; that parents were continually watching their children, and making them sit in a particular attitude; and that those children so watched, when unobserved, would naturally sink into another way of sitting, to have a little ease. Besides, that is keeping in action one set of muscles, and not allowing the other to act at all; whereas, every set of muscles should be kept in action. He said, you don't see boys go awry any thing like so often as girls, nor yet girls in a low situation of life. I remember when they wore stays to prevent this wryness; and stays, you know, are good things to let a person slip aside, without that slipping being seen; and that went so far, that I remember the time when it was a bet, that upon an examination, in an assembly of well brought up girls, you would not find one girl among them straight, but that they had all a crick in the hip, or a slip in some part or other. I remember Mr. HUNTER saying, you should dress your children lightly and loosely, let them run about and exercise all their muscles equally, and then they will not grow awry. To this parents have a sort of objection, which is, that children will become round shouldered,

and so on. Now, I have even endeavoured to refute that absurdity, by saying, if children were allowed and suffered to do as they pleased, the body would be formed according to that pattern which nature designed it should be. If two round shouldered parents had a child with a perfectly flat back, I should really suspect its legitimacy; and one might wonder people had not hit on a plan of making Roman noses for children. One cannot counteract nature. If the body be healthy, it will be well formed; but it will grow according to nature. But, said Mr. HUNTER, if it be necessary, from fashion, and so on, to carry the person in any particular manner, this habit may be obtained at any period of life; and quoting this instance:—You see a plough-boy, while plodding away at the plough, an awkward fellow; but he enlists; then he is put under drill of a serjeant; and then observe with what care and precision he marches, after he has been under that serjeant for a time! Now that shows that if the body is well formed, it may carry any fashion. But there is certainly no counteracting nature.

These are not diseases, but deformities; and yet points of great importance. In some of the cases, there is no room for the lungs on one side of the chest. It is this which first attracts people's attention: Sir, I want to consult you on my daughter's case; her shoulder is growing out, or her breast is on one side. Oh, the cause is in the distortion of the vertebral column; but that they cannot credit; but that you will find to be universally the fact, I believe.

Now I go to speak of *curvatures from disease:* curvatures produced by a diseased state of the vertebræ. The bodies of the vertebræ are of a spongy substance; the ends of the bones are also spongy, and scrofula may arise; and a curvature of the back be produced from a bend forward, and where many of the vertebræ are gone, then one of the vertebræ sticks out, and makes a very acute angle; and this angle, this sharp projection, is one cause of a surgeon's knowing the nature of the disease. Now, in the commencement of the disease, the question is, whether it begins in the bone or in the intervertebral substance? Now, I really cannot tell: sometimes it is in the one, and sometimes in the other, in all probability, it is very rare that we have an opportunity of examining it at an early period. When scrofula begins, the disease goes on to ulceration, and the bodies of the vertebræ are crushed by weight.

It is right to tell you, too, that you may have the disease without curvature, and you may have curvature without the disease.

Now, as to the *treatment:* unquestionably you must keep the patient still; motion of diseased parts is always exceedingly injurious; the greatest good is done by keeping diseased parts in an absolutely confined state. Ease is not only good in counteracting the morbid action of the diseased bone, but in counteracting the disordered action of the medulla spinalis itself, which is the cause of the disorder; and I believe I need not enlarge upon this, because it is obvious to common sense, that ease is likely to be of efficient good. But in all scrofulous diseases, a great deal depends upon the management of the patient's health; the correcting that state of health which gives rise to scrofulous actions, must be considered a most important part of the treatment.

OF THE PELVIS.

I beg you to remember, that the *posterior superior spine* of the *ilium* is met with half way down the *sacrum*. It is most important that you should remember this, for I have seen surgeons misunderstand the nature of accidents happening here. A man has had some fracture of his sacrum, and the surgeon, upon examining the pelvis, has traced the crista of the ilium, and thought it went on to the vertebral column. Indeed he might well think so, because there is a strong ligament going from the transverse process of the last lumbar vertebra to the posterior spine, and indeed, in some degree, from the one above, which seems to oppose some resistance, and leads to a belief that the crista goes on to the vertebral column. But recollect that it goes half down the sacrum. If you have any irregularity near the ilium, you may be sure it is from the sacrum, and not from the vertebral column.

Having now minutely described to you the bones which form the boundaries of the pelvis, I next request your attention to the bowl of the basin. They say it is a basin, and a very curious one it is, with a large hole at the bottom of it. But it is a cavity of great importance, and you should be well acquainted with all that relates to it; it is a cavity of different depths in different parts; that is, suppose I put an instrument in at the *os pubis*, and measure the space to the *brim of the pel-*

vis; I do not find that it is more than one inch and a half; whereas, if I put an instrument in at the *tuberosity,* and measure to the brim of the pelvis, I find that it is four inches; and if I put an instrument in at the *os coccygis,* and measure to the brim of the pelvis, I find that it is six inches; therefore the cavity is of different depths. Then the *forms of the apertures* of the pelvis are of consequence to be known. With respect to the upper aperture, they draw a line from the pubes to the sacrum, and say it measures four inches and a half, and from side to side, and say, it measures five inches and a half; then they draw a line diagonally, and say, it also measures five inches and a half. Of course the pelvis is of different sizes in different subjects; the *male* I know to be very irregular in its size, but it is not so with regard to the female, that is more uniform; but there are women with larger and smaller pelves. When you speak of those dimensions you take the average of different cases, which is what they call *a standard pelvis,* measuring four inches and a half from pubes to sacrum, and five inches and a half from side to side, or diagonally. Then, upon examining the inferior aperture, they say it is of the same size, but with the dimensions reversed: they find that it measures from sacrum to pubes, five inches and a half, and about four inches and a half in the transverse direction. Now children are born with different sized heads, but there is something of a standard for them, and I have now one in my hand which, if it be measured from the projecting part of the face to the back part of the head, it will be exactly five inches, and measuring it across it will be four inches and a half; so that there is just *half* an inch to be allowed for soft parts in the pelvis; and the midwife teacher will tell you how many ways the head may pass; I have only to tell you, that it cannot possibly pass down without turning.

It is also important for you to know that the *axis* of the abdomen are very different—are totally discordant; so that, in cutting for the stone for instance, it is of much consequence that, in introducing your instrument, it should not be too much depressed on the side of the os coccygis, nor yet too much elevated; and I take it to be important too, that you should put the patient into a situation in which you will be able to know precisely the situation of the os coccygis. I was educated in the old school, you know, and there I was taught that the patient should be put into the situation of half sitting and

half lying, so that you have a horizontal plane; and if you put your patient in that way, you will have the best opportunity of examining the *uterus* when you expect disease. Indeed it is necessary to put the patient in this way, that the uterus may descend more, and that the disease may be more palpable.

LECTURE XXVIII.

ON ACCIDENTS OF THE SHOULDER JOINT.

HAVING described the articulation of the bones of the shoulder, I shall proceed to speak of the *accidents* that befal this part of the body, and their treatment; and I begin with a *fractured collar-bone*, which is a very common occurrence. When a man's collar-bone is broken, what happens? Why, that the weight of the arm will drag down the scapular end of the collar-bone, and the scapula downward and forward; and, therefore, the sternal end of the collar-bone will seem to stick up in a contrary direction, upward and backward. Now, then, there is the rising end of the bone again; and can you depress it? No: or should you try to depress it? Certainly not; it is in its proper place. And how do you set the collar-bone? By raising the shoulder-bone upward and backward, and then the two broken ends of the bone will be applied together, and the bone seems as if it had never been broken at all. Set the patient in a chair, or some such place, take hold of his shoulders, draw the broken one upward and backward, with your knee placed against his back, and then the bone will seem as if it had never been broken.

Treatment.—Having put the bone into its proper place, the difficulty is with respect to the mode of treatment. The difficulty is to keep it in this situation, and that is a great difficulty.

The object must be to counteract gravitation, that continually operating property. It is a great difficulty to make a bandage so as to support the arm, without drawing down the scapula and collar bone, and without galling the patient. The bandages which are used for this purpose will be hereafter described to you. For my own part, I like the bandage which

a man of the name of Brasdor, I think, invented for the dislocated collar-bone; it is what is commonly used in this hospital; it is a bandage not unlike what you may have seen young ladies wearing, a square sort of thing, fixed on the back with straps, to draw the shoulders backward and upward: however, you shall see the bandage.

Now there is one kind of broken collar-bone which is very difficult to manage; in general they are very easily replaced, and you may replace them very accurately; but the difficulty is in preserving the arm in the proper situation, and in keeping the bones steady; it is a difficulty, however, which you are called upon to surmount, and which you may surmount. It would be a very annoying thing indeed, if a young lady who had once broken her collar-bone, was afterwards to be seen going about with a large lump on it; it would be a perpetual proclamation to the world of your clumsy surgery; but that may be prevented, and ought to be prevented. There is one case in which I could excuse a surgeon, if he did not set a broken collar-bone right; and it is where a person is pitched, perhaps, off the top of a coach, and falling on his shoulder, he breaks the bone, and drives the scapular end of the collar-bone under the sternal end. It is then exceedingly difficult to get it out, and to put it right, and therefore I say I could excuse such a case, even if there were some little irregularity in it. You are obliged to make extension to get it out, and you are afraid to do it because the sternal end sticks out, and you are afraid of the skin giving way, and of there being a compound fracture.

Dislocation.—I speak of the fractured bone first, because it involves the treatment of every other accident occurring in that part; but suppose the sternal end of the collar-bone was dislocated, and thrown downward and forward on the sternum, what are you to do? How are you to reduce the dislocation? Simply by drawing it backward. By that means you put it into its proper situation, and if you apply this bandage, which I have been speaking of, and keep it where it should be, then the torn ligaments unite, and the case does exceedingly well.

There have been instances, but they are exceedingly rare indeed, of the collar-bone being dislocated in a contrary direction; that is, backward instead of forward. They are exceedingly rare cases. Sir Astley Cooper mentions an instance of the kind, but it is a case that I never saw. Well, in the dislo-

cation of the collar-bone at the scapular extremity, the case is the same. The same treatment is likewise applicable to the scapular end of the collar-bone, in a dislocation; for suppose it to be dislocated, and to ride over the *acromion*, how can you get it again where it ought to be? By no other measure than by pulling the bone upward and backward, and then they apply themselves. Indeed, it was for this very case that that French gentleman, whose name I have mentioned, BRASDOR, invented the bandage I have spoken of. And with respect to the efficacy of the bandage in such a case, if it be efficacious in that case, it must be efficacious in all. I have been in the habit of relating the following instance which occurred to a gentleman who was a pupil in this place: he was a very tall man, and one day he went out skaiting, instead of attending to the lecture, as he ought to have done; when on the ice, his heels flew from under him, down came his head, and he dislocated his collar-bone. He did not like to mention this accident, and therefore he got some of his friends to put him on a bandage. However, the bone was not in its place, consequently he was very uncomfortable, and he asked me to see it, and I put him on this bandage; but this young man had a disposition to pulmonary affection; and from the time he met with the accident, having been stripped in the cold when he was warm, he had a violent cold upon him, which settled on his lungs, as the people say, and he was so horribly ill that he was obliged to be bled very largely: he was attended by the physicians of the hospital, and, in short, his life was despaired of. All thoughts about his accident were lost sight of, in the attention which was directed to the more important object, the salvation of his life. However, he got better, and then we began to think about what had been the fate of his shoulder; and I will venture to say, that if that man were now present and stripped, nobody would ever be able to assert, from any thing they could observe, that there had been a dislocation at all; notwithstanding that, the collar-bone stuck up when I first saw him, as if it had been coming through the skin. Now the treatment of all these cases is the same, but not so in the case of a fracture of the *acromion*. If a man breaks the acromion, the arm does not drag; the scapula still retains its place; it is tied by the ligament to the coracoid process. How then are you to treat this case? Why, if you are to treat it scientifically, you would keep the patient with his arm continually

extended in a horizontal position; but this would be a very awkward position indeed; and it has been found to do exceedingly well to put a small pillow merely under the arm, for the purpose of relaxing the deltoid, and to put the arm in a sling. It is an accident which does not require any specific mode of treatment, nothing more than what common sense would direct. Some people would say, that even the pillow is unnecessary. Now here I have always felt it a duty to beg of gentlemen to advert to the propriety of examining accidents with gentleness. There are some people who pull a man about as if they could not be satisfied that a man's arm was broken, unless they hear the bones cracking against each other. A poor devil is in a terrible fright when he has a bone broken, because he has heard of the setting of bones being attended with great pain. Now you should endeavour to soothe him, by telling him that he is not to be hurt; and then having undressed him, you draw your hand across the collar-bone, and then across the scapula, and you find all in their proper places; or if any are broken, it would be ascertainable by the least touch. Then as to the os bronchii, put your finger on each side of its head, and move the arm; and if it moves, then all is safe there; but if it does not move, then it is broken. Well, then, all may appear to be sound, and yet the patient may say, "Oh I am sure there is something broken, because I heard a cracking." Oh it may be the coracoid process that may be broken, and how do you attend to that? Why, it is necessary to know the muscles to ascertain it; and the treatment of a broken coracoid process is also to be known from your knowledge of the muscles. You must take care that the muscles going from the coracoid process do not move the part. You must put the part into its situation, turn the hand round, bring the elbow and shoulder a little forward, bend the arm across the breast, then no motion of muscles will disturb the coracoid process, and in due time an union will take place.

ON ACCIDENTS ABOUT THE SHOULDER JOINT.

In considering the great obstacles that exist to any dislocation of the shoulder-joint upwards, you cannot suppose it possible to happen; or if it did, it would require no particular treatment, for the weight of the arm would draw all the tendons down to their proper places. To draw the shoulder upwards,

so as to dislocate it, you would have to displace the strong tendons, break the *biceps*, break the *acromion*, and so on, so that it is next to impossible; and if you did meet with this accident, the weight of the arm would cause them again to descend.

A *dislocation* means a displacement of a bone, in consequence of laceration of ligaments without fracture. It is not indeed etymologically a dislocation, for every thing would be dislocation in that case. Well, a dislocation directly forwards, or backwards either, is not likely to happen, on account of the strong tendons attached to the tubercles of the shoulder. I have never seen any of those cases; Sir Astley Cooper speaks of a dislocation backwards, that he has seen, but I have never seen any. They must be exceedingly rare. The common dislocation of the os brachii, is that where it bursts the orbicular ligament below, and slips out of the socket. Force, commonly tends to produce this; a man may fall with his hand and arm outstretched, and the re-action, or what he strikes on, may drive out the os brachii; and if no more is done, it lodges by the *inferior costa of the scapula*. But the violence which caused this dislocation may operate further, because it may drive the bone up under the *pectoral* muscle before, and under the *latissimus dorsi* behind. It may be primarily forward and consecutively backward; and this is what most frequently happens; the bone is thrown out where it is most easy to be displaced. The os brachii may slip out of the socket above the inferior costa of the scapula with very little force; in that case, you may depend upon it the arm will stick out from the side, and that has been set down as a diagnostic of dislocation, though there are plenty of cases where it does not. Though it may slip out with very little force, it never can be thrown under the pectoral muscle while laying, as it commonly does, near to the collar-bone, with the ligament all rent around it. I have seen some dissections of this kind; they are not commonly to be met with, because if a man has a dislocated shoulder, it is put in again, or if it is not, it does not kill the man; and in the cases which I so examined, the tubercles were actually torn off from the bone, and the tendons were much lacerated. When it is in the simple way, and you raise it up, it goes into its place with *a crack*, and that shows the integrity of certain muscles; but no one ever heard any crack where the dislocated bone was under the collar-bone; it goes

in inaudibly then. And many times have I pulled longer than what was necessary, because I neither heard nor felt any sensation by which I knew it had gone into its socket.

Treatment.—These are the circumstances under which you find dislocation of the os brachii, and then the question is, what are you to do in such cases? You may depend upon it, that the ligaments are widely lacerated; it is not likely that the broad head of the bone should force its way through, without making an ample laceration of the ligaments. There are some people who say, the ligaments may bend over the head of the bone, and oppose an obstacle to this; but this is only an opinion, I believe, hazarded to account for their not having been able to reduce a dislocation; it has been an excuse which they have framed in their own minds, to account for their want of success. Then, if there be no obstacle at all on the part of the ligaments to oppose a reduction, I should be glad to know what prevents us, in the living body, to do what I am now doing, taking the bone and putting it back through the rent of the capsule into the socket? Why should we not? There is only one answer to be given, which is, that the muscles will not allow it. They are all in a state of irritable action, and, in consequence of some accident having happened, they seem afraid of the least thing taking place. If a person could will their relaxation, the surgeon could as readily do this in the living body, as I now do it in the skeleton; but who can? There is no surgeon, however philosophical his mind, who could do it. Then if it is the muscles, and the muscles only, that oppose attempts to replace the bone, what have we to do? We must either try to deceive them or to elude their vigilance; to slip the bone in, when they are not aware what we are going to do, or we must overcome their power. Now then, is it possible to elude the vigilant action of muscles? Oh! yes, it is possible. I have reduced several dislocations myself, by a sort of legerdemain. I should, in that case, say to a patient, Egad! I do not know what is the matter with your arm; I do not know whether it is dislocated or not, but you hold it so tightly I cannot move it, can't you leave your elbow to me? I will not hurt it; let me move it. Oh! now you are holding it. Does the motion hurt you? No, it can't do that. Well then, when you have got the patient to let you do this, you have only to engage his mind with something or other—and there is nothing more likely to engage his mind, than to

talk to him about the way in which he met with the accident,—you have only to push it in. Now if you do this, you make a lever of the bone, applying the prop at the same time.

I stated that a dislocated jaw was to be reduced on this principle; but I said I would not at that time explain it. If you move a patient's elbow upward and downward, backward and forward, even to a considerable extent, without putting any prop near to the head of the bone, what do you do? Seeming to do much you do nothing at all; the bone does not move one jot from its situation. But suppose I put a prop near to the head of the bone, and raise the head of the bone, then I raise the head into the socket? this is making a lever of the bone itself, applying the prop as near to the head to be raised as possible, in order to increase the length of the lever in applying the power to the other end. Well, I say, if you can get a person off his guard in this manner, you have just to put your hand up to the head of the bone, depress the elbow, and, crack—it goes in. But don't believe that you will often succeed in this. I will tell you, however, the case where there is the most hope; it is the case of a man who has been pulled at lustily for a considerable time, and who is tired and wearied, and does not mind slight motions, therefore he lets you play about his elbow just as you please. You then put your hand under the head of the bone; and you say, "Well, Sir, but I forgot how you told me you got this accident," and in the mean time you are trying to reduce it, and you do reduce it. Well, suppose you did this, what honour would you acquire? What would the patient say? "O Lord! that there should so many men pull at me, pulling first one way and then another; and there came, at last, a little fellow who cracked it in at once, and a devil of a clever little fellow he must be."

Well, suppose you cannot elude their vigilance, and I say it is not to be expected; nobody, who knew how alert the muscles are, would undertake it. Well then, you must overcome their power; and how are you to do this? Not forcibly—your conduct must be governed by certain known laws; you must be acquainted with the action of muscles. Muscles have great power, but no muscle can act in a moderate degree unremittingly. You know that wagers have been laid as to how long a man will hold out his arm horizontally; and a man wagers that he will hold his arm in that position for an hour. The wager is, that you don't do it for ten minutes; he laughs

at it, and as he is sure to win takes the wager; but ere three or four minutes elapse, the deltoid muscle aches excessively. Oh, it is very annoying; and in a minute or two longer he says, "damn the half guinea wager, I'll do no more with respect to it." I have seen people hold out their arm in this manner much longer than I could have imagined; I have seen a man do it for ten minutes, and that was the longest time I ever saw. To hold out the arm in this way, it is requisite that the deltoid muscle should be continually in action. Well, you have to engage the attention, and then you have to pull; yet you don't pull them violently, or in a way to create alarm or opposition. I should always be inclined to say to a patient, "upon my life you hold your arm so tightly—if you would but give way—only yield a little, and the thing would be done." Now I speak of a case where it is simple, near the cup, and where you have simply to draw it. As to using tackle and pulleys, and so on, in a case of this kind, I laugh at it; it seems like breaking a butterfly on a wheel. You have only to make a man fast round a bed-post, or something of that sort, put a napkin round him, and pull; pull so as you will not soon be weary of pulling. If you set persons to pull in a violent manner they get tired, and the patient gets tired; then they tug, then they slack a little; then they pull, then they slack a little again; and in this way they never will succeed. Oh! it is such a regular pull-baker pull-devil concern, it is quite shocking.

But I say, in a case of dislocation under the *pectoral* muscles, where the bone lies almost in contact with the collar-bone, for it does absolutely lie in that situation, I never then would attempt to undertake to reduce it without having pulleys, tackles, and every sort of machinery at hand, such as should ensure my success; and the way to do it is this: you must have a cord, surrounded with tow, and afterwards sewed up in leather or cloth; loop it sufficiently to slip over the shoulder, bring it against the inferior costa of the scapula, and bind the scapula up; in short, you make a sort of pad, such as people wear who carry pails on their heads. Then you have straps coming from the pad, which should be fixed in the wall with four screws; but let the straps be very far apart, otherwise you would squeeze the patient's body; and let them be fixed so as not to impede the head of the bone coming into its socket; then by pulling at the elbow you will bring it into

its socket. The best way is to put some buff leather round the elbow, and I have always been in the habit of doing it, by making a half hitch, or clove hitch, as the sailors call it; when this is put round over the buff leather, one of the ends will draw and the other will not, the one will wedge or jam, and you draw the other as tight as you please. What does it signify if you should stop the circulation? It does not signify for the moment. You put on two half hitches, and pull the bone down to, or under the socket, towards the inferior costa of the scapula, and take great care that the extension be unremitting. Now, under these circumstances, as soon as the muscles are engaged in resistance, you say to the patient, "Oh, Sir, you are holding—you are opposing, there is nothing that will hurt you; do give way, do yield a little;" and as the muscles become weary, you find the head of the bone to come further and further from under the pectoral muscle; and, if you are using pullies, you say to the patient, "just half an inch more, Sir; just half an inch more;" then, when it is below the socket, good may be done by putting a prop under the head of the bone and depressing the elbow. You may, with your own hands, use this expedient for reducing the bone. I never failed but in one case, and that is what makes me say I never would undertake this again without having all proper tackle, that there should be nothing wrong. There was a man who had a dislocated *os brachii* of considerable duration; I pulled it until it was as nearly in as possible: it was under the very brim of the glenoid cavity. I then said to the gentleman, "half an inch more;" then, egad, the cord broke, and it went back to the very place in which it was before. Well, that fellow never would let himself be pulled at a second time; he said it never would do, and I never could persuade him to try it again.

Unquestionably dislocations may be reduced after a considerable time, but I do not know exactly what time. This is the rule: if you apply this force, and it is no grand degree of force that is requisite, nor does it hurt the patient materially—if you apply this force, and find that the head of the bone separates very considerably from the place in which it was lodged, you have an assurance that there is no adhesion formed. When the bones are out a considerable time, it is possible the torn ligaments may contract adhesion with the parts where they lie, and then the head of the bone won't move from its situation, in consequence of the adhesion. Well, after trying

this, and finding that there is adhesion, you had better give it up, for I do not think you are authorized to rend the adhesion; but if you find, and I do not think there is any great length of time necessary for that purpose, that there is a removal of the head of the bone, then go on with your extension. There are some who say, bleed the patient, and put him into a warm bath, and a number of things; but all this I am inclined to smile at, for it would seem to doubt the principle on which dislocation is reduced; it would seem to imply a belief that muscles could act continually and without being weary, or without giving way, which is not the case.

There is an accident occurring about the shoulder joint, which I have always found to be a very vexatious one, and is very generally found to be a vexatious one; it is what some surgeons would call a *semi*-dislocation of the *os brachii.* Well, with regard to this word, I say, if you allow such definitions, we shall next have it, as in music, a *semi-demi.* Now the bone is out of its socket or it is not; and I say in this case it is not, but it bulges forward. A man tumbles down, falls on his shoulder, and drives the *os brachii* forward, but not out of the socket; he sprains, and perhaps tears, the tendons of the *os brachii;* the head of the bone is sticking forward, but it is not out of the socket. Perhaps many persons might call it a dislocation, but I should say it is not. Now those who would examine it, and say it was not, might say, "you must put your arm in a sling, and I will send you some wash." The patient puts it into a sling; and if you let a man put his arm in a sling, he generally puts it back, which is the very thing to throw the head forward; but put your elbow forward, and the head will recede and go into the socket. Now if the head of the bone is suffered to bulge out in the front, it will prevent the injured tendon from becoming sound again. Well, but I say all this may be prevented merely by bringing the elbow forward; put it in the sling, but let it be brought forward, and let leeches be applied to the front of the joint.

COMPOUND DISLOCATION.

I have had, in the course of my experience, *three* cases of compound dislocation of the shoulder joint, where the bone was not only out of the socket, but out through the skin. Two of these cases did perfectly well, and one did well event-

ually, though after a great deal of suffering. Two were reduced, and the wounds healed as well as wounds could do; but with respect to the other, the wound did not heal, but there was inflammation and suppuration, and a great discharge, with a great deal of suffering, but eventually the patient got well. It was a patient I did not attend; I only saw the case, and I report this from the surgeon.

FRACTURED SCAPULA.

Now I speak of the fracture of the neck of the scapula. In that case the whole joint drops down together; the cup and the ball, and all falls down, leaving a great vacancy under the acromion. This is a case, I will venture to say, any one who sees it for the first time, will mistake for a dislocation. To be sure there is something in the appearance of a case of dislocation, to a person conversant with dislocations, which will serve to enable him to discriminate with respect to the nature of the accident. In this case the arm drops down loosely, and comes in close contact with the body.

FRACTURED NECK OF THE OS BRACHII.

Next, with regard to fractures of the neck of the os brachii, I have seen some cases where the fracture was near the joint; I do not know exactly what the nature of the case was, but we believed it to be the fracture of the neck of the os brachii, where considerable motion arose from turning the end round. There are very serious accidents happening to such parts of the bone, but there are no rules to be given with regard to the treatment of those accidents.

FRACTURED OS BRACHII.

It might be affronting you to speak of the fracture of the os brachii—that is very simple indeed. But I say the grand rule is, to keep the elbow joint under the shoulder; keep it steadily fixed in its proper situation, and put two splints to it.

LECTURE XXIX.

ON ACCIDENTS OCCURRING ABOUT THE ELBOW JOINT.

I SAY, that a force which drives the *ulna* inward towards the body, so as to dislocate it, would have to force it over a high ridge, and that the elbow is not likely to be dislocated in that manner; not near so likely as to be dislocated in a contrary direction. Say that a force was to be applied to it, shoving the ulna out from the body, then that might push the *radius* off its tubercle, and cause the ulna to go into the place which the radius ought to occupy. This is by far the most frequent dislocation of the elbow. In every injury done to the elbow-joint, the injury is succeeded by such a great degree of swelling, as to obscure very much the outlines of the ulna; and this frequently prevents injuries from being treated as they ought to be. It is therefore necessary to fix your attention on some grand projection of the bone, which no swelling can conceal, such as the *olecranon*, which is one, and the *internal condyle*, which is another. If the ulna is shoved out of its place, you will easily know it; and if the radius is not where it ought to be, of course the radius cannot be in its proper situation; it must be thrown off upon the front of the *os brachii*. Now if you distinguish those two things, which you can do, you will know that the bones are dislocated; but you have this additional assurance, that you can neither bend nor extend the arm; the fore-arm is generally bent to a half right angle, and you cannot either extend or bend it; and if you find it in this situation, can you hesitate as to what has taken place? No; Then what's to be done? You are to pull the bones downward and inward, for the purpose of making extension, and, by using the force steadily, they slip into their proper places. And you are assured the dislocation is reduced, from this circumstance, that you can bend and extend the fore-arm to its utmost limits, and turn the radius into a state of pronation and supination.

But you may reduce the ulna and not the radius; and the radius may be dislocated without the ulna. In either case, the

reduction of the radius is to be effected by a separate operation. You reduce this by making a lever of the bone, applying a prop to the part, and bending the fore-arm. You press down the head of the radius. I now state to you the principle: you make a lever of the bone, raising the one end and depressing the other simultaneously; and, in short, if you know the principle, why you can apply it to a vast number of cases.

The radius may be dislocated *separately*, either forward upon the os brachii, or backward; and when it is dislocated backward, why it is a difficult dislocation to reduce. To reduce it, you have to pull it steadily at a half right angle, and to press back the os brachii at the same time. That is the mode of reducing it, as far as I am capable of judging; but it certainly is a difficult dislocation to reduce.

I go to speak of another dislocation, the dislocation of both bones backward, or of what is the same, the dislocation of the os brachii forward. In this dislocation, which is by no means uncommon, I suspect that sometimes the *coronoid process* is broken off. I judge of it, from the degree of projection I find. In some cases the olecranon projects considerably; in other cases I have seen it lie as closely down as possible. Now this dislocation of both bones backwards, or of one forward, is one that demands, of all others, to be replaced; for the fore-arm is quite extended, and cannot be bent. In all accidents occurring about the elbow-joint, where it is likely to be attended with some inconvenience, or defect afterwards, O, it is a great object to have the fore-arm bent, because if it is not, the fore-arm remains extended, and he cannot apply the hand to any part of his person, nor can he even feed himself; so that one would attempt to reduce it after a considerable period of time.

If there be a compound dislocation, and the wounds can be got to heal, then the dislocation will do well. There are many cases of compound dislocation of the elbow, that do well. I have seen a great number of them in my time. And I think I may say of myself, that I have been very lucky in the treatment of accidents of this kind—very lucky indeed. And I will tell you the source of that good luck: all the success is founded on a very trifling circumstance.—I say ligaments are very insensible parts, and also bones, and if you can but put them right, and keep them still, there will be but very little inflammation; but to put them correctly right, and to keep

them perfectly still—there's the difficulty. When I have been sure the joint was right, then I closed the wound with sticking plaster; and I always varnish the sticking plaster; dissolve sealing wax in alcohol, dress the wound, and varnish it over. It is that kind of varnish which they put over electrical machines, and you literally seal up the parts. Then it must be kept perfectly steady; do not let there be any motion; sponge, sponge it continually; in short, set a person with a sponge, to sponge it continually, so that there may never be the least perception of heat in it; then it will do well.

Fractures.—Now with respect to fractures: the lower part of the os brachii is liable to oblique, not transverse, but oblique fractures; and when those occur, there is great swelling, as in other cases. But you examine it, and find that the ulna can be bent and extended to its utmost, and that the radius revolves in the *sigmoid cavity* of the ulna; but there is a great swelling, and there is some insecurity, some grating, or something of that kind in the os brachii. Well, you cannot tell in what direction the fracture is extended, nor is it necessary you should. In many of those cases I never could feel any of those gratings. What I have judged of, as indicating an oblique fracture of the os brachii, is this, that I have found the internal and external condyles not exactly in the same line with each other; the one appeared a little before or behind the other, and that seemed to me to indicate a perpendicular fracture. Well, you put the arm in a bent position, and steady the os brachii by a splint, and the case does very well; indeed, in some cases it is not necessary to put the splint. But here it may happen that adhesions may take place between the surface of the bones, from a sort of jelly in the os brachii, and at the joints of all other bones. This may impair the action of the joints very much; therefore it is an object, as soon as the inflammation occasioned by the accident is over, about the third or fourth day, to move the bones of the fore-arm to such a degree as to prevent any such adhesion; as the old surgeons would say, to promote the diffusion of the *synovia* over the surface, and so prevent any permanent adhesion. With respect to the fracture of the olecranon, that is one of the fractures of the elbow; and if it is fractured, or if the bone is broken, you have to set the bone; and there is no mode of setting it but by extending the fore-arm. If you ex-

tend it, you bring the lower part to its proper place, then you set it, and then you keep the fore-arm extended until it is united, and it generally does well. I have seen hundreds of fractures of the olecranon that have done well.

There is an analogy between the fracture of the olecranon and the fracture of the patella, which is, that the fracture of the olecranon unites by ligaments when the bones are not close together. I have seen many cases where the arm, having been kept extended, though there was not an osseous union, yet the patient was able to extend and to bend the arm, there being ligamentous union.

ON FRACTURES OF THE FORE-ARM.

There is one position of the two bones of the fore-arm, in which they lie parallel to each other, and a splint put on each side gives them a sort of support, and keeps them together; it gives you the opportunity of supporting the bones steadily. It is, however, exceedingly difficult to retain the fore-arm in this situation; the thumb being bulky in the front, creates a disposition to turn the arm forward; the splints should therefore go into the palm of the hand, so as to support the thumb, but not so far as to keep the fingers straight; they should be allowed to lie comfortably. It is usual to put a little pasteboard tray for the arm to lie in, with a splint on each side; and be particularly careful that the splint on the inside supports the thumb. Also this is the position in which you put the fore-arm, when you perform amputation, so that you may saw both the bones at the same time. It is a position you ought never to lose sight of, for it is useful on so many occasions.

ACCIDENTS OCCURRING TO THE WRIST.

Well, then, I next speak of accidents occurring about the wrist. The wrist may be dislocated outward, or inward, by the rupture of the *lateral ligament;* or it may be thrown backward or forward. But there requires no great skill to ascertain that it is so dislocated, nor any great effort, as far as I have observed, to put the bones in their situation; the difficulty, however, is to keep all steady when they are in their proper situation, and that can only be done by treating it as a fractured fore-arm, that is, by putting a little tray of paste-

board, which will support the hand in a continued line with the bones of the fore-arm, not letting it drop down by its own weight, and steadying it by lateral splints, or at any rate by a splint put into the palm of the hand, not to suffer the hand to turn round.

But *sprained wrists* are perpetually accidents of tedious cure; and for a very good reason: because the injuries are not inflicted on the ligaments or joints only, but all the sinews are inflamed; inflammation takes place—there is a gumminess, and swollen state of the parts, which render the hand crippled for a considerable time. Now, in this said position of the fore-arm, all the sinews run in a straight line; there is no twisting of the fore arm, and all the sinews run in a straight line, when the fore arm is in that situation. Let it move about, and the sinews are twisted: and if sinews are inflamed, the twisting and motion very materially aggravate the inflammation. In short, in a common sprained wrist, if there was no ulceration or inflammation, the wisest thing a surgeon could do, would be to support it with a splint, keep the hand steady, and then, to keep down the inflammation as you would in the case of a dislocation of the *olecranon*. Often you are deceived with respect to a dislocation in the wrist: that is, you often think it is a dislocation when the bones are broken. In the case of a fracture, the fractured bones are a little oblique, and the fracture bulges out, so that you would sometimes think there was a dislocation; but generally, any person who knows the anatomy of the human body well, has no difficulty in distinguishing between the two cases, and in putting all right. And I can tell you that the cure all depends upon steadiness.

ACCIDENTS ABOUT THE CARPUS.

Now with regard to accidents occurring about the *carpus*; is there such a thing as a dislocation of the *carpal bones*; I never saw one, nor can I conceive it possible that there can be one. How can they be dislocated? Can a force drive in one of the wedges of such an arch as this? No. Can a force drive it outwards? There is the carpal ligament to prevent it. And yet there is an accident which seems like a dislocation of the carpal bone; it arises from effusion of blood under the sinewy substance which covers the carpus; and at that time there is, on the back of the hand, a hard tumour which a person, not

recollecting the situation of the carpal bone, might mistake for a bone. And the conclusion must show that this is not a dislocation, because the tumour is absorbed, the hard blood is softened, and you find that the carpus is all smooth as it should be. Now I say I have not seen this, but I think a *metacarpal* bone may be dislocated; a force may be applied to it that will knock out this proximal part of the metacarpal bone, and you may not be able to reduce it. In Mr. HEY'S *Surgery*, it is suggested that in a case of that description, you had better saw off the projecting end of the bone. But this is a thing I would not submit to myself, or at least I would not submit to it at the time of the occurrence of the accident, because, in doing so, you would make an opening into the joint of the carpus—you would expose the common *synovial membrane* that goes into the carpus. I should say to the surgeon, let it be where it is, until all inflammation connected with the injury, is gone off, and if it should then appear to be a material inconvenience to me, you may undertake an operation; when there is no inflammation arising from the accident, and when, therefore, the inflammation arising from the injury and the inflammation arising from the operation will not act conjointly. Well, these are my own private sentiments on the subject.

Then, though I represent the carpus as being so strong that I put no faith in dislocation of individual bones, I know that great degrees of force will knock it all to pieces. I have seen people tumble from on high on their hand, knocking the carpus, some before, and some behind the *radius* and the *ulna*, and knocking off the ends of the radius and ulna at the same time. There is no fabric whatever that can resist extreme injuries. You have also frequent injuries of this kind arising from the bursting of guns: a man has his gun burst in his hand, and it knocks off his thumb, and a third of the carpus may happen to go with it. Now I take this to be a very serious accident, any thing that destroys a part of the carpus, and leaves the other part as it should be; because certain muscles will not act, and the tendons may not be hurt, or the tendons may not act, and certain muscles may remain sound, and which may always have an appearance of injury to the hand. You must endeavour to support it and to steady it; but I say it is a difficult matter. However, I have seen cases that would induce me to try to save every case of this kind rather than have recourse to immediate amputation. I say, there is no warrant whatever for

any surgeon immediately lopping off any part of a man's body, except to save his life; it is his duty to give him a chance for the recovery of his limb. But I have a better opinion of an injury to the carpus by the bursting of guns, where the middle of the carpus is injured, though that may be the more material part; but then the lateral ligaments may remain sound, and the surgeon can keep the bones steady. I will tell you of a case of this kind:—There was a surgeon who went into the country, and then went to the sea side, and, cockney like, he went out shooting sparrows, and, cockney like, put his hand on the muzzle of the gun; the gun went off, and went through the carpus of the hand, knocking it all to pieces. He came to town, and sent for a neighbouring surgeon of his, and the surgeon said, God, you must have your arm lopped off, if you do not, you will have the locked jaw, and you will die; but I recommend you to send for another surgeon. He sent for another surgeon, who was an hospital surgeon. When that surgeon saw him, he scratched his head, and said, he did not like sudden amputations, for in cases of sudden amputations, his patients always died. A dispute then arose between those two surgeons, and they sent for me as an umpire. I went; I saw the case, and I agreed with the hospital surgeon that sudden amputations did not generally do well, and I agreed with the other that if lock-jaw came on, it would be better to cut it off at once; but I said this is an injury where the lateral ligaments are entire, and therefore I thought it might do well; in short, I would not have mine cut off under the same circumstances. I said, "will you give me leave to dress it in my own way." I was allowed to do so. I then dressed it, made a little case for it to lie in, and when I had fixed it according to my own notion, I said, "Now, Sir, sponge this continually, and never let there be any feeling of heat about it, and all will depend upon your constitution. You ought to feed yourself with particular food, keep your bowels regular, and so on." Now it was matter of perfect astonishment, in that case, that there was so little discharge, so little swelling, and that the case did so well. Pieces of bone came away, but the wound was all healed in the course of two months; and in three months the surgeon (a surgeon being an abominably bad patient, as they are all) was absolutely driving about in his gig in the streets of London, which he should not have done if he had been a patient, because when the thing is healed in that way, there is yet a great deal to be

attended to—the contracting of the new formed parts into some sort of an unyielding structure, the repairing of sinews, and so on. Well, I have no more to say about injuries to the carpus.

As to the fingers and the thumb, God bless me! there is no apprentice boy, in town or country either, who does not know when a man has got his finger out, and who cannot put it in again. He compares the one joint with the other, and if it is dislocated, he pulls it until it goes in.

With respect to

FRACTURES OF THE FINGERS,

I really cannot speak of those things; common sense tells you how they are to be attended to. There is only one dislocation of the finger, and that is the dislocation of the last joint, which is perplexing; and that is often a very perplexing accident indeed. It may happen that *the last joint of a person's thumb is dislocated:* say that the proximal end of the last phalanx slips over the distal end of the one above it; what is to be done? Can you pull it? No. What is to be done? How can you put it in? Can you put tackle to this small point? or can you pull this? Your tackle would slip off, and you cannot pull it. I have seen so much difficulty in reducing this dislocation, and have seen so many left unreduced in my time, that I am sure I felt myself exceedingly obliged to a gentleman who was a student here (Mr. CURWARDEN), for showing me how this dislocation was to be reduced. As to the extension, that will not do. Mr. HEY, of Leeds, and Mr. CURWARDEN had some communication with each other on this subject; but Mr. CURWARDEN told me distinctly how the thing was to be done, and certainly more intelligibly than Mr. HEY's book. He said, instead of trying to extend it, you had better try to bend it a little more, and that will humour the *flexor tendon,* and by only getting it so far bent that the proximal end of the last phalanx is raised up, you will have it in. Now I have reduced two or three cases of dislocation of this kind since I had this information, which I am sure I should have bungled at excessively in any former stage of my life.

LECTURE XXX.

ACCIDENTS OCCURRING ABOUT THE HIP JOINT.

You know the numerous obstacles that oppose a dislocation upward and backward; but, notwithstanding those obstacles, forces are so continually occurring to dislodge the thigh bone in that direction, that really the dislocation often happens; the bone is thrown out of the socket through the thick part of the *articular* ligament, and it slips over upon the dorsum of the *ilium*, generally with the head turned backwards. Now whether it lies on the dorsum of the ilium, or slips down into the *sacral* notch, does not appear to me to be a point of importance; therefore I make no distinction between those two cases. Then here is a dislocation characterised by the following circumstances: the limb is shorter than the other, and inverted; it is beyond the limits of the *acetabulum*, which shortens the length of it, and it is inverted. You cannot turn it out; the head strikes against the bone; then, I say, it is clearly indicated by those circumstances, and there is no difficulty in the method of using force to a dislocation so simple. You have nothing to do but to pull the knee with the thigh bent at a half right angle upon the pelvis, then the head of the bone will come down below the acetabulum, and having got it there, it generally slips into the socket. You cannot pull it over the acetabulum; you cannot pull it over the great ridge that is there, but you bring it down where the bone has no great rise in it, for there is below the acetabulum a groove, between the acetabulum and the tuberosity of the *ischium*, in which the *obturator* moves, and it is along this groove, provided you pull at the angle specified, the bone will come: and when it gets below the brim of the acetabulum, it slips in. I say, I cannot make a distinction in those two cases; there are some, however, who say, you must lift the head of the bone over the socket, but I know nothing about that, nor have I ever found any necessity for it. All I have seen of such cases have been easily reduced, and I can say this, that I have reduced the dislocated thighs of very strong and muscular men, in inns, and

at such places, where they have been brought after having been pitched off coaches, with no other assistance than that of a waiter or two, and a four-post bedstead.

Now the first thing is, to steady the pelvis of the subject; and this is done by putting a sheet round the thigh, crossing it round the groin, putting it over the pelvis, twisting it hard and tight, and then securing it to the bed-post. Mr. HEY, of Leeds, has told a good thing with respect to how you are to make soft cords for pulling dislocations: get any number of yards of calico you please, roll them up into a rope, and it is a cylinder of calico, soft and pliant, and, egad, of enormous strength? clap it round, as I have described, twist it harder and harder; then, when it is of the length you wish it, you can put one hand between the ticking of the bed and the posts of the bed, bringing it round one of the bed posts, and tying some hard knot—a *bowling* knot, or something of that kind. And you must learn to make these knots, for, egad, any thing giving way when you are pulling, is the most vexatious thing that could possibly be. Well, I fix my patient, and let one bed-post be diagonally opposite to the knee; then I put the rope round the knee, and then I put it round the bed-post; then I say, come and lean upon this. Now this is unremitting pulling. I see his muscles giving way; then I say, come, let us only have another hitch or two; then you twist it round the bed-post again; and then, by going on perseveringly, wearying the man, it has come in, in six or seven, or eight minutes. But if there is a case of difficulty, you must have pulleys; still you pull with pulleys on the same principle.

Is it posible that the head of the bone should be thrown out of the socket, and that the head should be thrown forward? It is possible. Such a case has happened; but I do not see any difference in the mode of reducing it. I do not think that the bone will come over the head of the acetabulum: I should, therefore, pull at a half right angle. But this case of the head of the bone being thrown forward, is so contrary to all we meet with in dislocations, that a man might be inclined to say this is fracture. Now here the head of the bone can be felt upon the dorsum of the ilium, and that was what made me so continually think of the head of the thigh bone of the *trochanter major*. You know the head of the bone in a full grown man, is about three or four inches off, and a little raised up; but if you feel the trochanter major, you know where the head

of the bone should be; and I say, if it were on the dorsum of the ilium, it should be palpable; but if you did not feel it there what is the inference? That it is a fracture—that the body of the bone has been driven up, and that the head is wanting. Again, I think you can distinguish a vacancy in the acetabulum, when the head of the bone is out of its place. But I do not know any special mode of reducing the one or the other dislocation. In the one case, the limb is turned inwards, and you cannot turn it outwards: in the other case the limb is turned outwards, and you cannot turn it inwards; and it is that case which chiefly characterises the dislocation from the fracture.

The dislocation downward into the *obturator foramen*. I say there is little guard against this, for force does not frequently operate to throw it down; but a man may be struggling with his legs, may be hit by something, thrown down, and the bone may be knocked out—the *ligamentum teres*, however, may remain entire, or it may be torn; but in either case, as I think, the limb will be elongated, for it is below the socket, and it is inverted. If the leg is inverted, then I know what the case is—if it be not, there may be some difficulty; but I have generally seen it inverted. Then what is to be done in this dislocation? Why, to be sure, a man who set himself to pull at the leg, would be pulling it further from the socket. You would perhaps say, pushing would be the best thing to be done; but you cannot push it over the brim of the acetabulum. Well, this can only be reduced by making a lever of the dislocated bone, applying a prop at the head of it, and a power at the other end: and thus would you lift it into the socket. And, unquestionably, if the patient was a pigmy, and the surgeon a giant, the surgeon would never do more than use his own hand for this purpose; but if the surgeon was the pigmy, and the patient was the giant, then what does he do? Why, he uses tackle and pulleys, and the distal end of the bone being across the other thigh, he pulls it a half right angle downwards; and he ties a sheet upwards, and pulls the upper end upwards and outwards, and by the compound action of those forces, the dislocation is reduced. Now, I think there is no difficulty in persons reducing those cases, if they only understand what they are about, and keep applying the force as they ought to do, steadily and unremittingly.

It does happen at times, that the head of the thigh bone is

thrown out of the socket directly upwards; and here the thigh is shortened, and there is no bending it. Well, then, you are to pull it down into the socket, you are to extend it, and if you depress the head of the thigh bone, making a sort of lever of it, that would be a very considerable auxiliary in the reduction. However, it may be brought down by simply pulling, but a little pressure at the one end, with a sort of lever at the other, would very considerably tend to assist it.

Fractures.—Now, whoever reflects on the mode in which the weight of the body is to be borne, will see how liable the neck of the thigh bone is to be broken. We jump from on high, and the force is on the head of the thigh bone; the force below, throws us up, and breaks this arch, and this is exceedingly common; but where does the arch break? Oh, generally, off from the thigh bone. Again, people fall upon their hips, strike the *trochanter major*, and what is likely to give way? They shove off the thigh bone from the arch it supports. The cases are often very complicated; sometimes the bend and neck of the thigh bone split, and the thigh bone gets wedged in between them; the cases are therefore often complicated. That splitting of the neck of the thigh bone is not at all uncommon; but when the fracture is on the outside of the *articular ligament*, the bone may crack, and be simply cracked, where the neck of the thigh bone is joined on to the body of the bone, and there may be very little suppuration between the parts, but the patient is unable to go one step forward. I have heard of cases where they have walked, but then it must have been cases, I am sure, of the neck of the thigh bone having been wedged into the body of the bone, not where it is simply cracked, because, where it is simply cracked, if you were to have a red hot poker at their buttocks, I am sure they would never bear on their legs to get one step forward. If a man, woman, or child, has leaped from on high, and fallen on their hip, where there is no retraction of the thigh, no eversion, nothing apparently wrong—but if, from the moment of that fall they are unable to move one step forward, what further evidence would you want of the neck of the thigh-bone being broken? I should want none; and I should treat the case as such. I know, for want of doing this, a great deal of mischief happens; the patient is not confined in one position, the motion goes on, the thigh becomes contracted, and the surgeon discovers, when it is too late, that

it is a fracture; when it is too late, indeed. Now, there was a gentleman who came from India to this country; he had just arrived, and was coming by the Dover Mail to London, and was then going to set off into Suffolk; designing to set off immediately, he thought he would arrive just in time for a family ball; egad, this Dover Mail broke down, and he was pitched off from the top, at Blackheath, and a surgeon was sent for. The surgeon arrived, but could find nothing wrong with his thigh; however, the patient was in the state I have described of pain or uneasiness. Another surgeon, an hospital surgeon, was sent for, and he also joined with the former surgeon, that there was nothing wrong—that it was only a sprain, or something of that kind. Well, but the man could not stir out of bed, and his thigh gradually got shorter, and distorted, and turned out by the action of the muscles; then it became evident to the surgeon who had attended him from the first, that it was a fracture; and at that juncture I was sent for. I said, unquestionably, it is a fracture, and you must put it right, and keep it steady; that is all. And the case did perfectly well. Now he has no appearance whatever of having had a fracture of the neck of the thigh bone. And I could tell a number of these cases. There was a lady who was pitched out of a gig, at a distance of about eight miles from London, and was attended by a very good surgeon, but he said, I can find nothing wrong about your hip. They wrote to me, desiring to know if I would go down? it was in the winter time, and I said I could not possibly go down on the Sunday, but that I should on the following day. I went, and I found the woman in a state in which there was apparently very little wrong; but I measured from the trochanter major, to the crista of the ilium on each thigh, and I said, what is the distance of the sound thigh? He said it was of such a distance; then said I to the surgeon—Well, now measure the other attentively, and observe exactly what the length of it is. He said, unquestionably it is shorter. Well, what is the inference, but that there is a fracture of the neck of the thigh-bone? Well, with respect to that lady, I said to him—Now, Sir, suppose we do nothing in this case, and suppose the woman never walks any more, what will the public say of us? They will say we did not understand the nature of the case, and that she had a fracture of the neck of the thigh-bone, which we never adverted to. But suppose we put her bones into the right place, and keep

her steady, then what will the public say? They will say we did every thing we possibly could have done, and that it was the nature of this case never to get well. I therefore put her into a right position. Then comes the question, what is the right position? I may differ from all great men of authority, but I have no hesitation in saying, lay them on their side. I would treat the fracture of the *neck* of the thigh bone as I would treat a fracture in any other part of the bone, and lay them on their side. If a person can be fairly laid on the side, and a splint put on the bone, so that the weight is supported by its own gravitation, then that will do; the rule is, that every longitudinal inch should press equally on the splint, so that you should be able to take up the limb, and carry it about on the splint as if upon a tray. Well, you have to put a counter splint. In the fracture of the neck of the thigh bone, where there is a counter splint, why the pressure of the pelvis on the head of the thigh bone keeps the bone steady, and causes it to press against the part which is broken. But all half measures are bad. I do assure you, I tell you no lie when I tell you that I have attended many cases where there have been fracture of the neck of the thigh bone, and where people have gone about without the possibility of knowing that ever there had been such a fracture; but all half measures are bad. You must therefore turn them fairly upon the side. And how are they able to go to sleep without moving? Why, by lying fairly on their side, the gravitation fixes them in that position; attend to the points of the pelvis, and I am sure they will never move. For my own part, I have no objection to people lying on a soft feather bed. All the trouble I have with the patients is for the first dozen hours, in watching them that they do not move; and if they are laid on a feather bed, they get sunk and fixed in that feather bed in such a way, as that out of it they cannot move afterwards. Well, that is all I wish you to attend to upon that point.

I am attending a child; I put the child in this position; the child lies there for three weeks, and never moves—it may be longer—and after that time the child gets to the other side of the bed; you cannot confine it any longer; and indeed when the bone is knit, there is no necessity for longer confinement. And why does the child, or why do patients lie for so long a time without moving? Say the case was mistaken, and that it was not a fracture, is any position so com-

fortable as lying on the side, with the knee half bent? Is there any position in which, in the case of a sprain, for instance, the parts could be kept so still, and the persons be so comfortable, as lying on the side? I believe not. And if you put them on the side, you put a drawn sheet under them, which may be removed at any time, for the purpose of putting clean sheets without disturbing them. That is the position I should put patients in, and it has the advantages which I tell you.

As to fractures not uniting when people are laid on their backs, why, I don't wonder at it, for unless you have beds for the purpose, there is a necessity for keeping a bed-pan under them, and then the motion of the pelvis causes the parts to move. I do not wonder, therefore, that there is no case of the fracture within the articular ligament which has united, because till of late, they have not had beds by which the bones can be kept still. It is not particularly with respect to the thigh bone, but there is no bone in the body that would knit if there was a little motion.

But they say the bone cannot unite if the fracture is within the articular ligament, and for this reason, that there is no *periosteum* covering it, nothing but a shadow; and that no blood can be received but through the ligamentum teres, and the synovial membrane. Now, I do say, and will maintain, that this reasoning is not according to the principles of sound physiology. Let whoever reason it that will, I will say, and even though it were in opposition to the fact, I would still say the same thing; because, granted that this part of the bone has but little power of life, much powers of life are not necessary for union, if there be life in the other parts. It requires but very little powers of life indeed, to unite with other parts that have life; for instance, you put the testicles of a cock into the body of a hen, and yet they live there; and again, you put the spurs of a cock into his comb, and they grow there. Well then, here are all the vessels necessary to nourish the parts, and why should they not be adequately supplied. Now it certainly is a case of rare occurrence, and no positive proof has as yet been offered to us, of any such union taking place; I hope, however, such proofs will arise in the after part of surgery, for the honour of the profession, and I now say, if I were to look for them at any time, it should be in putting the patient into that situation in which his body will never move, and in which the weight of his pelvis will make a

steady pressure against the top of the bone from which it has been broken. When I say a little pressure, of course I mean so, that a very little motion cannot disturb it.

As to your feeling fractures with your fingers, at all times, and making them grate and so on, it is out of the question ; I have myself examined cases, and could not tell them, but the patients have done it, and then I have seen that they were fractures. There was a very old man, who slipped down and broke the trochanter of his thigh bone ; I was sent for, and I put it in ; but he was a very old man, and not of sound bodily health, and the poor fellow, though he had no pain, sunk away, from confinement or something, I do not know how it was, but he died. We fed him as well as he could be fed, and we gave him medicine for his bodily health, but he died. When he was dead, I begged permission of the family to look at him, which they granted me, and I found that it was broken.

I remember a case in this hospital, where a man had fractured the neck of his thigh bone, and got his head knocked all to pieces. We were attending to the fractured skull, but seeing the man uncovered, I said—Ah! poor soul, he has broke the neck of his thigh bone into the bargain. Well, he died, and we were examining the brain, in the dissecting room, in the dead house ; and one of the pupils, who had been examining that thigh bone with a freedom which is done when a man is insensible of pain, said to me, I remember your saying he had broke the neck of his thigh-bone, but it was not so. Well, then, said I, he was a cripple before. I had occasion, however, to go out for a little time, and just at that time the youth went and cut down into the thigh bone, and sure enough he found that the neck of it was broken.

Diseases.—Diseases here are so deeply seated, that you cannot have that assistance, by sight and touch, which you have in other joints ; but it is my firm persuasion, that there is no difference in the diseases of hip-joints, and the diseases of joints in general. What did I say about diseases of joints in general ? I said, that sometimes the diseases began in the joints, and were eventually communicated to the bone, whilst in other cases, they began in the bone, and, eventually, got into the joint. Then, again, in some diseases of the bones, there was a kind of inflammatory matter found, which really did considerable mischief; which caused the absorption of car-

tilage, the growth of fungus, and so on. And that there were other diseases of joints, more of a constitutional nature. The question, then, is, have you any such diseases in the hip? And the answer is, yea, very clearly there are. Now I will tell you first of all, common inflammation will happen in this joint. The case I mention for this purpose, was the case of a boy whose parents thought they would consult Mr. Abernethy upon a *lumbar* abscess. Now you know, in diseases of this kind, it is usual to have a collection of fluid at the joint; and there was a considerable quantity of fluid here, which they had taken for a lumbar abscess. But the gait of the boy immediately disclosed the nature of the disease—it was that of a limp. Now, here I would say, it would be well for surgeons to study the gait of a person, as a horse jockey does the gait of a horse. A horse jockey puts the horse to move, in order to see his gait; and if a person has a diseased hip, the limb is only used as a crutch to help him forward, and therefore, the disease cannot be mistaken. But if any further evidence is wanted, I will tell you how to get it—just press upon the acetabulum. In this manner I have detected a number of those cases. Well, but as to my case: I told the patient he ought to go to bed; we put leeches on the part, washed him, ordered him doses of calomel to keep his bowels regular, and so on; and then, when there was no more pain or pressure on the acetabulum, I put on a blister; and this was just about the time when a new cerate came out, and I told the surgeon to dress it with this cerate; it was, certainly, a new mode of dressing it, but one dressing sufficed, for whether he put too much on or not, I don't know, but there was a slough came away, and a large ulceration, and that was, perhaps, fortunate enough, because it kept the boy quiet until the leg became quite well, and he walked about. I urged upon them the necessity of being very careful to keep him quiet, and the boy was taken down into the country, and sent to school. Two years afterwards, that boy came up to London, and was put into a merchant's counting-house, where he had to go up and down Cheapside every day with bills, and, in short, had as much going backward and forward as a two-penny postman. His thigh got bad again, and it happened that he lodged in a house in Smithfield, in which one of the students also lodged. I again went to see the boy, and I then told the father—you ought to be aware of the serious nature of this malady in the

hip; the hip may get well enough to bear moderate exercise, but it won't bear the exercise the boy has now to take; you must take him away from this situation, and, in the mean time, nothing is to be done, but what was done on the former occasion. Well, all this was done, and I then said to the father, now if any thing goes wrong again, if you see the boy limp in the least degree, you must let me know immediately, for it is a most important case, and you should certainly not let him go into his situation any more. I saw no more of him for about three months; I then observed the father going up Chancery Lane, and the boy limping after him. Now I was rather rough with the father, and I said, Pray, Sir, how is this; did I not tell you, the instant you observed your son limping, to let me know? I hope he is not still in the same situation. Yes, indeed, Sir, he is, he replied. Well, then, Sir, said I, I shall have no hesitation, whatever, if this boy dies, to declare that you are his murderer; and that was the case sure enough. Matter formed; dislocation took place; the father got the boy into this very hospital, and here the child died. The ligaments got destroyed, the action of the muscles dislocated the bone; and there is no mistaking a case of this kind.

I remember a case where the bone was diseased, and it had gone on till it had got into a very bad state. I said to the patient he ought to use some swathe about him, to keep his socket still, or, egad, it will be thrown out. The man said, "Lord bless you, Sir, I am obliged to be awake all night, and watch it, or I am sure the bone would be thrown out of the socket."

Now this is a common disease; well, but is there not disease of a constitutional nature? Unquestionably; and I will just bring those cases before you, and then I shall have done for to-day. And how am I to tell you the number I remember of them? I am in the habit always of selecting those cases which are best calculated to impress the anatomical facts on your memories; and I have no better case than that of a young lady, who was known to one of the students here, and he requested me to go and see her. She had a limp, but she had the most agitated state of breathing possible; her pulse exceedingly frequent; and how did this lameness happen? Some of her relations had been in a naval action, and she was uncertain of their fate. There had been a very severe

fight somewhere, in which some of her relations had been, and she had not heard whether they were dead or living; this disturbed her very much: she got excessively wrong in the state of her health, and her hip became painful. I said, I could not suggest either leeches or any thing else, but said you must foment your hip, keep it quiet, and attend to your diet, and the regulation of your bowels. I left her, and saw no more of her for about a month, when she was brought to town in order to see some other medical gentleman; I also was desired to be at that meeting. She then stood, supported on each side, without being able to move. We agreed that it was a case in which leeches would not do good. After a time, that girl went to the sea-side, and she eventually recovered. Sir Charles Blick (for it was he and I that met) had a Villa at the place to which she went, and he was always looking out for patients where he was; he attended her, and he afterwards told me it was all very well, and that she had recovered. If you set to work manfully with your leeches and blisters on irritable subjects, you will only make bad worse, that I am certain of, in any of these cases of constitutional disease. And this I will endeavour to show you by mentioning another case.

There was a lady who had consulted surgeons of great eminence, and who had an ulceration which broke out into a tremendously large sore; she lived about a dozen of miles from London. When I saw her, I said to the surgeon who had attended her, It is evident that this leeching and blistering has done no good; I should foment it; but it is her health that is necessary to be attended to. It luckily happened, that as the wound was healing, she had an attack of disease in her knee, and that so occupied the attention of the surgeon, that he never again looked at the wound until it got healed; then she got better, and went about on crutches. Now that is what I should recommend, that they should be allowed to exercise on crutches, never bearing weight on the joint. She went about awhile on crutches, afterwards got well, was able to walk perfectly in less than a year, and is now a perfectly straight woman, and walks as uprightly as if she had never been lame. Now I mention this case, on account of this other curiosity which is attached to it:—a brother of hers got ill in the same way, and I do verily believe, that if he had been laid in bed, and treated with leeches and blisters, and so on, his hip

would have got into precisely the same situation; but by not doing this, and only guarding against any motion that might aggravate the disease, and by putting his bowels right, egad, the boy did very well. Now, then, in these particulars, I see no difference in the diseases of the joint of the hip, to what I see in the joint of any other part of the body: and I have nothing more to add at present.

ON ACCIDENTS ABOUT THE HIP, KNEE, AND LEG.

I said there were many cases where the hip was dislocated backward, in which the dislocated bone had afterwards broken, so that though you attempted reduction, the bone could not remain in the socket. Those are cases which I do not like to speak of much, inasmuch as they might afford a person an excuse for not reducing a dislocation; but I am persuaded that those cases do occur, and it is right that every surgeon should know of the occurrence, because he may be blamed when really no blame is attachable to him. I remember the first case I met with of that kind vexed me exceedingly: it was the case of a French emigrant; on coming from Dover, on the top of the coach, he was pitched off; he was a large bulky man, and was brought to Lawrence Lane, and put to bed there. I set to work to reduce what I believed to be a dislocation, and I brought the knees so, that they perfectly coincided one with the other; I brought the limb into that situation, in which, if it had been a dislocation, it must have been reduced; but upon taking off the tackle, the leg was withdrawn again, the knee was inverted; and why that should happen I do not know, except that I conclude there is so much of the neck of the thigh-bone upon it, as to prevent its being drawn outward. Well, this was a poor fellow without any friends, and I got him into this hospital, and here he lay for a long time indeed. His limb wasted away, and you could feel the round head of the *os femoris* just like a ball. There are, therefore, cases where either that happens, or where the brim of the *acetabulum* is broken, and where you can bring the limb down to its proper situation, but where it will not stay. And these, I say, are facts which should be known; and yet there is a reason why I do not wish to dwell particularly upon them; for it affords a kind of excuse to a man when he cannot reduce a

dislocation, to say there is some fracture that prevents the reduction.

Now, speaking of the *diseases occurring in the hip*, I have now to speak of the disease beginning in the ends of the bone, and ultimately affecting the joint. The whole of these bones are spongy bones, and very obnoxious to scrofula; the acetabulum, itself, is liable to become spongy and scrofulous. In cases mentioned in Mr. Ford's publication of the hip disease, called the *ischias*, it is a scrofulous affection of the bones. In such a case the bones become light and spongy, and will not sustain the weight that is usually pressed upon them; they ulcerate, the head and neck of the thigh bone are progressively absorbed, and this is what goes by the name of ischias. In the patients who are affected with this disease, you will generally observe it beginning, by their halting and limping; and it does appear to me of importance, that you should pay attention to that. But I have to tell you, that I have seen many cases where people have halted as if they had this disease, and yet it was not the disease; they have halted in such a manner, as to impose the belief that there was some disease beginning in the hip of a *chronic* nature, and yet, upon putting their digestive organs right, they have got well. Still, in the scrofulous disease, there is that halt, and people seem to think they will get better from moving about, as a horse that is lame goes best when he is rode a little; and in this way the poor creatures are encouraged to move about. Parents say to their children, "O, you must walk, and walk it off." Now all this is excessively wrong, for the disease begins, as in all cases of scrofula, in an indolent manner; then there is generally an attack of inflammatory action, and suppuration happens round the joint; the abscess breaks; the head and neck of the thigh bone are ulcerated; then it is retracted by the muscles, and eventually it anchyloses to the pelvis; it does not always anchylose indeed; there is at times a ligamentous sort of union formed. Such are the cases of people that you see commonly in the streets of London who have irons to walk upon; and some of them you see going with their limb dangling about considerably retracted. I can say no more than this by way of description; but it is a very horrible malady, and requires the strictest possible attention, or you will have the limb much shortened, much distorted, and the person a very

great cripple indeed. Now I will say, if you know the disease early, and treat it properly, I am convinced you may do a very, very, great deal of good. I am convinced that I have seen cases of this disease where, absolutely, the patient recovered without any evident shortening of the limb, and I will tell you some of those cases, as an encouragement to you to treat the cases upon common principles of surgery with steadiness:— There was a young man, a young lad, I suppose he was about fifteen years of age, who had this disease, and his father wrote to me begging me to see him, and he said he would bring his son half way to meet me, if I would go half way to meet him; he lived about forty miles from London, and I was therefore to go about twenty miles. Well, I went there and saw the chap; and when I saw him, egad, I heartily wished I had never seen him, for he was a flabby, fattish boy, blear-eyed, red haired, and altogether a scrofulous-looking, unhealthy boy. When I examined the boy there seemed to be thickening about the *trochanter*, and every symptom of scrofulous disease in the bone. He was in a place, too, where there had been numerous medical men consulted, and they were all clearly and decidedly of opinion that this was a case of ischias; and I am sure, for my own part, I had not the shadow of a doubt upon it. But the father and I were at an inn where we had some dinner together, and that enabled me to give him a lecture. I told him to put the boy to bed, apply leeches, foment the hip, and to regulate his diet and his bowels. And I said, after a time, when the parts seem to have got knit together, so that a little pressure could be borne, you must have an issue, and that issue must be kept open. I said, when it is in that state in which it will bear a little swinging, without irritation of the malady, I will allow your son to go about on crutches, with his foot in a sling, not slinging it beyond what is absolutely necessary, and never, never bearing the least weight upon it. I mean, I would allow him to go about the garden to take exercise, a certain number of times, at certain periods of the day, for the good of his health; but attention to his health must be every thing. The father was a lawyer, and an intelligent man, and seemed to understand the surgery of the case, as far as I was able to recommend it to him, and we parted. We had often letters afterwards; he wrote to me telling me how his son was going on, and it was a year before that hip was in that state in which he could go about upon crutches. At the end

of the year he did go about on crutches; there was no apparent shortening, and the little exercise seemed to do him good. Then the question was, when he might bear upon the hip, or leave off walking on the crutches, and I said not for three years; for if any one would ask me, according to what I have observed, what would be the average time of the cessation of this disease, I should say not under three years. It is a scrofulous disease, and the history of scrofula is this—it is a local diseased action, which will not endure for ever, but it is very slow in subsiding, and will subside soonest in proportion as the constitution is healthy. The disease ceasing, the healthy action returns, and then there is the anchyloses.

Well, now I will tell you one of those cases. But, in the first place, to show you what sort of a constitution this boy had, I may tell you that the chap, after he arrived at the age of two or three and twenty, felt that nothing would serve him but being a soldier; a military spirit sprung up in him, and the father wrote to me to inquire whether it would be injurious to the hip or not. I replied I did not think it would; but I said, You must be aware that your son has not the constitution of a man born for a soldier; he can neither bear the privation nor the intemperance of a military life, and therefore it is absurd to think of it. However, nothing could alter the boy's mind; and the father came up with him to town, and called upon me; and, as I had nothing to say to him, but that I thought it a most foolish scheme, I wished him success, and he went away. When he got the length of Portsmouth, he was seized with another disease, which at once put an end to his military career; he was brought home again, and I cannot tell you whether he is now dead or alive. You may say, Aye but you did not see the disease in this instance; I say I did not, but that I remember seeing two cases which were *fac similes* of it. In those two cases abscesses were formed, and I had the management of one of the cases; I sent that one to a place a little distance from London, and treated it as I have told; the other went into the country. The one whose treatment I superintended, got perfectly well; I could refer to him now; by the by, he sent to me to see one of his children lately, and I am sure you could not say there ever had been any disease of the hip; abscesses formed in both cases, and both had the halt I have mentioned; and the boy that went into the country died of the hip disease. Now I am convinced that a great deal

of good may be done, and the good to be done is in keeping the part quiet, repressing all inflammatory action, instituting counter irritation, and taking care that that counter irritation does not disturb the health of the patient. And if ever there should be a fit of inflammatory action coming on, the patient should immediately be laid in bed, the pease taken out again, or beads, or whatever you may have put in to cause the irritation; and nothing but soothing treatment encouraged. You must not produce irritable action, while the internal disease is inflammatory.

To suppose that all cases will do well, is to suppose an absurdity; to suppose that those cases will do well in an hospital, is to suppose an absurdity; for the air will not admit of it, with regard to the patient's general health. But many of such cases will do well.

Now if there is absorption of the head and neck of the thigh bone, and retraction of the limb, and abscesses formed, and abscesses broken, there's a most important thing to be done, which is to get the limb into its right situation, that is, to get it into a situation descending perpendicularly from the pelvis, with the toe, neither turning in or out, so that when healthy action returns, and anchylosis takes place, let the anchylosis take place in a way in which the weight of the body will have a proper bearing on the limb; for of what use is a limb to a man, if the limb anchylose to the pelvis at a right angle. If it is properly done, and should afterwards be shorter, then you have only to supply the shortness with a cork shoe. Now this I conceive to be very important, and it is the concluding part of the surgical attention requisite in this case.

ACCIDENTS OCCURRING ABOUT THE KNEE.

I shall first speak of dislocations about this joint. The *tibia* may be twisted outward and inward, and it may be thrown forward and backward; and what of all that? You are to put it right again. And I say, this is one of the joints where any one may see what is wrong, for they may compare one knee with the other; and it is one of those joints in which I never saw any great difficulty in pulling what was wrong to rights—by pulling it straight, and making it in the same line with the other knee. Then, when you have reduced the dislocation, you keep down inflammation by washing it, by con-

tinually sponging it, and always keeping the limb straight; for if any stiffness were to occur in the joint, and the limb was straight afterwards, still you would be able to walk well; the leg and thigh would appear as if in one piece; but you would not walk so well if the knee was bent. As in the knee, so in the elbow joint; the great object is to keep them straight. In either of these joints, where the motion is likely to be imperfect after the injury heals, it is most important to keep the limb extended.

Well, I say, I do not see difficulty in reducing those dislocations; and I have seen some bad cases. I remember a very big man, when I was quite young in the profession, who was riding in Leadenhall Street, when his horse fell, pitched him off with force, and he came upon a projecting stone upon his *tibia*. He was a heavy man, as I have told you, therefore the fall dislocated the joint, and drove the tibia back into the ham. He was taken home to his house, in St. Mary Axe, not a hundred yards from where the accident happened. He sent to me, and I went. Upon looking at him, a most curious spectacle presented itself indeed; the knee-pan, and the ligament of the *patella* all drawn tightly together; and a large knob raised like a second knee-pan. The man was in bed: I made fast his knee, and soon brought it to its situation. I will not say it was very gentle pulling, but it was steady pulling, and by using that exertion, we did put it right. Very considerable inflammation, and so on, was the consequence, but after about three months, the man walked as well as ever, and had but very little tumefaction about the joint, after that time. Here there are oblique fractures, as there are in the elbow joint—oblique fractures in the *condyles*, and these are very vexatious cases; for sometimes the *condyle* is so twisted that it gets out, and you find it very difficult to get it in its situation, and when you have got it in, to keep it there; but this is the object, and you must try to accomplish it. The leg must be extended along the thigh, kept still, and the inflammation kept down. I have known a case of this kind go on pretty quietly and right for a certain time, and then the patient becoming fidgetty, some starting and motion has taken place, which has renewed the inflammation in the joint; ulceration has happened, and an opening been made into the joint, then a horrible fever has come on. So that I would always watch those cases very vigilantly. Now, as to the dislocations or

fractures of the leg, I have always been in the habit of speaking of them when the muscles were demonstrated.

You will know when you have set the patient's leg right, by attending to the *spine* of the tibia and the *skin*; for granting it was wrong, if it was laid in a horizontal situation, you would ascertain it by tracing the spine; if in a contrary direction, by tracing the skin. Now, you may laugh at all this, and it would be a most unimportant remark indeed, if it did not happen that a bandy-legged man broke his leg. If he broke his leg, and you were to put it straight, you would just put it wrong. I remember a bandy-legged man once in that situation; notwithstanding all the pain the surgeon was putting him to, he smiled and said, Sir, I think you are putting my leg straight, and, if you are, that is what I never remember its being before!

It is a question—where would you amputate below the knee? And the answer used to be, a hand's breadth below the knee. But, not to say that hands are of different breadths, I would say, where shall the measure be taken from? because a hand's breadth below the knee, when bent, would not clear the muscles of the patella; had you not therefore better say, four inches below the knee? The scientific reply would be, I would amputate below the insertion of the inner ham string—four inches below the knee. This is the only part where you have to consider where you are to amputate, because in all other parts you are to leave as much of the patient's limb or body as possible; but here you are to amputate with a regard to a person's convenience in walking afterwards.

FRACTURE OF THE LEG.

There is very little to be said with respect to fractures of the leg. It appears to me, those generally do best that are treated in the manner I have described. By having the knee half bent, you relax the muscles of the calf of the leg; and I told you what I considered to be the rule of practice—that which gives steadiness to broken bones. If you lay them upon a splint—upon a plane—a horizontal plane, then every longitudinal inch of the limb presses equally upon it, and you carry the limb as if upon a tray. Then you have an opposing splint. The half bent position is certainly the most comfortable; but, in saying this, do not understand me as advocating that as the

the position, because there are many oblique fractures where the bones will not lie so steadily, as if put on the back. Mr. POTT, who is considered as a great authority in surgery, strongly advocated this position of the limb; and Mr. HUNTER as strongly advocated the other. Now, it seems to me, if surgeons were to adhere to the one or the other method of putting up broken limbs, to the exclusion of the opposite method, they would be doing wrong. Each method is to be adopted in particular cases, as it may seem to answer best, according to the way in which you break your leg.

LECTURE XXXI.

ON ACCIDENTS ABOUT THE ANKLE JOINT.

As to dislocation, there is the dislocation of the foot outward; and in that case the weight of the body falls within the tibular side of the arch of the foot, and the foot turns outward. Now, under these circumstances, I can tell you, the weight of the body is retained on the fibula, and this being a slender bone it often breaks, about three inches above the ankle; it may be more or less. Also, very often the ligaments are lacerated between the tibia and the fibula. The force presses out the external muscles, so as to lacerate the junction between the tibia and the fibula, and the fibula is broken. But it is a bad thing even to look at, when the foot is twisted out in the manner in which it is in this case, and still worse in the worst of all cases, namely when the dislocation is a compound one, where the surface is rent in a semicircular manner, not all round the joint, but round about two thirds of the joint, and where the flap of the skin is turned quite over the astragalus, bending upon it; and this is the situation in which you find the patient. One cannot wonder that the old surgeons were inclined to amputate immediately in such a case, but I am sure no modern surgeon would do so, because there is now such an extent of experience showing that the case generally terminates well: at any rate, I can take upon myself to say I never knew but one that did not terminate well. Yet I know they are very horrible

cases, and therefore I do not wish you to think less seriously of them than they deserve; but I should steadily oppose immediate amputation, because the bones can be immediately replaced, and if they can, and can be kept steady, and inflammatory action can be subdued, it is important that it should be so; and I say no surgeon has a right to lop off any part of a man's body but for the preservation of life; it is your duty to give him a chance of doing well. I say, in one of the cases I knew the man did not do well; but I really did not know why he did not do well; he was a patient in this hospital, under the care of Mr. Lowndes, and he went out of the hospital with a very loaded limb, after having been a long time here. I heard afterwards that he died, but I do not know why. However, I mention that case for the reason that this man suffered for a long time, though his *urethra* was as sound as possibly could be—suffered a long time with a retention of urine, and that is what often takes place from injuries to the foot or to the leg. I remember being sent to draw this man's urine off every night and morning, and I piqued myself not a little in doing that. When I put the instrument in, it has been grasped, and actually taken out of my hand, by the irritability of the muscles, and turned quite round. There was nothing to be done but to go on coaxing it gently, and at last I got to the bladder; and I do say I did that for a whole week, when at the end of that time I could not possibly get the instrument into the bladder; and then I said to myself, egad, the fellow's bladder must burst. I told the nurse to put leeches to the *perineum*, and to give the patient some castor oil, or something of that kind, and I said I would go and tell the surgeon what was the matter. I went away with a troubled mind, because I certainly felt vexed at not succeeding, and I apprised the surgeon of the nature of the case. He said you have done what is very proper, and we shall see how he is in the morning; but before the morning came, he made water as freely as possible, and never was troubled with a retention of urine again; so that I am sure I prolonged that man's retention of his urine for one whole week, by the absurd practice of drawing off the urine. You have only to put leeches on, even where stricture is, and, after the inflammation is gone, they will make as much water as before, or a great deal more, and I am sure that people do keep up irritation by poking up instruments.

But to tell you what is likely to be the consequences of

these cases, I have been in the habit of relating three instances; and the first was the case of a poor *wee* man, who lived in my own neighbourhood, who was employed as a labourer at some buildings, and tumbling off a scaffold, he got a dislocation of the foot inwards. I was, at the time, sitting very quietly at home, when I was called upon to go to an accident that happened in an adjoining street. I went, and when I got there, the door of the house was open, and there I saw this little wee man lying on a mattress in his own parlour, with his foot up. I looked at it, and said to him, "Sir, this is a sad accident, but do not be out of heart about it, we must put it to rights again;" and so I took the skin out, which was turned in over the astragalus, which I think you should always do. I laid the man fairly on his side, with his limb in a comfortable position, his thigh half bent upon the pelvis, and the leg half bent upon the thigh; laid him on his side, and his foot, as much as could be, upon the fibular surface; then I had to use very little effort indeed to put it all right. The semi-lunar rent of the skin, and the other edge, came in such close contact, that it did not seem as if there had been any wound at all. The foot altogether went into its right position; and an accident, which at first you would have said was one of the most horrible kind, appeared to me to be of no consequence. And when the little man saw his leg and foot looking as they ought, he said to me, "Is it possible, Sir, that this case should do well?" "Yea, verily," said I, and I have seen many such cases do well. He said, "Why, Sir, they have gone for the instruments." Two surgeons had been there, and they had actually agreed that amputation should take place immediately. I had not been at all informed of this, so that I was put into an awkward predicament, and was obliged to wait for the arrival of those two heroes. When they came and saw the man lying very comfortably, they really seemed staggered, and said, "But, Mr. Abernethy, you are well aware of these complicated accidents, and can you give us an assurance that this will do well?" I said, "No, certainly not; I can do no such thing; but if it does not do well, you can but have recourse to amputation afterwards, and my surgical character is pledged no farther than this. I give you an assurance that no immediate mischief will come on to endanger the man's life; and you may wait to see if his constitution will allow him to do well." "Now," I said, "I feel I have got into rather an awkward

situation, and, if you please, you must just allow me to finish it in my own way." I then got splints, closed the wound, varnished the sticking plaster, and plastered it up. Then I told them about sponging, sponging it continually, so as never to let there be any increase of temperature. In short, there are but two holds you have over any person's mind—those are fear and hope; and I make use of them both with my patients. I say, if you will be perfectly still, I am confident you will do well; if you move one jot, I am sure you will do ill, that is all. You must not move, not the variation of a hair; not even if an army of fleas were biting you all over, because your safety depends upon perfect steadiness. Well, as to this poor little wee man, God bless me, the wound would not heal; abscesses formed, large ones they were; they were let out, and it is upon this point I have now to speak. The mischief is not done to the joint only, but all the muscles are sprained in a most horrible degree; sprained to such an extent, that the inflammation will take place between the fascia and the substance connecting the muscles; and as soon as you see the fluxion formed, you put a lancet into the abscess, and let it out; for the want of doing this would be, that the joint would come out, and the matter would burst out, leaving a large open abscess and cavity. Now that is necsssary to be attended to. But I suppose the wound of this man was all shut up and well in about three months; in six months he walked with a stick; in a year he walked without any stick, and after that he walked about, I believe, for twenty years, pretty well, as long as I knew him. Well, that is the case of an unhealthy man, but without any particular disorder of his constitution.

Now I will tell you the case of a strong man. This was a fellow who had a sack of malt fall upon him, from the top of Meux's brewhouse; they had been taking up malt with a crane, and a sack fell out of the hooks, and struck him on the leg, by which his foot was turned bottom upwards. The student sent for me to amputate his leg; I came, saw the accident, put it right, laid him on his side, laid him on the splints, told them to bathe him, and so on. The pupil stared, but did not say any thing, until I was going away, when, I believe, they thought I was drunk, or had lost my senses, and they said, "But, Sir, what is to happen?" "I do not know," I said, "but you will see; this is what is to be done; I have told you as plainly as I can;" and they reported that to the surgeon. They

said, such an accident as that ought to be immediately amputated. No doubt of it. But all this was said in a sort of whispering manner. I saw the case daily, and I opened an abscess below the fascia; but really the healing of the wound was surprising. Now that man was kept in bed for six weeks, and at the end of that time, he walked about the ward with a stick.

Now, for another: this was the case of a coach-maker, a fellow that used to live freely; not healthy; wrong about his digestive organs; and he got his foot twisted inwards from being thrown out of a gig, which he was driving. He sent for a surgeon, who reduced it; but that surgeon forgot to take out the skin, so that the skin was squeezed into the joint. When the man came to town, I was sent for; and when I saw him, I found that I was obliged either to dislocate his foot again, or to carry a knife into the joint, for the purpose of bringing out the skin, and I preferred the latter; therefore, I laid it open, and got the skin out. Now this was a man of a very disturbed state of health; gay, very impatient to have any thing to do with, irritable, and he had matter formed; had irritable muscles, considerable inflammation; had a violent degree of fever, and, in short, became light-headed. And they very often have their nervous system disordered in such a manner as to become light-headed; or they have a violent complaint in their bowels. In this situation, it was a question whether this man should be bled or not; and I said no, certainly not, we must attend to his bowels, and we must not give him such food as his stomach cannot digest, but which will produce irritation. The third day, however, the action of the fever was so vehement, and the man was so light-headed, and so disobedient to those regulations of quietness which he ought to have observed, that I consented to his being bled, and we took from him twelve or fourteen ounces of blood. His blood, however, was not inflammatory, and I do not think it did him any good at all. Now this sort of fever, arising from accidents, generally subsides about the fourth day; and so it did in him. But we had a great deal to do afterwards, I can assure you; for the foot was twisted, with the muscles, in a wrong direction; however, by straps, and plasters, and management, we got it to rights; and I was continually telling him what the error in the position of his foot was, and that the proper bearing of the body should be on the middle arch of the foot.

Egad, speaking to a coach-maker, a man who understood how the bearing of weight ought to be! He had always some new contrivance for keeping his foot in a right position; so that I really attribute the keeping of his foot in a good position in a great degree to his own ingenuity, and he has been walking about well ever since.

Now, then, I have given you three cases, and I could tell you many others, if necessary; but I say I have generally found them do well, in consequence of a strict observance of those principles which should regulate the conduct of surgeons in the treatment of diseases. If you can but put parts right, and keep them still, blood agglutinates them to one another; and if you can but repel inflammation, a great deal of good can be done; the parts will be united by adhesion, and therefore, where there is not inflammation, the case is rendered of far less importance, than it was formerly.

Well, but about the mis-direction of weight; that is, the weight not falling upon the arch of the foot, but falling on the *tibial* aspect of the leg and foot. O, that's a very bad thing indeed. There you see the internal ligaments are strained; they are strained amazingly, and they elongate.—That is what you see very commonly in growing lads, and particularly those who carry weights on their shoulders; in bakers' boys, for instance, you see the foot turned out, the internal muscles turned in, the knee—I do not know whether it is not called baker-knee'd or not—the knees approximating to one another; so that it is of the utmost consequence to keep the weight of the body bearing on the astragalus and middle of the arch of the foot. You may say, hang these chaps, I do not care much about them; but I will tell you of a set of patients who you will care about: a young lady has her foot distorted, the muscles strained, and, as you will find, the round head of the astragalus coming out of the bed of the *os naviculare;* if the foot is twisted out, it will necessarily bring the knee inward; and there is a case you will care about. Consulted by people of wealth and opulence, and distinction, "How am I to do with my daughter; could you recommend irons, or what is to be done?" Now I say to them just this: "I would recommend your daughter to observe how she walks, and to take care that the weight of her body rests on the middle of the arch of the foot. I would recommend her not to walk with her toes turned out, but with them as

straight forward as she can, for that is one of the things which gives cause to the weight to fall on the inside of the astragalus." In general you observe dancing ladies with their toes very much turned out, and I say, O, you must give up dancing, and do not turn your toes out at all. Then another thing to be attended to is, that you may heighten the inner quarter of your shoe, and that will help your weight to fall on the middle of the arch of your foot. You know that putting a strap round your shoe, and fastening that strap round your leg, is tantamount to the inner quarter of the shoe being heightened. If those things do not succeed in preventing the evil, why then you may have recourse to machinery, to irons, and so on; but I can tell you, after having seen a multitude of those cases, there was no necessity for irons at all, and by merely attending to those simple rules, and avoiding letting the weight rest where it ought not to have rested, they have grown up with ankles that were not materially wrong; the weight became transmitted to the astragalus, very much as it ought to be.

Now the bones of the leg may be dislocated forward upon the arch of the foot; in that case it is not very difficult to reduce the dislocation; extend the foot, bring it forward, and press down the bones of the leg; and if you know what you are to attempt, common sense will tell you how to set about it.

But there is another dislocation which I think worse, and that is where the bones of the leg are thrown backward upon the narrow part of the astragalus. Now that indeed seems to me to be the only case of difficulty, for you see the muscles of the calf of the leg, by drawing up the heel, tend to keep the foot extended in a very great degree; there is something in the form of the bones which tends to keep the bones of the leg in this improper situation; and if a man recovers with the bones here, the support of the weight of the body will be very imperfect, and the use of the foot also very imperfect, without any freedom of motion. Now to keep the bones of the leg upon the broad part of the astragalus, is a matter of considerable difficulty; I know, however, that it is to be done, and I have the satisfaction of saying, I have seen several patients turned out of this hospital, with a very right bearing of the leg upon the astragalus, who had had this sort of dislocation.

Now, with respect to the *tarsus*. I have seen cases where

the bones of the tarsus were subject to separate diseases, and puzzling diseases too. When there is a disease in the tarsus you cannot examine it, because the parts are swollen over; where there is a disease in the joint, and where the astragalus covers over the *os calcis*, there you cannot see the disease; there are no external signs of it, but the patient is very lame. Upon striking the heel-bone, and jarring these joints of the foot, the patient has complained of pain in them, and by that I have judged of disease in them. I remember a man who jumped out of a gig, hurt his foot, and the round head of the astragalus protruded. He came to town, and I tried to reduce it; I tied a cordage to the foot, and pulled it, but all in vain, and I was very much vexed at it. Well, I asked another surgeon to be present at another attempt of the reduction of it, and I did the same thing in his presence, but without accomplishing it. Then said the man, afterwards, "I thought I would let you try, for I thought you would do me an essential good if you could get that bone reduced, but it has been out for a very long time." It turned out, therefore, that the astragalus had been on one side for a long time, and that from jumping out of a gig the man had hurt it; but there was nothing extraordinary, and as you know all parts in the course of time will grow into a new modelled way, there was no possibility of reducing it.

DISLOCATIONS OR FRACTURES ABOUT THE ARCH OF THE TARSUS.

I have nothing to say about them. The fabric is exceedingly strong, and yet, nothing will resist extreme degrees of force. I have seen the tarsus broken, and the bones smashed all to pieces; I cannot say what the nature of the accident is, and I am sure I know not how to replace them. Yet in those cases, they have done moderately well, and people have had tolerably good feet to walk on. There was a medical man who called on me the other day, complaining that he had no spring in his foot. Now he had broken both his astragalus and his os calcis; I said that instead of complaining, you may congratulate yourself that you have such a foot to walk on as you have; it would not do for a dancing master's foot, but what has that to do with it?

Then, I say, there are cases where a part of the tarsus has

been knocked out, so that you can feel it projecting, but as to getting it in again, that is perfectly out of the question; the muscles seem to close the vacancy in which it should be lodged, and I really cannot tell how to get it in again; it is impossible to get it in. Well, how are you take it away? Aye, that is the question. Then I say, I would not, nor would I have it done. I would keep my foot still, and keep down inflammation, and if that protuberance became a matter of annoyance to me afterwards, then I would have it taken away, as a separate operation; I have seen a great many of these cases, but I have never seen any patient yet, who was willing to submit to a separate operation to have the bump taken away.

JOINTS OF THE TOES.

Now as to the joints of the toes, the grand object of knowing these joints, is to understand the amputation of them. In amputating the joint of the toe, you bend the toe downward, then cut away the lateral ligament, then you may dislocate the bone, and see what other ligaments are to be cut. But here you are to be aware that these are the largest *sesamoid* bones in the body; and therefore, in amputating this joint of the great toe, for instance, you are to make up your mind, as to whether you will take away or leave the *sesamoid* bones; if you take them away, you are to carry your knife toward the tarsus, but if you leave them, you are to cut close to the first bone of the toe.

Now, one thing more, gentlemen; I have now gone through the whole of the skeleton, but have not said one word about the holes by which the *medullary arteries* enter the body. I do not consider the subject important, but I think you will understand them better by mentioning them altogether; besides, writers have made remarks upon the subject. Then the medullary arteries are found in the *os brachii*, and in the *os femoris*, two thirds from the top, and one third from the top, and one third from the bottom, the canal sloping upwards; in the fore-arm, and in the *tibia* and fibula one third from the top, and two thirds from the bottom, the canal sloping downwards.

ON THE MUSCLES OF THE ABDOMEN.

Enough was said, in the Introductory Lectures relative to the structure and office of muscles, to enable a person who pretends to teach anatomy, to go on showing the muscles that are found in the different parts of the body · and I shall begin with those muscles which are the most apt to decay, namely, those in contiguity with the viscera. It might be said, take the simple muscles at first, and these are unquestionably not the most simple in the body ; but I always like to begin with the most difficult parts first, because if you once master them, all the rest is easy. And as to talking of difficulty, why there is absolutely no such thing as difficulty about anatomy; at least all that we know of it is easy, and not difficult.

Well, in describing the muscles, we make use of the words, *origin* and *insertion :* now, these are not good words to be used; we ought rather to say, the *attachment* of the muscles. But people will make use of these expressions, sometimes applying the names to the actions of the parts. Muscles, however, in acting, are not conscious of what they do ; they simply contract at one part, and bring the more moveable part to which they are attached towards that part at which they do contract. Now, notwithstanding these terms are inaccurate, yet, really, we should *boggle* very much in the description of the muscles, if we were not to use them ; and therefore they may be, for the sake of convenience, continued. Now the muscles of the abdomen are ranged in strata at the sides of the belly ; there are three strata, and the fleshy portion of those terminates in sheets of tendons—sheets of sinews, which are technically called *aponeuroses ;* and these aponeuroses separating, enclose others in a sort of sheet. Well, then, we shall begin with the external stratum ; and when you see it exposed to you, you will find that the muscle runs obliquely. It has been called *obliquus descendens abdominis ;* but that depends upon which part you take the attachment to be at. However, there is no objection to the term, and it is described as follows :—

Mr. Abernethy went over the description of the muscle. and then said :—

Well, there is the description of the muscle. You may get the descriptions much more accurately than I teach them ;

and then you may learn them more minutely afterwards, if you please. But now, after this general description, I come to certain particulars that it is important for surgeons to attend to; and one of those particulars is, the formation of an aperture through which the *spermatic chord* passes in the male, and the *round ligament* of the womb in the female subject. Now, to know how this opening is formed, it is necessary that you should understand how the aponeurosis is formed.

Mr. Abernethy described the aponeurosis, and continued:—

Now, a knowledge of this opening I hold to be a very important point: ruptures are often taking place there; and what would you think of a surgeon who would take hold of a rupture in this place, and who would use all his might to push it in through the very walls of the belly, to the belly itself. Then it is important to know how to find that aperture; and you may always know that, if you recollect these anatomical facts. I will venture to say, there is no subject so fat, that you cannot distinguish the angles of the pubes, and the jutting parts to be found there; then, knowing that by poking your finger along the bones, near to the angle, push it a little upwards, and then it goes into the abdominal ring. Let the skin be off or on, and I do the same thing with my finger. Now here I begin to say, what I shall often repeat, I dare say, that it is really necessary, as far as I am capable of determining, for gentlemen to come to London, or some large school, where they may have opportunities of dissecting and understanding the structure of the body: but there is a great deal of anatomy, and the very best part of anatomy, for *practical* purposes, that you may always remember without a subject, by recollecting that you continually carry about with you a subject in your own persons; and, egad, I hope you will all put your fingers into your abdominal rings, and learn for yourselves. And if you reduce a hernia by piecemeal, poking it in bit by bit in this aperture, then that is the mode of proceeding.

Mr. Abernethy gave a further description of the aponeurosis, as being connected firmly to Poupart's ligament.

Well, now I have described the muscle, and dwelt most upon those parts which seem to me to be of most importance, and now then for the *use* of it:—

What is the use of the external oblique muscle? Why, no one can tell what use it is of, but upon the supposition, that one part of it is fixed and the other moveable. Now grant

that the pelvis is the fixed, and the ribs the moveable parts, why it will unquestionably draw down the eight ribs to which it is attached, and by so doing it will be an expiratory power: but having drawn them down, it will bring the ribs to the pelvis, and it will bow forward the whole front of the body. But granting that one muscle acts with superior power, or without the power of the other, then it must be evident that this, the external oblique, will draw the ribs towards the front of the body, behind the trunk indeed, but behind it with an inclination to the opposite side. Now all this is upon the presumption of the pelvis being the fixed part. You have seen a little tumbler throw himself on his back, and cant his pelvis over his head; and no doubt he used the abdominal muscles in doing that. I say the effect of the actions will be various but it is utterly, utterly impossible for any one to understand the actions of the diaphragm. I never attempt to speak of the actions of the abdominal muscles, until he has seen the the use of the abdominal muscles at this first demonstration, and therefore you must postpone the full knowledge of that subject till you have seen the diaphragm.

Well, now, so much for the external oblique muscle; and then having examined this, the next thing you do in dissection, is to raise it. What's meant by *raising* a muscle? O, you cut away the different slips from the different ribs to which they are attached: here's a slip, here's a slip, here's a slip, which you cut away, and which you do, till you have cut away the eight slips; then you turn it back, and you expose beneath it another stratum of abdominal muscles, another stratum, another *steak* of fleshy matter, as I may call it. Here that muscle is, the fibres of which are oblique, but in an opposite direction to the others—*obliquus internus abdominis*, that is the name of it.

Mr. Abernethy described the muscle. So far for the general description of the muscle; and then, as to what I call the most important point to surgeons. Attend to the lower margin, half way up, below Poupart's ligament. A surgeon operates for an *incarcerated hernia*: he slits the ring up; he frees the stricture; he pokes back the hernia, and, egad, he feels a strong transverse portion of flesh running athwart the belly, running under the margin of that muscle. It is often necessary to pass the finger freely into the abdomen, because there is often stricture higher up than the *external*. It is at all times advis-

able to put your finger freely into the abdomen, to see that the bowels you are to return have no attachment, no adhesion to other parts, which, from inflammation, they may have. Now I say, that is important in an operation for hernia; but you are further to understand, that where the spermatic chord passes under the edge of the internal oblique muscle, it does pass through another sinewy aperture; there is a *fascia* extended up from Poupart's ligament that lines the lower surface of the abdominal muscles; it is a firm fascia, and has an aperture in it for the transmission of the spermatic chord, just half way up Poupart's ligament. This is called the *external ring*; that you will understand; and there is sometimes stricture there. But what I want you now more to consider the subject for, with respect to the physiology, is this: you see the spermatic chord, a large chord, is not made to pass right out of the belly through any one aperture. If it had, what would have been the consequence? A weakness in the walls of the belly about that part, and in the straining actions we are perpetually undergoing—egad, some of the contents would have burst out. But not so; the spermatic chord goes through an oblique canal from the internal to the external; it is called the *inguinal canal*. You have sometimes hernia in the aperture, which never comes out of the ring, and that is called *incomplete*.

Again, you may have hernia coming out of the external ring, which is not strangulated by the canal.

Moreover, you may be called upon to tie the *external iliac artery*, before it passes down under Poupart's ligament. Well, but I do not think you can get at it while this muscle remains stiff and firm; and it is necessary, therefore, to divide it, that you may be able to pass your finger down, and clearly ascertain it.

Now, having dwelt on this, what is the *use* of the internal oblique muscle? It being fixed to the ribs, and as it draws the ribs down to that part to which it is attached, so far it is an expiratory muscle; but as the fibres go obliquely in a contrary direction to those of the external oblique, so the left internal oblique, and the right external oblique, will join in twisting the body to the right side.

I have no more to say about that muscle, and therefore raising it, we bring into view another stratum of muscle, the fibres of which are not oblique, but transverse; and so it is called the *transversalis abdominis*. The transversalis and the

internal oblique are so connected together as to make it very difficult to separate them, and to say what portion you should leave to the internal oblique and what portion you should leave to the transversalis.

Mr. ABERNETHY described the muscle more particularly.

Well, here is a muscle that will not move the pelvis to the trunk, or the trunk to the pelvis, and what can it do? O, simply—simply embrace and support the abdominal viscera. It does that which the other muscles do, but this is its especial office, to support, and occasionally to compress the abdominal viscera.

Now there would be no use in raising this muscle, for if we attempted to do it, we should find no flesh beneath it. What should we find, then? A membraneous bag, which seems to hold the bowels—the bag of the *peritoneum*, that is what we should find—the bag of the peritoneum, with cellular substance on the outside of it; and if I look where the spermatic chord passes out, I shall find at that part that there is a sinewy substance lying upon Poupart's ligament.

Now, then, there was one muscle which I omitted to mention; where the spermatic chord goes under the edge of the internal oblique, muscular fibres are sent off from it, to cover the spermatic chord, which runs down in front of the spermatic chord; and, as they descend, gradually separate round the spermatic chord; but when they come to the testicle, they separate and encompass the testicle, as in a bag: you will see that when you come to examine the testicle. This is called the *cremaster*, or suspensory muscle of the testicle. That it can have this effect, no one can doubt. Every one will admit that Poupart's ligament is the attachment of the muscle, and that the testicle is the moveable part. But then it is a muscle not under the command of the *will*. It certainly cannot be said to support the testicle by any will. There are people in ill health where the testicle hangs dangling half way down the thighs. The testicles seem to be very important parts, and this muscle does seem to act involuntary. It is curious that in an ancient statue, this action of the cremaster has been expressed by the statuary, which only shows what observing fellows those Greeks were. It is a muscle which acts at the time of copulation, and under the excitement of the venereal desire. It draws the testicle up towards the abdominal ring, and compresses them, as it were, to urge forward the fluids. I say

it is a muscle, however, over which the will has no influence. I have known it act involuntarily and spasmodically, to the great annoyance of the patient. I remember one patient who had *hernia humoralis*, and, egad, this cremaster, every now and then, drew up the testicle, squeezed it, and kept the fellow bellowing out for an immense length of time. It is a suspensory muscle, and it acts in compressing. Now, I never tried to make any minute examination of the nerves that supplied this cremaster muscle, for I did not think it necessary: I am perfectly well satisfied, they are filaments of the lumbar nerves which supply the muscles in the neighbourhood; but these very same nerves supply muscles over which the will has the the most perfect command, and yet they supply those over which it has no command. If you prove to the contrary, you will show us something that may be done, I suppose. For my own part, I do not know, but I believe it is supplied by the branches of the lumbar nerves, passing between the strata of the abdominal muscles.

Now then I have done with the three strata found at the sides of the belly.

Mr. ABERNETHY then described the *rectus* muscle.

Besides, there are three muscles about this part in a sheath, attached to the *symphysis pubes*, and going up to be connected to the *linea alba*. They are called *pyramidalis*; there are, however, no such things in the subject before us, and as they are sometimes wanting, they must only be *coadjutors* to the other muscles, and are not of much importance, or they would not be wanting, you may depend upon it.

Well, now, what is the *action* of the rectus? It supports and compresses the abdominal viscera. Well, and what is the use of those transverse bands? Probably to allow the different portions of the muscle to act separately, for I think you may put the upper part of the muscle into action without engaging the whole of the muscle; for instance, you can bend your body forwards and take in part of the muscle, but the whole of it does not seem to act at the same time, which probably it would do if it were not for these transversed bands; and I have observed, a man put the operation of the muscles, on both sides, into action, and exhibit an appearance which was perfectly astonishing to all his medical attendants. The man was a madman, and, which is no uncommon thing with such persons, he thought he had got some monster in the inside of

him ; and when a man has once got that notion into his *noddle*, the very growling of his guts is as a tremendous rumbling to him. But this man could not only hear his guts rumble, but he could see them move. He put his hand upon this part of his body ; some thought he had an *aneurism* ; what he might have done I do not know, but I am convinced that this was the only rational solution of the phenomena.

Well, now, this rectus muscle goes down in a sheath, and it is connected to the sheath by nothing more than cellular substance, save and except where those transverse bands join the cellular part ; so loose is it, that I can put my finger behind it and strip it up.

Well, now, wherever matter forms beneath an aponeurosis, it is a case that should, and that does call for the surgeon's attention, because an aponeurosis, a sinewy expansion, is, by nature *indolent*, and it forms a sort of separation between the matter and the skin, and if the skin do not break, the matter will go on separating the cellular substance by which it is confined to the sheath, to an enormous extent. Now I am perfectly convinced that I once saw a case of this kind in an hospital in this town. A woman, who had been a physician's patient, had had very severe pains about the stomach ; the symptoms were rather abated, but she could not be prevailed upon to get up and move about ; what was the nature of the disease the physician could not very well make up an opinion ; she still lingered, as it were, in bed, though she acknowledged that she had no violent pain, but only was unable to move. A considerable lapse of time took place, and then the lower part of her belly grew bigger on one side ; she remarked it, spoke of it to the physician, and the physician said, you must let the surgeon see this. The surgeon came, saw it, and said there is a *fluctuation*—a fluctuation ! Why, put a lancet into it. The lancet was put into it, and out came an enormous quantity of matter. The surgeon made a good gap, put his finger into it, shut up the wound, put the patient to rest in bed, and said, you must keep yourself very quiet, for he thought there was some horrible mischief likely to ensue from his operation ; but the woman got well, and there was an end to it.

I will turn down the rectus muscle, and that will show you what seems to be important with regard to this sheath, which is, that it will show you the *front* of the sheath is very *thick*, and the back part of it not near so thick ; it will show you

also that the back of the sheath is very thick above and very thin below. I say that is important. They may make what *fuss* they please about the suppuration of the aponeurosis, but this is important, that the surgeon should remember the sheath is much thicker in front than behind; but it is thick above behind, yet as you go down there is very little sinewy matter, so that you see through it. The different degrees of thickness of the surface is what every body should remember; but I have turned down this muscle for another purpose still. If you wish to see the situation of the *epigastric artery*, you will never see it so well as now; if you wish to have a distinct view of that, you must not look at an injected subject, but you must dissect a subject for the purpose. It is to be met with at about two-thirds from the inner edge, and about one-third from the outer edge of the muscle; this is the main branch of the epigastric artery. Well, these vessels you should very carefully avoid in *tapping* a patient. I will venture to say that I have known wounds of this artery, in tapping, *thirteen* times in the course of my life.

People, when I was young, always used to tap where they were likely to wound the epigastric artery. What was the rule for tapping? Some said, wishing to tap in the linea alba, you are to tap between the *umbilicus*, and the superior anterior spine of the ilium; accordingly, they used to tap there frequently, and hence came those wounds. It happened that a surgeon in this town, going on in the old practice (he an anatomist, too,) did wound the epigastric artery; he drew off the water, and knew nothing of what he had done. The woman died of internal hæmorrhage into the abdomen. The surgeon afterwards had the manliness to tell the case, and every body after that time tapped in the linea alba, where they could have no chance of injuring this artery. But you cannot always tap in the linea alba; you have to tap in dropsy, in the *ovaria*; you must tap where the fluid is; you must tap at the side, but if you do, take care to keep fairly on the outside of the rectus muscle. How shall I know where that is? Why it is a very difcult thing to say, for the rectus muscle yields, in a most uncommon manner, over any accumulation in the abdomen. Water accumulates in the abdomen, and the rectus muscle spreads itself over that water. It is really a very curious thing to see this muscle spreading itself over some tumour below it. I have seen the *sartorius* muscle spread out to a most uncommon breadth, in the case of a tumour in the thigh. The rectus

muscle also does it in a most extraordinary manner, and we make ample allowances for it. You must not suppose, if you go on tapping at the side of the linea alba, that that will be sufficient. What I tell you is a fact, which I am sure you will all see and feel at once, that the rectus muscle yields in a particular manner. All parts do not yield in the same manner: you do not see the parts in fat people yielding in this way; you have seen *Punch's* belly pretty fat, I am inclined to think; and what is it that yields there? Why, the muscles are kept in, but the front of the belly yields, so that Punch could lay his belly on the table. And, therefore, I cannot tell you exactly where you are to tap at the side; but, I say, make ample allowance for the yielding of the rectus muscle.

Linea semilunaris (Mr. Abernethy described this). Well, what is the linea semilunaris? What is it but the three sinews appearing together without muscles intervening on the outside of the rectus? Now the rectus muscle going down, reaches the lower end of the symphysis pubis, and the outer edge gradually tucks in; so that it is the *tucking in*, as I have called it, which occasions this semilunar sweep, in the white appearance. *Semilunar* it ought not to be called; it is but one horn, one half moon, or horn.

There are, sometimes, *ventral* herniæ; sometimes, from great exertion, those sinewy chords are rent, and, at the gap made in them, the bowels come out. There was one woman brought into the hospital, who, from the straining of parturition, tore both her linea alba and linea semilunaris; the bowels came out *en masse*, as the Frenchman said. I never saw such a thing in my life; an immense quantity of her bowels. She got sent to this hospital; where the plague she came from, nobody could tell, and it was not till after a great deal of labour and attention that we got a great portion of her gut back through the linea alba. When that part was restored, some one put his fingers into another gap, and so the whole was reduced. Of course bandages were applied; and it was a case that could not be expected to have been got well on a sudden; but the woman did get well, and she went out of the hospital, as she thought, pretty comfortable.

Well, so much for the importance of these parts; and what I have said with regard to ventral hernia, is only said for this reason: to show how every fact of anatomy ought to be attended to, because they are applicable to every step in surgery

LECTURE XXXII.

THE MUSCLES ON THE FRONT OF THE BODY.

I SHALL proceed to give an account of the muscles that are found on the front of the body; and first, of those that are found on the front of the neck. It is a rule, in dissecting, to detach the muscles from those attachments which are most plain and obvious, and to leave them attached to those parts which are the most obscure; because you can afterwards examine those parts deliberately.

Here I find the *subcutaneous* muscle. Animals have those muscles generally, by which they wrinkle the skin and strike off any thing that may be annoying them—frighten away any fly that may be disposed to bite them. But as man can apply the hand to every part of the body, there was no need of this subcutaneous muscle; yet there is here a fleshy thing that bears the name which Galen gave it, *platysma myoides.* This muscle is stronger in front than in the back part: much need not be said about it. As to its *action*, it tugs up the integuments beneath the collar bone, and tugs down the integuments of the face.

Then there are certain practical observations respecting muscles; which I deem worthy of attention; and one is, that the *external jugular veins* run down beneath this muscle; and if you open an external jugular vein, you had better carry your knife in the direction of the fibres, and not go against them, otherwise unpleasant circumstances may be produced. Also I know, that where there are *cysts* in the neck, in the situation in which I have often seen them, that the tension of the fibres gives a firmness to them, which would make you think there is something solid where there is actually nothing but fluid. However, this muscle is raised, and then you see beneath it another, which is a very remarkable and a very important muscle, a muscle which comes from the *mastoid process;* its tendon is partly attached to the process, and partly spread over it, and it is named, from its attachment, *sterno-cleido-mastoideus.*

Well, what will that muscle do? Of course, bringing the mastoid processes towards the *sternum* and collar-bones, the head is brought forwards. But suppose one of the muscles will act and the other will not; suppose the right one does act and the left one does not; why then that will bring the right mastoid process down, and twist the neck. Again, the muscle may act in a sort of irregular manner, and, without approximating the attachments, it may bring them opposite to one another; for instance, if I turn my head to one side, I do it with this muscle, yet I do not make the muscles approximate. Now this case, with respect to this muscle, seems to be well known to every artist—to every gentleman who has a taste for the arts. In making a figure of the head, turned round, what do you see? Why, every gentleman understands that it is the sterno-cleido-mastoideus which presents itself to view, and it would be shameful for a medical man to be ignorant of it.

Now for practical remarks:—There is a *wry neck*. Cases of wry neck are no uncommon occurrences, and they seem to indicate undue actions of the sterno-cleido-mastoideus. I have always said, in these lectures, that a very pretty little book might be written on wry necks; for sometimes inflammation in the glands, in the neighbourhood of this muscle, may excite it to this irregular action. Something on the opposite side of the neck, preventing muscular action, may cause the neck to be twisted; but the muscles on the side to which the neck is twisted are not the faulty muscles.

Then, as to wry necks in general, they are the result of the irregular action of muscles; and many muscles are concerned in the production of the wry neck. Now in those cases of wry neck which result from the irregular action of muscles, if the sterno-cleido-mastoideus be chiefly affected what is to be done? Why, you must endeavour to tranquillise the muscles. There are many of those cases entirely the result of a disturbed state of nerves, as I believe, caused by disordered digestive organs: and whether this be universally the case or not, I will tell you of one absolute undoubted instance of it. This happened in a tall boy, at school; he was seized with a wry neck; they leeched him, and blistered him, and made bad worse, and, after a week, he was sent up to town. I say he was a tall, lanky boy; and, upon my life, I thought a *pullet's* neck could not have been more twisted round than his was: that, you

know, admits of a particular turn from the mechanism of the vertebræ of the neck, in order to let the fowl put its head under its wing to go to sleep. But so much was this boy's neck twisted, that I told him to lie on a sofa, or couch, or bed, and to support the head with pillows; never to sit upright; never to put the head in a position to demand muscular action for support, since, if the muscles did act, they would act in this faulty manner. I told him to foment his neck by flannels; to keep it in a kind of tepid bath; to keep himself in a kind of perspiration; and I applied the whole of my attention to put his digestive organs to rights. His bowels were all wrong—his tongue furred, and of a bad colour. A week elapsed, and his stomach and bowels got into a decent state, and his wry neck was entirely gone. But you see if this is neglected in the outset, then muscles get a habit of perverse action, and you have wry necks established beyond removal, even when the digestives are put to rights, though that I believe to be a most direct and efficient mode of cure in every instance. But I know that there are cases of wry neck, where people have their necks twisted, and the muscles put into such an irritable state of action, that the cases do not yield quickly, even to what I should consider the most judicious and appropriate treatment. But, now, in reading books of surgery, you find the proposition of dividing the sterno-cleido-mastoideus muscle, in order to set people's heads right on their shoulders. Now, I lived for a very great part of my life, and I did not know what the plague those people meant by this; I never could meet with a case where such an operation could for a moment have been rationally thought of; then, as curious cases are something like misfortunes, that is, they never come alone, in one year I met with *four* cases requiring an operation in the sterno-cleido-mastoideus. All these cases appeared to me to be of the same nature; I mean to say, it appeared to me that the muscle was originally *mal*-formed; that the clavicular part of the muscle was shorter than it should have been; it was irritable, and drew the mastoid process towards the collar bone. And note the consequence: the people—children they were, for they were not more than fifteen or sixteen years of age—had all grown wry in their backs; the vertebral column had become completely deformed. And it is natural to suppose that that would be the case, for if any thing oblige a man's head to lie pretty much in contact with his shoulder, he is un-

der the necessity of bending the vertebræ of the loins to the opposite side, in order to support the gravity of his head perpendicularly upon the *sacrum*. The inversion of one vertebra, creates the necessity for another inversion. *Hang* it, says I, these children shall not remain in this manner; so I made a longitudinal incision in the sternal portion of the muscle, cut it fairly through, and up started the head perpendicularly upon the neck. I did not touch the clavicular portion of the muscle at all; that did not seem to be at all faulty, and when the clavicular portion was set right, all seemed right; and I have great satisfaction in informing you that these children became straight in their backs. Now that is a very curious thing, that the curvatures of their spines became involved without any mechanism. In one of the children, indeed, a second contraction took place after about a year and a half. I shall not stop now to explain the process, that is explained in the surgical lectures, but I divided it a second time, resolved that the child should grow up to its full stature without deformity of the vertebral column. I have seen several cases since; and there was a student here who chose to have the operation done on his neck; not that it was in any considerable degree deformed. He would have done very well for a *courtier* in the court of Alexander the Great, where it was the fashion, in imitation of their monarch, to walk with their heads a little twisted. But, however, he chose to have it done, and done it was, and with a very satisfactory result to him; he then could do with his head what he could not have done before.

With respect to the *depressors* of the *larynx*, there are six of those muscles; but I have not stopped to mention their actions separately, for it would have been needless; they all have the same actions, namely, to draw down the larynx towards the sternum.

With respect to the *elevators* of the larynx, the first I have to point out, is a muscle which is generally known by the name of *digastricus*. As to its *action*, why, I say, we never can tell what the action of a muscle will be; it depends upon what are the fixed parts of the muscle. Admitting, then, the jaw to be the fixed part of this muscle, it will raise up the *os hyoides*; it is an elevator, then, of the os hyoides. This muscle has been somewhat dignified by a dispute between two great teachers of anatomy, namely, Dr. HUNTER and Dr. MUNRO; not the present, but the past Dr. MUNRO. Now those two

great men never would have disputed about this; if they had made up their minds to have done what I tell you you must unquestionably do; that is, first of all to determine which is the fixed part of the muscle. The dispute was, whether the *anterior belly* of the digastricus would not depress the jaw Unquestionably, if the os hyoides is the fixed part, then the anterior belly of the digastricus would depress the jaw. The jaw wants no depressor in general; it will fall by its own weight, as you will see in a man going to sleep. But suppose a surgeon was operating on a child, and he were to say, "Do not cry, you little rogue," but the child bellowed lustily, what would he do? He would draw the jaw down as far as possible, and, in doing that, there is no doubt that he would use the anterior belly of the digastricus to effect this purpose.

The *stylo-hyoideus:* They tell a trivial circumstance about this muscle, which is, that some of the *tendons* spread in going over the digastricus muscle, and others pass behind. It is a poor trumpery muscle, and it is not worth while to be talking about those minute parts of anatomy that belong to it. It must be considered as an elevator of the os hyoides.

Then there are two more muscles which elevate the larynx, the *mylo-hyoideus,* and the *genio-hyoideus.* There are, therefore, three muscles on each side to depress the larynx, and four to elevate it.

Now I remember, when I was young, people used to say, "O, it is not necessary to study anatomy, taking into consideration all these small muscles; *damn them,* they would puzzle any body." They used to call them the *little* muscles with the *long names.* Now I was pleased to hear that, for it always gave me an opportunity of saying, it would be a great desideratum to have all the muscles named the same, in one way. I heartily wish that that most laborious man, now dead, he who had such great knowledge of the languages, *Dr. Barclay,* had but made the attempt. What is the use of calling a muscle *oblique* or *straight?* Does that tell you where it is, what it does, or any thing about it? There are many muscles named after mathematical figures, to which they bear no similitude whatever; but I defy any body to hear the name of sterno-cleido-mastoideus pronounced, without imagining the muscle; it recognises the attachment of the muscle, and if you know the attachment, you know the use; and the use is, to bring that which is most moveable to that which is most fixed. What is

the use, for instance, of calling one of these muscles digastricus? Really the whole nomenclature of muscles is quite ridiculous. But you must know the muscles if you would be anatomists, and you must be anatomists if you would be surgeons. I dare say Mr. STANLEY has told you, that the knowledge of the bones is the foundation of anatomy. Aye, I dare say he has; and now I have to tell you, that the knowledge of the muscles is the next in superstructure. How is it possible you can know the arteries and veins, unless you know the muscles? Come, now, I would ask you, What is the situation of the *sub-maxillary salivary gland?* You must answer me, that it lies immediately below the jaw, and upon the mylo-hyoideus muscle. Now I ask, what is the situation of the *sublingual gland?* It must be answered, immediately under the tongue, and upon the mylo-hyoideus. The oblique direction of this muscle is the cause of the unequal depths in the mouth; the mouth is very shallow in the front, but it deepens behind; and what is that thing you touch with your tongue. It is the mylo-hyoideus muscle.

But I cannot quit this subject yet, for I know it must be said, it must be very strange that the larynx, which you call the organ of the voice in the windpipe, should have such multitudinous powers to raise it up. Does it facilitate respiration? Does it affect the tones of the voice? No, I do not think it does. It affects the note of the voice, as you will afterwards learn. Unquestionably, the note depends upon the situation of the larynx, but the tone is a totally different thing. But was it for this, that we might vary the note of our voices? O, no. Then why had we those powers? Because the bag which receives the food when we swallow, the bag of the pharynx, is attached to the back part of the larynx; so that the larynx is lifted up, that the bag should be lifted up to receive the food. Put your hand upon the larynx, and then attempt to swallow. *Gulp*—up and down it goes with the convulsive motions. Why does it go up? O, for more reasons than I can tell you at present; but we shall at present say, that the bag may be raised to receive the food. Why does it go down? O, that is to be answered in this way, because it is necessary for respiring. We are breathing at all times, and swallowing occasionally, and it is necessary for respiration that the bag of the pharynx should be brought down. Therefore have you these muscles;

and now I have done with them, but I must show you another muscle.

Pectoral muscle. What will this do? Why, if the arm is lifted up, unquestionably it will depress the arm on the side. When the arm is depressed on the side, it will bring the arm forward; and the ridge being in front, or external to the bicipital groove, it will roll the arm in. All this is said upon the presumption of the trunk being the fixed part, but the arm may be the fixed part. Have you never seen a little boy climb up a tree? If you have, you will have observed that he catches hold of a bough with his arm, and draws up his body, which is done with this muscle; sometimes, also, he lays hold of the branch with his legs.

This muscle is well known to every artist, and gentlemen having a taste that way, and it would be most unpardonable for a medical man not thoroughly to understand the pectoral muscle. And to show you of how much consequence it is to know the attachment of muscles, I mention this: Suppose a muscle to be burst, and subsequently inflamed, what are you to do? What? to approximate the different attachments as much as you can, and to let the fibres be as loose as possible, for then the inflammation will go down, and the parts will unite; but if you let the attachments be separated, and the fibres stretched, God knows when the inflammation will go down, or the parts of the muscle unite. But suppose the muscle to be divided; if you cut it, it gapes, it retracts by the living power, and you may bring the parts of the skin together by adhesive plasters, but how do you close the wound in the muscle? There is no other way but by compression; and if you approximate the fibres, then they will unite together, and the wound will be closed. There is no other way of closing muscular wounds. You may think as you please, but I am sure that no one is fit for his profession who does not understand the attachment of the muscles, neither can he learn the rest of anatomy, without understanding the muscles: do not, therefore, suffer your minds to repose, or be contented without acquiring this knowledge. They say when you take off a diseased breast, you should put the pectoral muscle on the stretch: I remember the time when they used to place a patient in a chair, when that operation was going to be performed, and put a plane under the arm to keep it stretched. They do not do the operation so now; but of course, they put the pectoral muscle on the stretch still.

Deltoid muscle. If there is any part that surgeons should be acquainted with, it is the *gap* between the deltoid, and the clavicular portion of the pectoral muscles. I put my finger into that gap in my own person, and I stop the beating of the pulse at the wrist. Suppose a man has got a bleeding in his arm, which there is a difficulty in stopping, how are you, as a surgeon, to stop it? By passing your finger over the first and second ribs, pressing the artery, and you stop it at once. When I talk about this sort of thing, and hear a medical man say he cannot do it, I always say, there are others that can. And I tell this story of a lady who was examined by a great number of *learned doctors;* they could not tell what was the matter with her; they had *never* met with such an *intermittent* pulse in the whole course of their lives, nor any thing at all to equal it, and they pronounced their opinion that she could not live a moment, and left the room; and *egad,* all this time she was *tweedling* with this artery at the shoulder.

Now I will only just say, with respect to *system,* that there is a great deal in that. In acquiring a knowledge of any science, system is almost indispensibly necessary; but it is especially so in the medical profession. With this view, the abdomen is divided into *regions;* and from want of attending to this, I have known something of this nature occur elsewhere: the examiner has asked a young gentleman who presented himself for examination, "What do you see upon opening the abdomen?" The chap has stared, looked aghast, and stuttered out, "wh—wh—wh; what—what do I see, Sir?" "Aye? "Why, th—there's there's the *liver,* Sir." "Well?" "An—an—and there's the *stomach,* Sir. An—an—an—and—and there's, there's the *spleen,* Sir. And there's the *cæcum,* Sir." Now, then, just let me tell you, that you will do well to proceed *systematically* and to *pack* up your knowledge as you go along, in such order, that when you are asked for it, you will at once know where to find it.

ON THE MUSCLES OF THE FRONT AND SIDES OF THE BODY.

Diaphragm.—With respect to the *action* of the diaphragm: when the fibres of the great muscles contract, they lose their curved form, become straight, and with this alteration in their direction, there is a descent of the muscle from the chest

towards the abdomen. As the diaphragm descends, air rushes into the cells of the lungs, and in this way, principally, the enlargement of the chest in a vertical direction takes place. When you know that the lungs are at the side of the chest, you will easily understand that the diaphragm descends principally in its lateral portions, and physiologists have doubted whether the central tendon descends at all. Perhaps it may be admitted, that in an ordinary inspiration, there is no descent of the central tendon. I have already told you, that the heart rests upon this, and if it were to move, the heart would be jolted upwards and downwards. Now I believe, that in an ordinary inspiration, the central tendon does not descend, but that in a deep inspiration it does descend beyond all doubt. In this way, then, is the diaphragm the great agent in inspiration, as far as the greater muscle is concerned. But what does the lesser muscle do? How is it concerned? The lesser muscle will draw the central tendon downwards and backwards. With regard to the action of the *lesser* muscle of the diaphragm, as to its influence upon the *œsophageal* opening. Upon the openings for the *aorta* and *vena cava,* it can have no influence, but upon the œsophageal opening there is an influence. *Laughing,* for instance; that is an action of the diaphragm. Again, we seem to possess the power of closing the termination of the *œsophagus* in the stomach. How do we do this? By putting the fibres of the lesser muscle into action; in this way we explain the possibility of stopping the eructation of fœtid air from the stomach.

In the next place, you are to consider how the diaphragm *alternates* its action with the abdominal muscles; how, by this alternation, expiration and inspiration are produced. In inspiration the diaphragm descends, the abdominal muscles relax, the air is brought into the cells of the lungs, and the muscles being relaxed, the viscera are brought downwards, and the diaphragm enlarged; but in expiration, the viscera are pushed upwards and inwards; the diaphragm retracts, the lungs are compressed by the contraction of the abdominal muscles, the air is expelled from the air cells, and thus is expiration effected.

But there are occasions on which these two great powers are combined, the muscles of the abdomen and the diaphragm, which we call straining, and which is of great advantage to us. at the time of passing the *fæces.* Under such circumstances

too, it is likely that hernia may occur. There are other phenomena produced by what we call modified actions of the diaphragm.

Hiccup.—In what does this consist? To explain the thing satisfactorily, I ought to be able to hiccup; but perhaps it will not be necessary for me to do so. It is a short convulsive inspiration, followed by a very quick expiration, which, in its progress through the lungs strikes the sides of the cartilaginous box containing them, and thus the noise of the hiccup is produced. Sometimes hiccup is attendant upon disease; and hence in strangulated hernia, it is an unfavourable symptom, as indicating mortification of the intestines. *Laughing* is another action of the diaphragm; what does it consist of? A quick succession of short expirations; and if the laugh is powerful, you know it is combined with a motion of the chest and abdominal muscles. To explain this, if a jolly fellow is laughing heartily, you will see his chest and abdomen moving in a very powerful degree. *Cause* of laughter.—Partly physical, and partly moral, you will say. Physical cause—tickling certain parts, such as the soles of the feet, palms of the hand, axilla, and so on; but perhaps you will say, it is not altogether physical, as the effect will in part depend upon the tickler. *Sighing.*—A deep inspiration; and it gives us ease frequently in cases of distress, in a way which is not easy to explain, It is thought that by sighing, a relief takes place in the vessels of the brain, but that is doubtful. And there are various other phenomena, such as *crying, sneezing,* and so on, resulting from what we call *modified actions* of the diaphragm.

Psoas magnus, Iliacus internus.—As there is a single tendon proceeding from these muscles, we speak of them as one, and we may say their principal action is to bring the thigh forwards towards the trunk, with reference to the *trochanter* minor; and at the same time that they bring the thigh forwards towards the trunk, they will turn the whole of the lower extremity outwards. In this way are they concerned in *progression.* You may say, that they also contribute to maintain the *erect position* of the body, in which there are numerous muscles concerned. With regard to this insertion at the trochanter minor, it is well to remark what will take place when a *fracture* occurs in this part of the *femur;* when that happens, the upper part of the femur will be drawn up. In any case of fracture at this part, it is a great object to prevent the draw-

ing up of the trochanter minor; for if that be not prevented, great deformity will arise; and the only way to prevent that, is to bend the trunk as far forward as possible, that these muscles may be relaxed.

Quadratus Lumborum.—The action of this muscle will be that of drawing down the last *rib*; it will bend the loins to one side, and both muscles acting together, will assist in maintaining the trunk erect.

Pectoralis minor.—The action of this muscle will obviously be, that of drawing the coracoid process, and whole shoulder downwards and forwards: as it does that, it will carry the inferior angle of the scapula backwards: thus you will consider the pectoralis minor as one of those muscles contributing to give the scapula its rotatory motion from the chest. This supposes the coracoid process to be the fixed, and the ribs the moveable parts; but then the shoulder may be the fixed part, and then it will become an inspiratory muscle, by raising and drawing outward the several ribs to which it is attached, thus contributing to enlarge the cavities of the chest.

Serratus magnus.—To understand fully the action of this muscle, you should have pictured before you the fibres of the muscle. It is to draw the whole scapula downwards, and to fix it most firmly against the sides of the chest. Looking at the skeleton, you would say, the scapula is not fixed at all; but then in the subject it is fixed most firmly by the muscles, and especially by the serratus magnus. What is it that supports the shoulder when you see a man carrying a large weight perhaps of 100 pounds upon it? It is this muscle. This account supposes the ribs to be the fixed, and the scapula the moveable part; but it may be just the reverse. At a country fair you see a mountebank submitting to have an anvil put on his chest, and a horse-shoe hammered upon it; what is it that enables him to do that? Is it the elasticity of the ribs? Yes, to a certain extent. Is it the cartilaginous nature of the chest? Yes, to a certain extent; but it is especially this muscle, the serratus magnus. During the time Dr. Hunter was teaching anatomy in London, there was a man that used to go about the town performing this feat; he would lie down in the road, let a carriage wheel go over him, and get up again, without its having hurt him: the name he got was certainly a very appropriate name; he was called *Leather coated Jack.* It happened that when he died, he was taken to

Dr. Hunter's theatre for dissection, and the only difference between him and any other person was the enormous thickness of his *serratus magnus*.

ON THE MUSCLES OF THE FACE, EAR, EYE, AND SCALP.

Now in speaking of the muscles of the face, they perhaps by some might be considered not of much consequence, but the subject is interesting physiologically; and more, I should say, philosophically; for these muscles are so much under the influence of the mind, that they impress permanent lineaments on the countenance. Muscles grow precisely as they are used; they produce prominences by their bulk, on one part of the face, and depressions on another part; by their action they fold the skin, and, in short, all these things give a character to the countenance. I remember that a very great painter told me once, that a very great change indeed had been produced on the countenance of a very celebrated man: he said I painted his countenance once, and did it very well; I painted it lately, but I found a very great difficulty in drawing it.

Now, with regard to the muscles of the face, it is necessary that the lips should be moved for the articulation of our words, and it is necessary that the cheek should be moved in mastication; and I shall begin with the muscles that move the lips, and which ordinarily terminate in the corners of the mouth.

Buccinator.—The question will be, what will be the effect of the action of this muscle? Why, supposing the bone to be the fixed point, it would draw both the lips back towards its fixed attachment, and this produces an unpleasant distention of the mouth to one side. The muscle acting in this way distorts the mouth, and produces a kind of unpleasant grin, which has been denominated the *risus cynicus*. But it is possible that the lips should be the fixed parts; and what will be the effect of the buccinator then? Then it will impel whatever distends it into the mouth. O! we use it in masticating, and also in blowing. This is the reason why it is called the *trumpeter muscle*. I might have quoted a better use of this muscle for the assembly that I am now addressing; I mean in the use of the *blow pipe*: I do not doubt that many of you are, and that all of you may become, mineralogists; and you know if you have a little bit of stone, and you should wish to know whether there is any mineral in it or not, you have not large fur-

naces for smelting it, but you keep the blaze of the candle constantly upon it. To see a man try to do this through the means of the blow-pipe, for the first time, is truly ridiculous; for he keeps puff, puffing, and applying the heat remittingly to the stone; but a man used to it, will keep the light steadily blown upon it, which he does by the use of the buccinator.

There is a great difference in the dissection of the mouth of a man, and that of an animal. In an animal the muscles simply draw the mouth in various directions; there is none of that complication of muscles, capable of producing contrary effects to one another, which there is to be found in man; but all these muscles were necessary for us for the articulation of our words, and that is the chief peculiarity of the mouth.

Constrictor Narium.—This is a muscle which draws the *alæ* of the nose downwards; and it must be a very powerful muscle, as you must be convinced from your own feelings. It is used in sneezing, and also in smelling fine odours.

Orbicularis Palpebrarum.—The tendon of the orbicularis palpebrarum is very important; the knowledge of its situation is what gives you the power of opening the *lachrymal bag* with the greatest facility; the knowledge of its situation is what gives you the power of performing the operation for the *fistula lachrymalis* without the slightest difficulty. You put a knife below the orbicular palpebrarum, keep it within the ridge of the orbit, and where does it go? into the lachrymal groove, and you carry it on till you can carry it no further, for it is resisted by bone; you divide the bag, put the probe down into the nose, clear away any obstruction that may be there, and you will find that your probe rests upon the nasal process of the superior maxillary bone.

Corrugator Supercilii.—The eye-brow admits of motion; there is no especial muscle for raising it; it is raised up by a muscle that moves the whole of the scalp; but there is a special muscle for depressing it; it comes down from the roots of the *ossa nasi*, and fibres go up to terminate in the integuments beneath the eye-brow; it draws the eye-brow downwards and towards the root of the nose; it is called the corrugator supercilii.

Fronto-Occipitalis.—Now I have done all to the muscle which moves the hairy scalp, and this is a muscle which it is difficult to demonstrate clearly; but when things are not to be

seen by the eye, why, they must be seen by the understanding —the mind's eye. Here then is a considerable muscle, called the *fronto-occipitalis*, which has an *aponeurosis* covering the whole of the scalp. Suppose an Indian was to scalp the person whom he had vanquished, what would he do? Why, cut a part of this muscle, twist the hair round his fingers, and tug it off, just as you have seen a boy pull up a sucker. In the first part of Mr. Pott's book, he states cases in which a cut upon these aponeuroses is necessary. He tells you he has known patients who had got a puncture in the scalp from which they suffered intolerable pain—extreme agony; that he had seen them in a state of fever, almost amounting to delirium, but that they had been instantly relieved by a cut into the *fascia*. I have not seen any such case, but I cannot but believe that there are such cases. A small wound will cause them, and a large wound will relieve them. Just so it has happened when people have got their heads jammed between a cart-wheel and a wall; the whole scalp has been taken away and a most hideous appearance presented.—Again, blood is often shed upon this aponeurosis; and Mr. Pott used to say, in his lectures, the first time you meet with such a case, he was much mistaken if you did not think that your patient had fractured his skull, and that part of the skull was depressed. Now I say the same thing. Where blood is shed under the aponeurosis, the edge of the aponeurosis seems hard, irregular, and like the edge of a broken bone, and the middle seems soft, so that you would think you can almost depress it at the supposed broken part. You may say, I can never be deceived in this manner; but I can only tell you, that I have seen very able surgeons deceived. I remember, not very long ago, being called upon by a surgeon of very eminent knowledge, to attend upon a gentleman's servant who had been knocked down, and this surgeon was persuaded there was a most extensive fracture in the *cranium*. He says, "feel it, Sir, feel it." I said, "O, I do not trust to my fingers; but I do not believe that this man has broken his skull, because he does not show any symptoms to indicate any such serious injury; and, granted that he had broken his skull, I am sure I could not venture to trephine, except under the certainty that by doing so I should produce more serious symptoms than are now apparent: he must be bled and purged." He was bled and purged, and did well. It was a considerable time before the blood was absorbed. I

got him into the hospital here, where he lived for a considerable time, and at last got well.—When the blood was altogether absorbed we could examine the bone, which we did, and found that it was in its proper place.

I have known instances of *matter* forming under this aponeurosis, and I remember a case which a surgeon had, a surgeon of no very great talent, and it was a case which he called a very curious case, and to me it certainly was a curious case; it was the case of a child whose head was of a very uncommon form; when it was touched it was all soft. The matter had begun in one part and had spread beneath the whole of the aponeurosis. I said, there is a fluid under it; put an abscess lancet into it; he did so; there was an immense quantity of fluid escaped, and the case did perfectly well. Now I say, in any case of matter formed beneath fascia, you should not wait to have it spread throughout the cellular substance, until it detaches, which it will do if not prevented, the subjacent parts to a very considerable extent.

Retrahentes Aurium.—With respect to the muscles of the ear; the external ear can be lifted up by them, and drawn backward and forward; but, to say the truth, these muscles are very often inactive. We have the powers given to us by nature by which they might be employed, but it would be considered very inelegant if we were to make the pinnæ of the ear stick up. We bind them down with bandages and night caps, and they are never employed. But I remember a very eminent Professor, who could use his pinnæ in an extraordinary manner; I have seen him prick his ears amazingly. He was very touchy, and we could not ask him to display his powers; but I have seen him use these as I have told you. Whether he considered it as an *asinine* property or not, I do not know, but he would never have it spoken of. We disuse them because we live in a civilised society. We have no occasion to listen to the noise and approach of wild beasts, but savages have, and they use their ears for that purpose. It is apprehension which is the great excitement to attentive hearing. O! it is apprehension. And here I would tell you a little piece of jockeyship: if you buy a horse, if that horse is run out before you, and if the horse pricks his ears at every thing before him, if the man who sold him was to swear till he was black in the face that the horse was not a timid one, I would not believe him, because it would be a sure sign that the horse is timid.

LECTURE XXXIII.

THE MUSCLES TO BE FOUND ON THE BACK OF THE BODY.

Trapezius.—I HAVE spoken of the *ligamentum nuchæ*, as being connected with the trapezius, but here it is necessary to explain what that is: it is really very insignificant in the human subject; but there is an elastic ligament, of a tough yellowish substance, very strong but inanimate, which is called the ligamentum nuchæ. In animals it is very important, for it supports the head and neck when they are grazing, without the expense of muscular action. This ligamentum nuchæ you must be well acquainted with, for you must have seen it often when cutting up necks of veal and mutton. It is not necessary for the support of the head of man, but still he has a ligamentum nuchæ, though it is of an insignificant kind.

Now having spoken of the attachment of the trapezius, I come to speak of the use of it; and granting the posterior attachment to be the fixed part, the trapezius will draw back the scapula towards the vertebral column. But here is a muscle which may act in two different parts; there is no necessity for its acting altogether. Where a muscle is inserted into a sinew it acts on that sinew, but here is a muscle having a great breadth; the lower part of it may act separately, by drawing the scapula downwards; the upper part of it may act by drawing the scapula backwards and upwards. And, again, the portion which extends itself towards the collar-bone must be admitted to have a separate action, and that will turn the face to one side; and this concludes what I have to say about the action of the trapezius.

Latissimus Dorsi.—With respect to the action of this muscle, if the arm was elevated it would draw it to the side; having done that, it would draw it back towards the vertebral column, and then it would roll the arm inwards.

There are two great muscles which depress the arm; the pectoral muscle draws it forwards, the latissimus dorsi draws it backwards, and both concur in rolling the arm. Granting the arm to be the fixed part, then those muscles draw up the

sides of the trunk to the arm. Now here I might repeat the advantages surgeons derive from a particular attention to the muscles. If a muscle is burst, you must bind the parts together in such a way as that they will approximate; if a muscle is divided, you can only close the wound by approximating the parts, and it is always convenient, when you dissect tumours, to put the muscles on the stretch.

Rhomboid Muscle.—Action of this muscle. Why it is as plain as a pike-staff that it will draw the scapula towards the vertebral column—draw the scapula backwards. But then here is a muscle, the parts of which may act separately; the lower part, the rhomboideus major, acting separately, will draw back the inferior edge of the scapula, and I might say, that the rhomboideus minor, acting separately, would draw back the superior edge of the scapula.

Levator Scapulæ.—This is an elevator of the scapulæ, and we are to associate with that the elevation of the shoulder joint.

Now here I show you three muscles to day intervening between the trunk of the body and the bone of the shoulder, namely, the trapezius, rhomboid, and the levator scapulæ. Let me speak of them as one, and let me call upon you to remember that there were three described on the other front of the body, intervening between the trunk and the scapula, namely, the *subclavius, pectoralis minor,* and *serratus anticus.* Now the question is, can we account, by the action of these muscles, for all the movements of the shoulder upon the trunk of the body? In all the motions of the shoulder, whether upwards, downwards, backwards or forwards, the two bones move as if they were in one piece; there is no motion between them, the ligaments will not permit it; but if the scapula has a motion at all, independent of the collar bone, it is a motion upon the scapula and of the collar bone, on which it revolves, as it were, upon its axis. These things you will understand better as you understand the skeleton further, for no one will understand the skeleton from merely examining the dry bones; they must be seen connected with their natural ligaments before you can understand the real structure.

Serratus superior Posticus.—This muscle may be classed among the elevators of the ribs, and so far it is an inspiratory muscle; it may move the vertebral column, draw it back a little, and twist it in an inconvenient way.

Splenii.—There are two of these, the splenius *capitis* and splenius *colli.* What do they do? Extend the head and extend the vertebral column. They assist in turning the head round; and it is really curious how the head is turned round; it is not done by a single muscle, but by many muscles.

Complexus.—This muscle was formerly split into a great many; but thanks, say I, be to *Albinus,* for having it now considered as one. This is a muscle which, of course, supports the head, and draws it backwards towards the vertebral column; but by drawing the head towards the transverse process of the vertebral column, it will turn the face to the opposite side, concurring with the *mastoideus* and trapezius on the same side; and, in that same act, the *splenius* of the left side also concurring. Now you see there is a great necessity for muscles continually to support the weight of the head and neck: when we are quite upright, and have our faces looking straight forwards, the whole weight of the head is so poised on the top of the vertebral column, that it is perfectly balanced; it has no tendency to preponderate one way or the other. But it is poised as the false balance is; it has a greater lever on the one side than on the other, and the least preponderance of weight causes the head to drop forwards, and that, too, with no inconsiderable effect, as you see in people going to sleep. Well, then, there was a necessity for having the muscles continually to act for the support of the head, and this is the general use of the muscles which I have been describing.

Semi-Spinalis Colli, Rectus Capitis Posterior, and *Obliquus Capitis Superior.*—I will put all these three muscles together, for they must have the same use; they must support the weight of the head, and extend it.

Obliquus Inferior.—This is a muscle in bulk equal to all the rest; it is a very important muscle; it has a very efficient function to perform; it turns the head and the first vertebra round on the second vertebra.

Trachelo-Mastoideus is a muscle supporting the weight of the head, and extending it a little, turning it from one side to the other, but it is not a muscle of general importance.

Serratus Inferior Posticus, and *Serratus Superior Posticus.*—Both these are muscles of no importance. They act as inspiratory muscles, and will tend to steady the chest upon the vertebral column.

Longissimus Dorsi, and *Sacro Lumbalis.*—It is not worth

while bothering one's self about the actions of these muscles; I have heard disputes about them, but they are not worth attending to. If, however, it were necessary to have muscles to support the head, and to draw it back, how much more is it necessary to have them to support the vertebræ? These muscles do support the weight of the vertebræ. If a person was inclined a little forwards, and these muscles were a little paralysed, plump forward would he go with great weight upon his breast; but these are muscles to prevent that. And to give you an idea of the bulk of them, I would only ask you to look at the back of a skeleton, or an emaciated person, and what do you see? The vertebræ sticking up like a ridge. But look at the back of a fat person, and what do you see? Why, that which was a ridge, is here a furrow, with two fat lumps on each side. Well, then, these two muscles are continually acting for the support of the body; if they did not, the body would immediately fall forwards. And I would take this opportunity of saying, many of our motions are effected, not by muscular action, but by muscular relaxation. Suppose I wanted to make a bow, do you think I would use my abdominal muscles, and put these into full stretch? No; but I would relax them, and downwards would my nose drop. I remember, at a very early period of my life, having received a lesson of this kind: I caught cold, as the saying is; and, in my opinion, a very pretty little physiological book might be written on the subject of catching cold, for many people say that some colds are infectious: sneezing, for instance, by one man, will set another man about the same job. There are many others who will say, upon my soul and body, there is no infection at all about colds. But what is called catching cold, is where you are exposed to a stream of air; and if the wind blows upon some delicate persons, or if a little bit of a blast from the door comes on them, O! off they will go sneezing, and have a cold immediately. Well, I had caught a cold; I had inflammatory fever, acceleration of pulse, white tongue, and thirst; was bother-headed, could neither read nor do any thing else, and all my muscles ached most surprisingly. As I did not know what to do with myself, I went to the play; and when I got there, egad, my loins ached in a most horrible manner; I was glad to get to a place where I could lean to support myself, and then there was no pain. I held up my arm, but the pain in the deltoid was intolerable, and down it dropt immediately. Well,

you know, all this is followed by sneezing, then by coughing, and then by expectoration. Now, I say, can you account for all this, that just a blast of air blowing on one part of the skin should so disturb the whole constitution? Can you account for it? To me the account seems easy enough; which is, that one part of the skin being affected, the whole of the skin becomes affected; a sort of shivering and suppression of secretion takes place; and then we have what I may call the electricity of the body, the nervous system, and the muscular system, all become disturbed, and there is febrile excitement. Now a little of *James's powder,* and throwing yourself into a perspiration, will take off these feelings pretty quickly. But this relates only to one kind of cold. People come into the hospital with their loins full of pains, saying, O, my loins are very bad indeed; and if you let them do as they please, they will remain for many months: that is, if you allow them to get up and use their muscles, the pains will continue; but if you keep them in bed, they will soon entirely go away.

Now I have got over these muscles, as far as I have gone, as briefly as I could, because I do not like to be too minute with respect to any subject; and I am firmly persuaded that it is a piece of affectation to describe these muscles with accuracy, for they have not exactly the same attachments in different subjects; it is the general view of the muscles on the back, which it is important for you to learn; and having learned that, you may afterwards learn more if you think proper.

FUNCTIONS OF THE EYE, &C.

I have now to speak of the functions of the eye, as an organ of sight; but this is a very difficult subject. I am sure I do not pretend to have knowledge enough of optics to give you a correct account of it, and I have always questioned whether the class had knowledge enough to understand it, if it were given. Well, under these circumstances, I have made out as good a way of speaking of it as I can, by reminding the auditors of certain circumstances relative to *light,* which they may ascertain to be facts, and consider at their leisure.

First, then, it does appear, that light consists of very minute parts of matter following each other in straight lines, and moving with incredible velocity. These light particles of matter are subject to attraction and to reflection: like a ball

upon a billiard-table striking against an object which reflects it, it rebounds at the same angle. To ascertain that, you have only to consider this common observation : suppose there was a mirror here, and that three persons were to stand— two at the side of it, making the same angle with the mirror, and one in front of the mirror, what would be the consequence ? Why, that the two people who were at the side would see each other very distinctly, but they would not see the man who stood in the front ; and the man in the front would see himself, but he would not see the other two, for the rays of light going upon my person, (supposing I was one of those standing at the side of the mirror,) impinging on me, and being reflected, would rebound, and affect the person who stood at the same angle, whilst the rays of light going to the person in the middle, would rebound his own image, and would be evident to himself, but he would not see the image of those who stood at the side.

Now I say, then, they are susceptible of attraction—attraction by things which contain more condensed matter. Suppose all below a line to be water, and all above that line to be air, and suppose a ray of light to be passing from the water into the air, it will be attracted when it gets to the surface of the water by the denser medium, and its course will be broken. It will be *bent*, to use the optical language, it will be bent from the perpendicular light falling on the surface of the water ; the ray of light will appear broken, but we make no allowance for the refraction ; we always suppose the rays of light to come, as they usually do, in straight lines to our eyes.

Well, now, here is the experiment : I put a shilling at the bottom of a basin ; I stand so far distant, as that the edge of the basin conceals the shilling from my view ; I half fill the basin ; I let another person half fill the basin with water, and then I see the shilling. O, persons who had never tried the experiment would think the shilling must have been moved ; but the man who filled the water into the basin would know it had not been moved. You know, too, that a straight stick dipped into water appears crooked in this way. Well, the attraction by the difference of the media of water and air is well known ; it is as three to four ; and the difference between glass and air is as two to three. I am not clear that I tell you exactly, but it is computed, as you will find, that the eye may reflect in this way.

There is another cause of the alteration of the course of the

rays of light. If they impinge upon a convex body, those which touch the greatest convexity of it will suffer no alteration in their course, but those which fall on the sides of it will of course be attracted by the convexity of the body; they will be nearer to it in one point than in another; they will be bent inwards, as the phrase is, and all the rays will thus be brought to a point.

Now the degree of refraction from the convexity of the body is ascertained; they say, if you complete the circle, the point of convergence, the focus, will be at one semidiameter of the circle behind. This is the way they have calculated the refracting power of convex bodies. You know it is this power, for you have seen the effect of a burning glass in bringing light to a focus. Well, the rays of light come to the *cornea*, which reflects them partly on account of its convexity and partly on account of its concavity. A great deal of light falling on the cornea impinges on the angles of it, which will be reflected again, and that is the reason of the glistening of the cornea. It is only a certain quantity of light that will go through the cornea; from that a great deal of light will go to the *iris*, and will be reflected from it again, and this is the reason why we see the iris in some persons more than in others. It is only that cone of light that goes into the *pupil*, and then falls upon the *crystalline lens*. Well, then, the rays of light will be brought to a focus; but as to saying where the focus is, I suppose that that is more than any man can do; but the rays of light are brought to a focus behind the crystalline lens, and they are separated, and decussate and arrange themselves upon the *retina*, exactly in the same order in which they proceed from the different objects we see. Thus, if a ship were the object I saw, it would be represented on the retina, but inverted; the masts would be downwards and the bottom upwards.

Now the eye has all those powers, so that it is a most perfect and complete *camera obscura*, which any one may satisfy himself of, by the following experiment: Take off the back part of the *sclerotica* of an animal's eye, and part the vitreous humour with thin paper, then present the eye to certain luminous objects, three candles, for instance, in a room otherwise dark, and you will see the flames of those candles most beautifully depicted on the paper, with their tops turned downwards, glimmering and moving, just as the flames do. O,

it is a complete *camera obscura.* You have seen those cameræ obscura where the landscape, or whatever it is, is depicted upon paper, most accurately and vividly done. The eye has those powers of so refracting and bringing to a focus the rays of light, that they, afterwards separated, will arrange themselves on the retina, in precisely the very same order in which they emanated from the different surrounding bodies. But the retina *feels*, it does not *see.* In the common camera obscura we are obliged to use another glass, to correct the inverted position of the object; but as there is no correction for the inverted position of the object in the human eye, it has entered into the heads of some persons to suppose, that we really see things *topsy turvey* in the first instance, and correct this error by experience. Now that is a very strange opinion, and a very absurd one. We always believe the rays of light to come in a straight direction from the object to the retina, on which they impinge; and suppose the rays of light from the Gentlemen now in the gallery were to impinge on my retina, and produce an impression by which I knew they were there; and suppose I saw that hat, which now lies on the ground, on the top of my retina, the impression causing me to believe it was on the ground, would my eyes in all that deceive me? Do not the rays come in a straight direction? We know they do. My belief is, that the rays come in a straight direction from the object to the retina, and that is as it should be; the reason of it is, to give us our knowledge of the objects, which it unquestionably does. Well, there is one thing with regard to the eye which nobody understands: the eye is a correct camera obscura, when the object is placed at a certain distance from it; but if you were to approximate the paper to the candles, for instance, the flames would become indistinct at once. There is no adaptation in the dead eye to objects, but there is a great adaptation to them in the living eye, and how this adaptation is produced, I believe nobody understands.

I have talked of the elasticity of the cornea, and the convexity of the eye. Mr. Ramsden, who was a very clever man, an instrument maker in this town, did contrive an instrument by which he could try the sphericity of the cornea, so that when a person was looking at one object here, at another there, and at a third in another place of the same room, he could tell which object the person was looking at, by observing the

sphericity of the cornea; but that is not sufficient, and therefore some people have been induced to suppose, that the crystalline must be moved occasionally, or that it had power to alter its sphericity. All this is hypothesis.

Now when I speak of each organ of sense, I always take occasion to note those circumstances, with relation to sensation in general, which that organ is most calculated to display; and with regard to the light, we must see how subtile, how very subtile indeed are our senses, when capable of having vivid sensation produced from such wonderfully minute matter—by the atoms of light. So it is with regard to sound: what wonderfully subtile matter must that be, and yet what vivid sensation does it produce! There cannot be a question but that there is some subtile matter vibrating through the air which is the cause of sound. It has many affinities to light. Sounds cross each other without intervening, just as the rays of light do. But from the subtilty of our perceptions—and yet I do not dwell much upon that point—the matter produces actions on the nerves, produces some vibration in the nerves by which the action is propagated to the brain, and thus are we able to perceive why very vivid sensations often take place from the slightest impulse, and why, at other times, the most powerful impulse fails to produce a sensation. A man may be shot through the body and not feel it. It has been said, that a man has had his leg shot off, and that he never knew it till he attempted to take a step. Again, the slightest *odours*, or any thing of that kind, often produces a most powerful sensation on the *cerebellum*.

Now that there are those actions produced on the eye is perfectly clear. It was to this subject Dr. Darwin bent his attention, when he affirmed this piece of physiological information, which I believe now is commonly received, that sensation does not depend upon impression made upon the nerves but upon actions excited in them. There are some people who sleep with their eyes open; and a man may stand before another man in such a situation, with a lighted candle in his hand, so that the image of that person who has the light may be vividly depicted on the retina of the sleeping man; but does he see—is he sensible of it? No. Arouse the slumberer, awake him that sleepeth, bring but the natural excitement into his nerves and muscles, and he would exclaim, "God bless me, how came you here at this time o'night?"

But this is the point ; it is the consideration of *ocular spectra*. But what is meant by ocular spectra? O, you know ; you must have remarked, that if you look at the setting sun for some time, so as not to fatigue your eye, and turn about—turn to the East, you will see a ball of fire before you. O, you will see the spectrum of the setting sun. But say you have looked at the sun till you have tired the eye—completely tired the eye, and then turned to the East, what would you see? Not a globe of fire, but a round black ball. You will still see the *spectrum* as far as relates to *form*, but not as far as relates to *colour* ; and it is very curious that you may look at scarlet till you tire the eye, and then that it will appear green ; and that you may look at green till it appears scarlet. It is the reverse of the colours that appears. You look at the window, and if the eye is not fatigued, and you take it away, you will see the spectra of it ; but if it is fatigued, and you continue it, the light part will appear dark, and the frame-work will appear bright and light. These things you must have observed ; perhaps you may not have reflected on them, but it is the reflection on them that leads Dr. DARWIN to conclude that the retina perseveres in those actions which have been reflected on it, and therefore we observe the object which is no more before the eye ; or the eye being fatigued by looking at it, you have another object, you have the spectrum as far as relates to form, but not so far as relates to appearance.

The eye of course is one of the organs which adapts the manifestation of the sense to the situation in which we are placed. A man may live in a dark place, and objects there may be very distinctly visible to him ; whereas, a man coming out of a very strong light can see nothing in the same place. This adaptation we know has been *expressed*, but I am sure I cannot say it has been *explained*. The vividity accumulates in proportion as the exciting causes accumulate, and diminish in proportion as the exciting causes diminish, but it has certainly not been explained.

Mr. ABERNETHY now described the *lachrymal* parts of the eye.

As to the *eyebrow*, I think you cannot fail to see that it is in the human species an organ of expression. I know that any painter would concur with me in this, for by a little arrangement in the eyebrow he can make a great alteration of expression in the face, and he cannot produce that alteration by the variation of any other single feature. No animals have this.

And now you may think as you please upon this, but I have told you what I think of it, and that is all I can do.

Secretion and course of the Tears.—It seems all very beautiful that there should be this contrivance to besmear the tears equally over the whole front of the eye, and that there should be, too, the groove that we find here to convey away the surplus tears from the eye into the lachrymal bag. But the question is, What are the tears? Now any body making such an inquiry would really surprise a person who had not reflected on the subject. What are the tears? Does not every body know what the tears are? One would think that a person who instituted such an inquiry, had never seen a *blubbering boy* with the salt water running down his cheeks. Aye, but are those tears? Those are tears to be sure, such as are shed from irritation or from sorrow, but they are not the *common* tears. They inflame the eye; they excoriate the very cheek down which they run. What are those salt water tears? O, they are the product of the lachrymal gland, which is lodged in a slight fossa in the orbitary process of the *os frontis*. It is the property of these glands—the salivary glands, to secrete occasionally, and not continually, and to secrete profusely at times. This is the source of the salt water which is shed from grief, or when any thing irritates the surface of the eye; but it is a kind of salt water not calculated for lubricating the surface of the eye, that you may be assured of. What are the common tears? Unquestionably a very lubricous fluid to facilitate the motion of the eyelids upon the front of the eye-ball—a mucilaginous liquor—a thin mucilage, secreted from the whole surface of the concavity. That it is mucilage is manifest; for where it is abundant in quantity, and perhaps having a greater abundance than common, in consequence of inflammation, does it not *gum* the eyelids together? Have you not little follicles of gum which you may dissolve in warm water? I say it is a mucilaginous secretion, excellently calculated for preserving the front of the eye, and for preserving it moist, so that it may be transparent.

But, now, if they are mucilaginous, they might gum the eyelids together; they do; and here it seems that something is wanted to prevent this gumming and adhesion, under ordinary circumstances. Now, for that purpose, there is a row of glands and ducts situated beneath the lining of the eyelid upon the *tarsus*; they are called *ciliary glands* and ducts; they go

also by the name of the anatomist who described them—*Meibomianæ*. You may press them, and you will force out of the orifices fine threads of oil—suety sort of oil, and this is to prevent the gumming. When those ducts do not perform their functions well—when there is that agglutination, medical men have been in the habit of painting this part with some ointment to stimulate the orifices of those ducts, to produce the natural secretion; and, undoubtedly, it is good to anoint the edges of the tarsus, because it does prevent the agglutination I have been speaking of.

Now, then, there is another good in the *tarsi*; the stiffening of the tarsus gives an opportunity for hair that grows from the very edge of the palpebræ, to arrange itself in a right direction —the eyelash. Were the tarsus not stiffened, the hairs must take their regular direction, and they project considerably beyond the apertures of the *eyelids*. Now, what is the use of these? O, they protect the eye against the approach of any foreign body; the eye is shut against any thing approaching it with a celerity that is proverbial—*the twinkling of an eye*. And the excellence of this contrivance is very well manifested to you, by a common occurrence which you must be all acquainted with: a little fly, in flitting through the air, touches the eyelid, and the eyelid is closed upon it; the eyelid, in this respect, becomes an excellent *fly-trap*, which crushes the little insect to death in a moment.

But there is one thing more: when the eyelid is closed over the eye, when the superfluous tears have got into the groove made by the two tarsi, when the orbicularis presses the eye, and gives a torrent of tears towards the *puncta-lachrymalia*, there is a question, why the tears do not come out at the *internal canthus*, why do they go no further than those *puncta* which are to convey them into the nose? There is a projection here to prevent that, which is called *caruncula lachrymalis*. It is manifest in every eye. It is a little bit of fat rising up, to fill up an apparent vacancy. It was thought to be an organ of secretion; but we have now reason to believe that it is merely mechanical—merely placed there to prevent the tears from going beyond the puncta lachrymalia. In some animals it is covered by *cuticle*; I allude to the *horse*, and therefore it cannot be a secretory organ.

Another question which has arisen has been, whether the puncta lachrymalia transmit the tears to that which leads to

the lachrymal bag, in consequence of the fluid being impelled by force through them, or whether they have the power of absorbing the tears? Now that the *orbicularis palpebrarum* muscle does jointly, with other powers, impel the tears on, is what we have reason to believe. If a person has a redundancy of tears, what does he do but shut his eyes, and gently press with that muscle, to get rid of them? But yet there is reason to believe, that there is that absorbing power. Dr. FULLERTON was convinced they had this power; and I think it reasonable to believe that they have the power of absorption.

And now I have done. I have told you of all the circumstances—of all the artifices which Nature has employed for lubricating the front of the eye-ball, and keeping it continually transparent. In no other part of the body have you such a series of contrivances, the uses of which you so well understand. Every thing seems required, and every thing required is met with, and every thing met with seems to be admirably adapted to its function; so that I cannot but think that the lachrymal parts of the eye may serve as an instance of the effects of a designing cause, operating in the formation of Nature's works. However, I have done for to-day.

LECTURE XXXIV.

ON THE STOMACH.

IT may be questioned from the human stomach, whether man's stomach is *carnivorous* or *herbaceous*; for my own part, I know nothing about it, except that I think a man's stomach is very like a lion's, and that a lion's stomach is certainly carnivorous. But that a man was meant to eat herbs is to be collected from his teeth, from the articulation of his jaw, and from his masseter muscles. That he is capable of living also on animal food is perfectly clear; and it is evident, taking both the alimentary canal and the stomach together, that he was meant to feed on both, for the alimentary canal is between herbaceous and carnivorous.

There was a time when persons thought the *wrinkles* in the internal coat of the stomach, rubbing against each other, produced hunger; thence arose the vulgar phrase—"Come, take the wrinkles out of your stomach," by satisfying the appetite; but all that is mere nonsense, and I may as well speak upon that now.

Hunger and *Thirst* are mere *sensations*, sensations of the stomach, and they seem really to be incompatible with one another; for a thirsty animal is not hungry, nor is a hungry animal thirsty. Hunger does not produce thirst, and an animal does not drink till it has digested its food. People will say otherwise; they will say, I have seen a cow drink the moment she began to eat. Well, so they may, for that is just the time when she should drink; the stomach is then empty, and the water that she then drinks does not remain above what the cow may have just at that instant ate, but it passes down into the stomach and fills the paunch, enters into and fills up the *cooking cavities*, as I may call them. People feel hungry when their stomach is full; this you will frequently find to be the case. A healthy young man, after eating a pound of beef steaks, and after having washed them down with a good quantity of strong drink, will be as hungry as if he had never seen the beef steaks, and ready to swallow a barrel of oysters. O, there are people who eat continually, and are never satisfied but when they are eating. There was a woman in this hospital who was eternally eating; they gave her food enough, you would have thought, to have disgusted any body, but she crammed it all down; she never ceased but when her jaws were fatigued. She found out, that when she put her feet into cold water she ceased being hungry; and, therefore, when she had a mind to rest her jaws, she got a bucket of cold water, plumped her feet and legs into it, and there she sat till she thought she would up and at it again. O, all this is a morbid sensation; extreme thirst is a morbid sensation, and you must cure the morbid sensation of the stomach if you want to cure thirst; but the way people generally take to cure it keeps up the thirst rather than allays it.

Now, in looking at these follicles in the internal coat, some people say they are for secreting *mucus*; others say, they are for secreting the *gastric fluid*. Now every person knows that the stomach does secrete mucus and a peculiar liquor, but I do not see the sources of this; we see distinct sources in

the intestines, but we do not in the stomach; at least I cannot.

With respect to the *physiology* of the stomach, to the best of my knowledge and belief, Mr. HUNTER was the first man who broached those opinions which are generally accredited, or have not till very lately been disputed, which is, that digestion depends upon the sufficient quantity of the liquid which the stomach secretes—of the gastric fluid, and that this liquor has the properties of rendering any nutritive matter solid, so that it should not pass through the *pylorus* until it had undergone this peculiar solution by the gastric fluid. Well, if the white of an egg is given to a hungry dog, in a few minutes it will be as hard as if it had been boiled for hours. The juice of the stomach instantly curdles milk. Take the gastric fluid out of the stomach of a calf just born, and it has most curious properties; the little of that fluid that is in the stomach they dry, then dissolve it, and then curdle gallons of milk with it, as you all know. Then, as the fluid of the stomach has the power of dissolving that which is nutritive, the question is, why don't it dissolve itself, it is flesh? JOHN HUNTER would never boggle at an answer for that, and he says, that the life of the stomach preserves it. There is no chemical agent will act on it. Worms live in the stomach, and yet there is no doubt but that a meal of dead worms would form a very nutritive repast to a dog. But does the fluid ever dissolve the stomach? Yes, it does. The proof of this is, that if a man eats a large meal, and is suddenly afterwards killed by a blow on the head, as has often occurred, you will see that the gastric fluid is acting, not on the food, but on the stomach, that it makes a hole in the stomach, and that the food gets through that hole into the belly. Various stomachs have been examined having these appearances. But Sir ASTLEY COOPER told me of a case where a person was met with after death under these circumstances: the body was laid in what they would call *a very ungain attitude;* it laid with the thorax lower than the abdomen, and upon opening the body, the stomach was found to be dissolved at its lower arch; it is usually at the larger arch where it is dissolved; in that case the diaphragm was dissolved, and the stomach was in the lower part of the thorax.

Some people say there is no gastric juice; why? Because it is not secreted but at certain times; it is only secreted when it

is wanted. There are many who think, even now, that it is the vital powers of the stomach, and not the liquid that causes the digestion. Well, they may entertain that notion if they please, but all I am bound to do is, to tell you what appears to me to be the most reasonable physiology.

There are animals who feed on what appears to be most innutritive to others—the woody substance of things. But what do they do? They first pour upon that substance, a liquor, and then they eat it: there is a liquor evidently runs from them, and dissolves that which is afterwards to become their food.

Now, I say, analogy would lead us to suppose—and I think I have told you enough of facts, to warrant us to a pretty considerable extent in believing—that the stomach does prepare a liquor of this kind. Mr. HUNTER thought so; and he was employed to open the body of a patient of Sir JOHN PRINGLE'S, in whom the stomach was dissolved. To Sir JOHN PRINGLE it was new; Mr. HUNTER said, O, I have seen it frequently, this is not disease—it is the effect of the gastric fluid. Sir JOHN PRINGLE, who was president of the Royal Society, urged him to write a paper upon it, and he did. Now, two or three years afterwards, a Dr. STEVENS, who was preparing his *inaugural thesis* at Edinburgh, met with a man who was a *stone eater*—a man who would swallow stones and let you hear them rattle in his stomach; and the doctor thought this would be a good time for making experiments; therefore, he got him some food consisting of balls, ivory balls, metallic balls perforated with holes, which he gave him filled with animal substance; for it was said that irritation might be, and that trituration must be, the cause of digestion; but trituration was not the cause of it, as appeared from the balls after they were voided. Had putrefaction destroyed the whole? O, the time was too short for that. Well, he went on, and gave the balls, filled with matter, to carnivorous animals, but they also voided them undissolved. It was eight years afterwards, when this was taken up by another person, but yet no one speaks of Mr. HUNTER but myself. I believe him to be the author of these opinions; certainly that has been doubted, it has been contended against, but I believe not on sufficient grounds. I may just mention further, that if you give putrid meat to a hungry dog, it will sweeten in the dog's stomach, and that the gastric fluid of a dog's stomach has been applied to sores.

Now this is all I have to say about digestion; but here I come to a very curious point, indeed; which is, that all Mr. HUNTER's notions of digestion has been, I may say, proved by experiments on the stomach: and which is, that nervous energy is required for secretion; that secretion depends upon a particular action, and that the nerves being incapable of exciting and governing that action of secretion, it will not take place. Dr. HAIGHTON divides the eighth pair of nerves in a dog, and curiously enough the dog takes its food at regular periods when it is given to him, but he shows manifest signs of sickness and disquietude; something is voided from his bowels, as aforetime, but the dog wastes away, and becomes nearly a skeleton. In the course of a certain number of weeks, the nerves become united, and the dog digests his food, looks well, and again recovers his flesh. Dr. HARWOOD, of Cambridge, feeds two dogs of the same age, brothers or sisters, in an equal state of health, with flesh meat; they are pointers, and after he has done so, he decoys one of them into the fields and hunts it—induces it to hunt, to exercise it: after hunting it for four hours, he brings it home and kills both the dogs. The one that was hunted has all the flesh in his stomach, softened a little, but not digested; the other, which had been suffered to do what nature dictated—that is, to lie down by the fire and go to sleep, has all his food digested, and it is found in the high road into the blood vessels; the lacteals are all full of the chyle which they have absorbed.

Now these things deserve the particular attention of medical men; because, what is the chief object of medicine, in a great number of cases, but to give strength to patients? For my own part, I can see no permanent source of strength but in digestion. You may give *bark;* aye, and if the stomach will digest it, it will give strength, but I know not how it will give it otherwise. We ought to caution patients, not to take too much exercise after eating, but to keep themselves quiet, and not to allow their minds to be disturbed, for you know the eighth pair of nerves come from the brain to the stomach, and if the mind be agitated and disturbed, will the stomach digest food, or will it even receive it? No; O, there is a great sympathy between the mind and the stomach: if a man has received some intelligence which really distressed or annoyed him—thoroughly distressed him; if a merchant heard of the loss of a great adventure at sea; if a surgeon heard that a

patient of his had died in whose fate he had taken great interest, who he thought would have recovered, and by whose recovery he would have had credit secured to himself; or, if a lover heard that his mistress had cheated him just at the time he was sitting down to dinner, would he eat his dinner do you think? O, no; or if he heard of the tidings just after he had dined, would what he had taken digest? O, no. Well, to secrete, there must be vital actions regulated by the nervous system; and, in every part of the physical body you will see that fact proved. Now I go on: the food is digested, but what does it become? Not chyle in the stomach, but a kind of pulpy adhesive sort of matter—sticky sort of matter, of no very pleasant odour. People vomit after they digest their food, when they have had a blow on the head; and what are we to call that? O, it is generally called chyme. I may inform you, that there have been opinions entertained, that the right end of the stomach is more calculated for food, and the left for the fluid. But these are things, I believe, concerning which we have no absolute knowledge; and, therefore, we are ignorant of them.

ON THE INTESTINES, OR ALIMENTARY CANAL.

Small intestines.—With respect to the physiology of the small intestines, the digested aliment passes forward to the *jejunum* in a continued track of small intestines, and at that time the bile and pancreatic liquor have been observed to flow copiously into the alimentary canal; and indeed it is natural to suppose that the change the food undergoes in the small intestines must be owing to a commixture of those liquors. And so it may be, but we have no proof of it; and what I have got to say about bile or pancreatic liquor cannot come in now; it must come in with the physiology relative to the liver and pancreas. Relative to this, Mr. HUNTER observed that the bile did not seem to commix with the digested aliment; he said it seemed to flow over it, and that the bile and excrementitious part went into the lower part of the bowels, and moved on. Now Sir ASTLEY COOPER, in those lectures he delivered before the College of Surgeons, and for which he made numerous experiments, gave me additional information as to this, which, if it be correct, I hold to be very valuable: He says, that the digested aliment coming on into the intes-

tines, coagulates to the very surface of the tube, adhering to it most firmly and most tenaciously. This fact he demonstrated, for he produced a piece of the intestines with the chyme, as I may call it, adhering to it, so that when the intestines were slit up, you could not see the villous coat; with a pair of forceps he pulled off a thick slip of chyme and sheath, and beneath it the villous coats appeared, and were injected as red as possible. He said the digested aliment acquired a kind of coagulated or adhesive quality, by which it firmly adhered to the internal coat of the small intestines. Now you know the power which might be called spontaneous coagulation, seems to be a very peculiar and a very universal property in the nutritive fluids of animals. It is extremely unlike what we see happen in fluids, not of a vital nature; nor is this like a chemical change. Mr. HUNTER believed that this was the result of vital properties; that the blood—the coagulation of the blood—was the result of vital properties; for blood sometimes coagulates and sometimes does not, apparently under the same circumstances. Then Sir ASTLEY COOPER was led to believe that the digested aliment had that peculiar property of self coagulation, as it were, which would enable it to cling to the surface of the intestines, as coagulated blood clings to the side of an artery in an aneurism. It seemed to him to show that the stomach had not only dissolved the food into something that had a very peculiar nature, but had conferred on it a power of self-coagulation which enabled it closely to adhere to the surface with which it was in contact.

Now I say, if this is true, and I tell you there was a demonstration of the truth of it to a certain extent; it seemed to throw new light upon the subject to my mind; it showed how the juices of the intestines were not wasted, for what was secreted from the surface of the intestines would be immediately applied to this chyme, and act upon it, and convert it into chyle. Whatever properties bile and pancreatic liquor may be supposed to have, analogy, which is our guide and rule in all circumstances as to the formation of opinion, would lead us to suppose that the change the food undergoes very much depends upon the juices that commix with it. The surface of the intestines is excessively vascular, and it has a villous origin. We must believe it is an organ of great secretion; we have evident proof of its being so in morbid affections; the quantity of morbid secretion poured out is very great indeed;

but of the nature of the healthy secretion of *succus intestinalis* we are as ignorant, as we are of that of *succus gastricus*. And what Sir ASTLEY COOPER says is satisfactory to the mind, inasmuch as it shows how those juices are not wasted; why their action is limited to that which is converted into something ulterior for the benefit of the body.

Now what we know is this: that as soon as this stuff is in the lacteals, it resembles the blood—it is chyle; open a lacteal, catch the contents, and what do you find? You find a fluid, which, like the blood, separates into serum and crassamentum; there is concretion, there is a crassamentum formed, analogous in every respect to the crassamentum of blood. And what is more curious still, this liquid abounds with globulous particles, like blood; they are not red, to be sure; in that only does it differ from blood. So that you see how speedily the change of digested matter takes place, which is designed to be effected. What is the object of digestion? The conversion of the food into something resembling the substance of the body, and for the support of the body.

Now that is all I have to say concerning chylification; and I go on. The residue of the food and the bile, which is certainly incorporated with it, is urged on by the *peristaltic* motion of the intestines, and eventually propelled from the *ilium* into the *cæcum*, the *caput coli* or *cæcum*. Well, now I have to speak of the structure of the large intestines. Mr. ABERNETHY described the structure, and then proceeded:

With respect to the *valve* to be found here, it prevents the fœtid air from getting upwards; but does it never get upwards? O yes, often, often, often, when the valve is in a *morbid* state. Where the valve does not perform any function attributed to life, how often do we hear it reported that people have vomited up clysters; but is it the clyster, or is it the contents of the stomach, scented only by the clyster, that is vomited? I believe the latter. But do not understand me to say that people do not vomit up clysters, because I have seen cases in which they have, and I have known the people to live long and happily afterwards. I remember a lady, who had a state of bowels which caused every thing to go the wrong way; no medicine could bring back a proper state of the bowels, no stools could be procured; she could take nothing, she had had nothing for a week; a copious clyster was ordered, of water-gruel and oil; she vomited it; the oil was seen floating

on the top of what she did vomit; how could the oil have come there, had it not been for the clyster? And yet, I say, she recovered, and lived long and happily afterward.

In cases of *hernia* too, there is a stinking matter brought upwards, and you smell the most unpleasant breath of the patient. O, it is a very horrible sign; the chances are, that the person will never live, I am sure.

Functions.—Now as to the functions; undoubtedly there is a great change wrought in the alimentary matter in the large intestines, a great and a sudden change. You may slit open the alimentary canal, as far down as the ileum, and you may find stuff in it, but it has not at all the odour of *fæces*; but cut a single line further, cut but a line of the cœcum, and the nose is assaulted by a tremendous smell. Formerly, people thought the putrid smell of the fæces was a chemical change on the food; but it so suddenly takes place, that this opinion is quite untenable. And to what can we impute this change? Why analogy leads us to suppose that it is a change which takes place of the food, by the quality of that which it secreted, the succus intestinalis of the large intestines. And what is the nature of the change? The fæces have a bad smell, but are they putrid? O no, not at all. In health there is no putrefaction in the large intestines; but the contents of the large intestines suddenly go into putrefaction, and the product of it is *ammonia*, as you may believe, if you go into a common necessary, for there the smell of ammonia is perceptible; but is there ammonia in the fæces before they are expelled? So much the contrary, that *acids* are found in the fœcal matter, so that if there was ammonia in them, that could not also be found in them. There is no chemical decomposition in the large intestines when they are in health. Chemical decompositions take place in the stomach; food there becomes rancid. When the succus gastricus does not produce its proper effect, several chemical changes take place in the food. *Hydrogen* is found there; the air that is discharged is inflammable, as many school boys know. Now all this is owing to the state of the health, and in health there is no chemical decompositions, even in the large intestines, so that whatever notions we may form as to changes in the food, wrought in the large intestines, we may attribute this as one, that the change which is produced in it is protribitive of the food going into those pernicious chemical changes which it would otherwise undergo. Pernicious che-

mical changes which it would otherwise undergo, I say, because, if you think for a moment, you must admit, that if the alimentary matter was allowed to go on into noxious putrefaction in the large intestines, it would produce most horrible fever. Do we not know, that when putrid matter is absorbed and goes into the vessels, that horrible fever is the result? I mean *putrefaction*. You do not use the word *putrid* in a vague manner; we use it to a thing having a bad smell. You may apply it to the fæces. Fæces have a peculiar smell, but they have not the smell of putrefaction; they are fœtid, we may say, but not putrid.

Well, this may be disputed; but there is reason to believe that there is a change wrought there which still produces something nutritive. For in *herbaceous* animals, the *cæcum* is exceedingly capacious, and there the residue of the food is not likely to go into putrefaction. More particularly in the *horse*, which does not *cook* its food, as I may call it, which does not ruminate, the cœcum is large; and, on a former occasion, I told you it was a sort of reservoir for the liquor the horse drank. Well, then, we have reason to suppose, that the food does undergo a second sort of digestion here, of converting that into nutrition which is capable of it. And here again, Sir Astley Cooper affirmed, that whatever was nutritive clung to the surface of the intestines; acquired that property of adhesiveness, of coagulation, which enabled it to cling to the surface of the intestines, while that which was not fit for nutrition was gradually propelled forward by the peristaltic motion of the intestines, and ultimately expelled. O, there is a great change takes place. I have always said in these lectures that we talk of *chylification*, and why should we not talk of *fæcification?* for there is a wonderful change produced in the intestines; and though we do not know the extent to which that change is produced, yet it is a very important change. I have sometimes said, that a very old man, a queer character, a humourist, who lived in the early part of my life, whom I knew, a Dr. Maunsey, saw these things very clearly, and who left his body to be examined and dissected after death. He said, that all the surgeons in the kingdom, let them do what they would, they could never make a *t—d*

Morbid Anatomy.—Now I have done with the functions of the alimentary organs, and next I have to speak of the morbid anatomy: and I always consider this part proper to be begun

with requesting gentlemen to make up their minds as to what they are to do when people have taken poison. You may read books, and you may use the stomach pumps, on which I have but little to say just now; but I know it is most important that every man should have his mind prepared for what he is to do on such an occasion; for then he can have no time for consideration. You may make them drink, you may make them vomit, you may thrust your finger down their throats, and you may extract with the pump, but the object is to neutralize as much as possible the noxious quality of that which has been swallowed, and to get them to throw it up. There are even *salts* which admit of dissolution; and the object is to get that out of the stomach, which has been taken, by inducing vomiting, or by the use of the pump. Now I have this opinion upon morbid anatomy, that it does not carry with it the interest to my mind that it does to many others, because the appearances are so excessively various; and yet, as far as general classification goes, O, we see the same sort of diseased actions fill up the same sort of diseased structures in every part of the body.

The only way in which I could explain what I know relative to the morbid anatomy of the alimentary organs, in an useful manner, is to speak of them in their morbid state; first in their *peritoneal coat;* next in the *internal coat;* and lastly, in their *intermediate coat*, which I may call the walls of the vessels.

Peritoneal parts.—Well, with regard to the peritoneal parts, what have I to say? What have I to say, why, it was a thing generally believed, when I was young, that when a person had *peritonitis*, it would either kill him or he would get the better of it!!! People were not then aware of the *chronic peritonitis*, which would go on for a very long time indeed, till it changed the whole and entire form of the abdomen. I know I have been often called upon in cases of this kind where, if I had been brought into a room and desired to tell what I saw, I should never have known that it was the cavity of the abdomen. The *viscera* are all united together; sometimes there is no cavity at all; sometimes in trying to separate the viscera I have torn the bowels asunder without separating them. The peritoneum every where rough and thick; parts adhering, and the vacancies between those parts filled with fluid and tubercles; the peritoneum studded with tubercles.—Here are

specimens of that kind. I therefore was exceedingly pleased when I saw published an account of the nature of this persevering disease, which gradually destroyed life.

Internal Coat.—Well, now, as to the internal coat; why it is liable to ulceration, and that is a very odd thing. How ulcers form there is curious. Here are specimens of ulcers in the stomach, and the stomach is liable to have many ulcers in it, but those ulcers will of course bleed occasionally, and yet they may eventually heal. Now I say this is curious, that an ulcer should penetrate, beginning in the internal coat, through all the coats of the stomach, and yet leave no aperture out of which the contents should escape. I say this is curious; it happens both in the stomach and in the intestines, and I have seen it frequently. Curious at first sight only, not curious upon reflection, because, as the ulcer proceeds to the external coats, it produces inflammation in the peritoneal coat, so that this peritoneal coat adheres to the contiguous bowels. The stomach may adhere to the walls of the abdomen, and the aperture is shut up by this adhesion. Dr. Bailey has given, in his plates of anatomy, instances of the stomach in this state and I had instances of them too, many such instances, among my preparations, but they go—they vanish! With respect to ulcerations, of all the coats most likely to be attacked, Dr. Bailey says, it generally begins in the mucous structures, all through the small intestines, as in this instance, in which the preparation was taken from a man who died of *diabetes;* and when I have to speak of diabetes, I shall declare it as my opinion, that the chief cause of that state of urine, is an imperfection in the digestive organs; that the vegetable part of the food is not digested, and that it is imbibed in the blood undigested.

Well, now, in the next place, as to ulcerations in the small intestines, I know of nothing more that is to be said about them. It is the same with respect to the large intestines; there are ulcerations also to be found in them.

Intervening parts.—Now then, in the intervening parts of the coat, the walls of the vessels, O it is very curious to observe what is set up there; that the walls become excessively thick, the internal coat, and the peritoneal coat not appearing to be diseased. Now here is a stomach, which you will hardly recognise to be a stomach, but the *form* shows that it is one; it is great at one end and becomes less at the other. This was

given to me by a gentleman who was the surgeon of the patient; it was the stomach of a poor watchman, who went round calling the hour as long as he could; when he could do it no longer he went into a poor-house; he was always complaining, and the surgeon who attended him thought there must be something very curious in his internal composition, and upon examining him this was found to be the state of the stomach. So with respect to the intestines lower down, they present the same appearances; parts of them are excessively thickened, whilst other coats of the same portion remain undiseased.

Well, the *scirrhous* disease begins in this manner; it is found intermediate between the two coats, but it is of that nature that eventually ulcerates. Dr. Bailey has given the best description of incipient scirrhous, in the case of *carcinoma*, that can be given; and this disease is found in the *pylorus*; it is found in thick, white bags.

Stricture.—Now here are specimens of stricture in the *rectum* and other parts, not of the cancerous nature. Though some *strictures* lead to carcinoma, yet all strictures are not of a cancerous nature, and it is curious to know how frequently strictures occur in the alimentary canal. In all mucous canals, the *urethra*, the *œsophagus*, and in all the *alimentary canals*, stricture occurs; and in these cases there is something wrong in the muscular power. A disposition to contraction becomes established, thickening goes on, and a permanent sort of diminution of the tube is induced. Meeting with these strictures all through the alimentary canal, the consequence in some cases is, an accumulation of the fæces above the part contracted; this is exceedingly complicated. I can tell you of a striking case of this kind. There was a young woman who came out of the country that I once saw, and having a grand disorder of her stomach, she was of course advised to consult *Doctor* ABERNETHY, and she brought a statement of her complaint with her. I told her I was not a physician; that I could not understand her case by the statement of others, but that I would examine her if she pleased; at the same time I advised her to get better advice. She allowed me to examine her; I traced the boundaries of a large swelling in her abdomen. I said to her, you had better stoop and lean forward with your elbows on a table, for by doing that, if there is any swelling in the belly it is sure to descend. I went on examin-

ing her, working in my own mind to make out what her complaint was. In the course of examining her I put my hand on the surface of the swelling on the belly, and upon gently pressing it I heard a rumbling and jolting sort of noise; I went on, and pressed it altogether away. It was some contraction of the alimentary canal, and an accumulation of the alimentary matter above it. I requested her to see Dr. BAILLIE, and I was much gratified to find that he gave the same opinion, both as to the nature and treatment of what she had complained of; which was, to take care to excite a purgative action of the bowels above, to cause them to carry down, through the contracted part, that which ought to pass. She soon got rid of any tumour in her belly, and I fancy she got well; at least she did so far as I ever heard any thing of her.

Contractions and *malformations* might be spoken of. It should be remembered that there are malformations occasionally occurring in this organ; a curious sort of process going on in the alimentary canal. This should be remembered; for supposing one of those things to occur in the case of a *hernia*, it might slough off, and do a person no harm, I am sure.

Worms.—You find worms in the alimentary canal. There are worms of different kinds to be met with there; one long worm, pointed at either end, which they used to call *lumbricoides*. Then you have the *tape* worm; and there is generally but one of this kind to be met with, so that the French have called it *ver solitaire*. To be sure it breaks, and a great deal of it may be brought away, but it grows again, and no good is done unless the head of the worm is loosened, and the whole of the worm brought away; then the patient is cured. There are other intestinal worms met with occasionally, but those I have mentioned are the most frequently found.

Involutions. Next of involutions,—that is a curious thing; but Mr. HUNTER published a paper upon this subject, showing how those involutions were likely to take place: *intus-susception* it is called. In these cases the bowels act capriciously and spasmodically. Sometimes this happens to an enormous degree; it has been to that degree that the valve of the ileum has been known to present at the anus. The French memoirs abound with such cases; and when I used to read them, before I met with similar cases, I used to say,—Odd hang it! these Frenchmen are always having their bowels come away; but such cases are very frequent here. There was a woman in

this hospital, in whom the *intus-susception* portion perished and came away, and yet she had natural stools afterwards; but she died.

Here, however, I talk of things for which there seems not to be any direct remedy; but still it is right to call your attention to them, and, having done that, I have finished for to-day.

LECTURE XXXV.

ON THE LIVER, GALL BLADDER, AND GALL STONES.

It is probable that there is a great deal of absorption from fluids that are secreted, and that secreted fluid does not immediately acquire its due properties; that it is undergoing chemical changes, and becoming more and more perfect as it passes through the excretory ducts of the glands. This opinion I am led to entertain, from what takes place in the liver.

Functions.—As to the function of the liver, it is a strange thing—it is what does not occur in any other part of the body—that a vein should secrete. And we cannot wonder that the older physiologists thought that there must be some peculiarity of the blood in the *vena portæ*, which qualified it for the secretion of bile: they supposed it came back from the bowels laden with oil and saline matter, and so on, and therefore was qualified for the secretion of bile. But the blood of the vena portæ cannot be distinguished from any other blood I have seen. Then it was thought that the motion, that the current, was necessary for the secretion of bile,—that a regular current should be induced. And then some thought the vena portæ acquired the property of an artery, and so on.

Now all these opinions are completely quashed by a fact, which is, that it has been found that the vena portæ, in some subjects, has not gone to the liver. I happened to meet with such a case as this; it was new to me then, and I believe to the public here. The history of it was this: There was a little subject brought into the dissecting room, thought large enough to make a vascular preparation of; I suppose it might have

been a twelvemonth old; it was a fat and muscular subject. As nobody chose to take it to himself, it was thrown about; and I said to the person who attended to the dissecting room, at that time, you might as well throw a little injection into it, and just see how it will turn out; but when he opened the body with a view to inject it, this little despised thing turned out to be of the greatest value; for, upon opening the thorax, he found the heart, instead of pointing to the left side, pointing to the right. Now this happened just after Dr. BAILLIE published his paper with respect to the *transposition* of viscera; and Dr. BAILLIE suggested, that if there was a transposition of blood-vessels, there probably would be a transposition of viscera in general; but here was a case where the viscera were in their ordinary situation, except that the liver lay more in the middle than it usually does, and that the heart pointed to the right side: well, the person I refer to, threw in an injection, and proceeded to dissect the subject, but not at all aware that a still greater curiosity would be found in it. He took away the bowels, as we do in dissecting, to make a course of vessels along the vertebral column; and lo, and behold! he found the vena portæ injected, the superior *mesenteric vein* filled with injection from the venous system! Why, this was most strange; and, tracing it, he found that it terminated in the inferior vena cava, just in a line with the *renal veins*, and that it never went to the liver at all. Then the question was, what vein supplied the liver? None; we could find none. What artery? None, but the *hepatic artery*, and that was one third larger than common; it went to the liver. The next question was, was there any bile? The gall bladder was opened, and a little bile escaped out of it: the gall bladder was not large, it was a third less than we usually expect to find it, and the little bile that was in it was very healthy, and had all the qualities of bile to be found in the gall bladders of children. The intestines were cut open, and their contents were dyed with bile. If no bile had been prepared, the gall bladder would never have been as it was. I have seen many instances where there was no gall, and there the bag was almost in a solid state; the size of it, however, showed it had undergone distention from bile. But this is not singular, for the same thing was met with by the late Mr. WILSON; but I speak of more advanced periods: Well, if these old opinions respecting the secretion of the bile are quashed, as I have said, what

opinions are we to adopt? Instead of saying it requires peculiar blood, and a certain regulation of blood for the secretion of bile, may we not now say, that in all probability it requires arterial blood for the preparation of every other secreted liquor, but that bile was a secretion that could be prepared from venous blood? And if this were true, you will see at once that an economical sort of purpose is answered by preparing the bile from venous blood, for, by this plan, all the blood returned from the bowels, which otherwise would go unemployed and useless back to the heart, is made to pass through the liver, and to prepare the bile in its way to the heart? Well, but if this were so, could not other secreted fluids be prepared from veins? Might not nature have ordered any of the venous blood from the extremities to pass through glandular parts and prepare the fluid? Would not an economical purpose have been answered by this? Well, we are decidedly ignorant, there is no doubt about that, of a number of things; but we must think—we cannot help it; people say we should not think; but, I say, how can we help it? It is natural for us to think. Then the question is, since we must think, whether we had not better endeavour to form the most correct opinions we can of the things which are before us?

Well, then, I hold it to be a very, very great curiosity, the variety that takes place in the biliary secretion: now if there is great variety in arterial secretion, we do not marvel, for an artery is a tube of acknowledged active powers. It may do more than ordinary, or less than ordinary, and by modification of action, prepare fluids of different qualities, which we do not wonder at; but is it not wonderful, that from a vein all this should take place? Do we not find that all secretion of bile is suspended for a considerable time, and then that copious secretion of unhealthy qualities takes place? That there is the same variety in the secretion, though it may be performed by the agency of the vein, which we observe when the secretion takes place from the arterial system, I say is curious, but it must be acknowledged as a fact.

Well, then, the bile so prepared passes on by the *ductus hepaticus* into the *duodenum*, for the course of the ductus hepaticus is continued in a straight direction into the duodenum.

The way in which the gall bladder is replenished with bile has been spoken of; and it is a curious thing that the bile

should be detained in the gall bladder, for it is urged out in the greatest abundance at the time the digestion takes place, at the time when the digested aliment is passing downward into the pylorus.

Mr. ABERNETHY described the structure of the gall bladder.

As to the intermediate structure, the *walls* of the gall bladder, what are they? I do not know that any body knows what this part is; and the question is, is it muscular? It does not present any such appearance, so that HALLER set it down as having no contractibility; but HALLER's experiments in contractility are known to be badly advised. HALLER opened the abdomen of an animal, broke the fundus of the gall bladder, and then sewed up the wound; afterwards he found the animal died of peritonitis; that there was a small aperture in the bladder through which the gall passed, and that the gall bladder was contracted to the least possible compass; so, I tell you, it is if the bile never gets into the gall bladder at all. For my own part, I have an aversion to making experiments on living animals, but I have done this: I have got the bladder of a sheep recently killed, where the gall bladder was duly distended; I have made an opening just in the part emerging from the bladder; put the whole into a basin of water, not very warm, but just warm enough not to be cold; left it there for a time, and when I came to it again, I found that the bile was all purged out, and that the gall bladder was contracted. I then blew up the gall bladder to the size it was before, that is, to do away with this contraction, which I may call the last contraction of life. I blew it up, left it again, but I never found it contract any more; therefore the contraction was not owing to the contraction of tissue, not owing to any property it had as matter, but the contraction was the contraction of the gall bladder. Now this slow, but powerfully acting irritability, does prevail to the extent that is required for the functions of the gall bladder; for what is required but to urge the bile slowly and gradually from the gall bladder into the intestines, when the aliment is passing into them? And that is the contractibility which it seems highly probable is performed.

Qualities of the Bile.—Next I come, in physiology, to the qualities of the bile, and with the common properties of it you are all acquainted; it is a sort of soapy and viscid fluid, and this viscidity is owing to gelatine—a sort of gelatinous property which it possesses. Now that may be precipitated by

tannin or by alcohol. If you pour alcohol upon bile, you precipitate all that which gives it viscidity, and you will make what they call the tincture of the bile; you get rid of this gelatinous matter.

Now with respect to this tincture of bile, as I may call it.—But first let me say, it is evident that there is a good deal of *oleaceous* matter in bile, for it crystallises and forms stones. Here are specimens of gall stones; they are composed of a sort of spermaceti, used as soap. If there be a kind of oil in them, why this, incorporated with water, will make a sort of soap, and then we see why gall may be employed to clean cloth with. It has been supposed that there was something peculiar in bile which gave it its bitterness and quality of colouring. Dr. Powell told me that which he was convinced of, and that which I am convinced of, that there was no peculiar matter in the bile which gave it its bitterness and colouring; for if you expose this tincture of bile, as I call it, to the air, it becomes white; and if you drop nitric acid into it, it also becomes white; so that it does not appear that the bitterness or colouring arises from any distinct principle, but from some arrangement which we do not understand of course.

Well, now, I have said as much as seems to me to be requisite to say, for I would never go into the minute analysis of the animal fluids in this theatre. It would be easy to tell you what is said as to the products of bile, and when chemistry shows any thing that is illustrative of physiology, then ought it to be mentioned here; but when it does not, then it is not with that that the anatomist has to deal in this theatre. Well, this sort of fluid goes into the intestines and I have told you that Mr. Hunter says it does not seem at all to incorporate with the digested aliment. It cannot incorporate with it firmly, because the least quantity of bile will taste and tinge a very large quantity of other matter. The chyle is white; it does not taste bitter; they say it has a sweetish taste, that means, I suppose, that it is not sour, or of a particular taste; it cannot, therefore, formally incorporate with the digested aliment, and positively it does incorporate with the fæcal matter, it colours it; and some have thought it tended to purify the digested aliment, and that the gelatinous and resinous matter should mix with the fæcal matter.

Now this is all supposition, we know very little of it; in short, we are very ignorant, I am convinced, of the physiology

of the liver. Some have thought it was an excrementitious matter, something that should be got rid of from the blood; and that is not improbable. It is said to be a proper stimulus of the bowels to keep up the peristaltic action of the bowels, and that any thing excrementitious and useless should be made to pass on. But we are exceedingly ignorant as to what the uses of the liver are; and therefore I come to this proposition, that I never could make my mind believe that nature would construct the very largest organ in the whole body for a trivial purpose, or that that organ could be materially altered in its functions without prejudice to the rest of the body. That is all I have to say with respect to it, and I would have you all to keep it in proper order if you can, and to pay proper attention to this, the largest organ in the body, in all respects.

Of late it has been said, that it goes to the preparation of chyle; but I do not know that such proof is given. I see patients whose biliary discharges have been suspended for months, and yet they seem to be well nourished; still I cannot believe but that they must suffer in some way or other, from the functions of the liver being suspended.

Sympathy.—Now with regard to the sympathies of these alimentary organs in general: that the stomach should be sympathetically affected from the state of the head, is a fact that every vulgar man is acquainted with, and which no medical man doubts, because the eighth pair of nerves proceeds from the brain directly to the stomach. But do not the bowels equally affect the brain? There is no question about it. A very great change of feeling indeed, in the state of the mind, is produced by the relief of irritation in the bowels. The nervous system is amazingly disturbed by disquietude of the bowels. I will tell you a story that was told to me by a London doctor, who was coming home from the London Tavern Dinner: he felt his bowels a little uncomfortable; he had not ordered his carriage, for he thought it would do him good to walk home: he set out, but he limped and crept over the ground, and O, he was most excessively uncomfortable. He found a turmoil in his bowels, which called upon him to knock at a door, and fee the servant, to be admitted to the temple of *Cloacina;* and having cleared the bowels, egad, he said he leaped, and jumped, and walked home with boyish alacrity.

Now does not the liver affect the head? O, undoubtedly;

there is the strongest proof it; but I do not say it is merely the situation of the bile, it is the nervous, the hepatic irritation. And here I call your attention to what is as old as medicine itself; for the very first physicians who taught medicine as part of the natural sciences, at the different schools of Greece, were persuaded of this fact, that dejection of mind, irritability, and fidgets, were produced from something wrong in the *hypochondrium—hypochondriasis,* that was the expression they used with respect to it. Now, how did those men get that knowledge? By adverting to the excretions of their patients. What came from their bowels was examined, and they saw that those discharges were not of the colour, or in any respect resembling those of the healthy state. Well, then, again, the very word *melancholy,* which they also used from a fixed and settled despondency, clearly shows what was on the minds of those men, for melancholy means nothing but black bile, μελαινα being black, and χολη being bile. Now the question is, whether there is any truth in these things? For my own part, I am perfectly convinced, that hepatic irritation disturbs the head, and that therefore, we, as medical men, ought to keep the functions of the liver as right as we can.

Morbid structure.—So much with respect to physiology; and next, with regard to morbid structure. Here I get over the ground rather lightly, for there are certain organs in the body which, if the vessels go into a state of diseased action, they seem to me to produce but one, or scarcely any thing else but one kind of morbid structure; it is an infusion of something into the interstitial parts, in larger or smaller masses, and this we call *tubercles.* The liver is diseased—it is tubercular. There are divers specimens of this sort of disease; sometimes the tubercles are very large, and sometimes they are very small. But what is it that is deposited in the interstices? I am ready to grant, that the newly-formed matter may be so newly laid down, as to give a solidity to the whole mass, and that is *scirrhus,* not cancerous scirrhus, but a solid state of the liver; it is, however, generally in masses, so as to constitute tubercles. Now here is a liver *(presenting a large preparation)* where the tubercles are distinctly shown; they are very large tubercles. I am accustomed to introduce this preparation by saying, it is a very small slice off a very large liver; for positively the patient from whom it was taken, had the whole of

the abdominal cavity filled with the liver; it went down even into the pelvis. Before her death, some speculated about the nature of this large hard substance. Some thought it arose out of the pelvis, and went up to the hypochondrium; and thought that it proceeded from the hypochondrium down into the pelvis; I thought with the latter class. But what was astonishing was, that the patient never had any symptom of a diseased liver. There was no sickness. nor any thing that may be generally observed, as indicating disease of the liver. But you are to understand that these tubercles, to use the language of Mr. HUNTER, may be considered rather as disease *in* a part, than *of* a part, for in this state the internal parts of the liver will secrete bile; and it does happen, that very diseased livers will sometimes secrete very good bile; I have known it repeatedly.

Then, with regard to these tubercles, the question is, are they vascular? And that is a question we cannot determine; there are vessels going through them, but whether they are vascular or not, we cannot tell. This relates to the tubercles of the liver and lungs; and, in either case, it is notorious that the tubercles *suppurate*. You meet with abscesses there, and those abscesses are, no doubt, suppurated tubercles. I have seen numerous abscesses of this kind, which have discharged half a wash-hand basin full of matter, and yet the patients have recovered and done well, the rest of the liver having been sound; and I have known, as I believe, one tubercle breaking in upon another, abscess after abscess forming, and the matter issuing from the side: I remember a man very well, who was a captain of a ship, and of course, a man who had drank a great deal of *grog*; he was in that situation, and the disease in the liver communicated with his bowels. Now I have seen this: the abscess has been dressed while the patient was lying in bed; a rumbling has taken place in his bowels, air has got into them, puffed them up, and actually blown off the dressing!

Hydatids.—Now there are hydatids found in the liver; and wherever you see these *animalculæ*, they form in cysts. You find them, however, in natural cavities, as well as in cysts; and the cysts are so large sometimes, as to occupy the whole lobe of one side of the liver. I say, wherever you meet with them, they are either found in natural cavities which have secreting

surfaces; in the abdomen, in the bursæ of joints, in the bursæ of muscles, and so on; they grow to a considerable size, and burst.

Now, with respect to these things, some believe they are nothing more than coagulable lymph, and some believe they are a peculiar species of animalculæ. The latter opinion have I. You find them in the brain. A sheep has one in its brain, and it goes off in a fit of apoplexy. Now I have had the opportunity of opening the brain of a sheep that died in this manner; upon taking off the top of the skull, opening the brain, and making an incision into the cyst, out has leaped one of those globular hydatids. If you catch one of them in this way in warm water, really you would undoubtedly think that there is vitality in it. They grow to immense sizes, and there are *specks* upon them, which seem to be young hydatids. Now there was a person in this hospital who had them in his belly; it was supposed that something else was wrong with him, and he was ordered to be tapped; and he was tapped, and that let out something like a sort of soapy stuff; he was punctured afterwards, from time to time, and then there seemed something to come out like onions. However, as this mode of going on was very painful to him, the wound was closed, and upon examining the contents of the abdomen, where the pain from the disease had been most acute, there was found to be an immense number of hydatids in the fluid that was contained there, of all sizes. Afterwards I examined some of them with a microscope, but I could find nothing in them more than I could observe with the naked eye, which was, that as the bag was formed, there were specks found which were subsequently thrown off. Now, if they can live and grow —if they can multiply and detach young ones—if those can live and propagate, I think they have every right to be accounted as a species of animalculæ. Dr. John Hunter has published just such a cure as I have related, in a transaction of a society for the promotion of medical and chirurgical knowledge.

Well, you have hydatids, and you have the liver solid, and you have inflammation in the surface of the liver, and adhesions formed—adhesions between the peritoneal covering and the surrounding parts. But this is what does not require to be particularly dwelt upon, because it is what you naturally would expect.

Gall Stones.—Now, with reference to gall-stones, all I have to say is, that they are of a nature to crystallize; there is oleaceous matter in them; there seems to be some law of crystallization prevailing among them. Here are some that were taken from one gall bladder, out of which were taken no less than 1500. Now very large gall-stones do pass through the gall-ducts: the ducts becoming proportionately large; but there are gall-stones voided so large, as to preclude the probability of their having passed through the gall-ducts; and it has been ascertained that there is another way in which an immense stone may pass, without getting through a gall-duct: you recollect that the arch of the colon does not lie far from the fundus of the gall-bladder; and irritation may be produced in the bladder by such a stone, inflammation may be the consequence, ulceration may follow, and through that ulceration the stone may pass into the colon from the fundus of the gall-bladder, and then its further transit is not interrupted.

ON THE LUNGS, AND RESPIRATION.

You may open one lobule of the lungs, introduce a blow-pipe, blow into it, and fill the whole of the lobules of the lungs with air; there is, however, no communication between the air cells, and the interstitial or common connecting cellular tissue. The air you blow into one lobule, will pass on into another, and from that into another, until you have blown up the whole. You may do the same thing with an injection of quicksilver. It appears, therefore, that the air vessels terminate in cells, which are air tight. In blowing up the lungs this way, you never can also fill the interstitial substance. So, again, you may fill up the interstitial substance, or common connecting cellular tissue; but, by doing that, you can never get the injection into the air cells at the same time. In short, the lungs are a sponge, with the air vessels leading into the sponge; and the parts of the sponge are conjoined with one another, but the conjoining substance has no connexion with the sponge itself.

I have represented these air tubes to be *mucous membrane*, and they are mucous membrane throughout; there is great secretion of mucus from them, as every body knows, for it is coughed up, and produces great irritation.

Irritability of the *air cells*. With regard to their irritability

these air vessels are primarily irritable; and the question is, whether these vessels, which permeate the lungs, are irritable or not? Now who can tell that? It is believed that all the mucous membranes of the body are irritable. I do not believe that any body can demonstrate an irritable mucous membrane in the urethra, and yet that part has been known to be of considerable irritability. Now it has been a question among surgeons and physiologists, as to the irritability of these vessels in the lungs; and you may ask my opinion on the subject, or I may at once tell you, that I have no doubt of the air-vessels of the lungs being irritable. What is to be our guide? Our senses are insufficient to inform us; why then, observation, of course, must. Ordinarily, respiration may be said to be a mechanical process; we enlarge the capacity of the chest by the *intercostal muscles*, and the air is forced into the lungs: we diminish the chest, and the air is forced out of the lungs, just as if you were using a pair of bellows. Ordinarily, respiration is carried on merely as a mechanical process; but, extraordinarily, do we not find manifestations that air cannot get into the lungs, though we do endeavour to enlarge the chest? You know you never could lift up the board of the bellows, if you were to stop up the holes that admit the air, and why? On account of the immense weight of the atmosphere. Just so, if you were to put a rope round a man's neck, and stop the air from entering into the trachea; it is not the strongest man that ever lived that could afterwards enlarge that man's chest; to do that, would be to lift up an immense load of air. A man having irritable lungs, may be sitting comfortably enough at the fire-side, but a little smoke comes into the room, and he can breathe no more: he gasps for breath, he cannot enlarge the chest, and he finds the utmost difficulty in respiring; but where is the difficulty? Where is the sensation of pain and contraction? Why, in the lungs themselves; the hindrance is there; I believe it is all irritability. And it is very curious to observe, where you find an irritable state of the lungs disposing to contraction, what slight things affect, what slight things bring on, and relieve, this asthmatic breathing: pure air generally relieves it. People in town, who have asthmatic lungs go about the streets with their shoulders hitched up to their ears, and using every auxiliary to enlarge their chest; and the advice that is generally given is, to take them into the country. I perfectly well remember a neighbour of mine,

whom, upon my life, it was painful to see walk about the street, or breathe any impure air, for you would have thought it would have suffocated him. He had a house in the country; he used to get into his carriage in Bedford Row, to go to it; as soon as he got to the end of Gray's Inn Lane, he drew in a mouthful of fresh air, and then breathed perfectly well. Now, *vice versa*, there are people who cannot live in the country, but who come up to London, and live very comfortably here: and I remember once being a little deceived by a case of this kind; it was the case of a man whose lungs were so asthmatical that he could never lay down in his bed at night, and really his case was so distressing that his physicians advised him to go to the South of France, and winter there. He came to London, in his route to France, and, most whimsically, called on me. I told him, what I tell every body else, that the best thing he could do was, to take care of the state of his stomach. The stomach and the lungs are supplied by the same nerves, and I shall boldly declare to you, that I believe the irritation of the lungs does proceed from the state of the digestive organs. I told him thus, and that that was all I could say; if he chose he might attend to it. In about three weeks afterwards, he called on me, and said, O, I have been living in London, I have been doing as you bade me, and I have not had the least difficulty in breathing since I saw you; and I begin to doubt whether I should go to the South of France at all; it is very inconvenient to me to go there; it is taking me away from my business, from the society of my friends; now what do you think? I said, I do not know what to think, nor do I know how you are to determine, except by going back to your own residence, and seeing how you are when you get there. He went back, and, egad, the very first night he got into bed in his own house, he was almost suffocated. Now, that man lived on the top of a high hill, where the air was of course fresh and pure.

I will tell you of another case; it was that of a man who lived in a state which you would have supposed would have suffocated almost any body. His room was filled with sulphuric acid gas, and he found it relieved his difficulty of breathing in an amazing degree. I say that is whimsical; but all this leads to convince me, that there is a state of irritability in the lungs, and which proceeds from the state of the stomach too.

Now, I have done with what I may call the aerial structure of the lungs—the air tubes, and air cells; and I go on to the blood-vessels.

Blood vessels.—Here are the ramifications of the great pulmonary artery supplying the lungs; but on injecting that artery we find certain discrepances between it and those of the other parts of the body; in others, we see them communicating with one another, but we do not see that in the pulmonary artery; the ramifications of this artery are distributed on the outside of the air cells of the lungs. The blood, as you know, returns to the left auricle of the heart by four pulmonary veins. With regard to the *bronchial* vessels, they unquestionably accompany the ramifications of the *bronchia* throughout the lungs, and are considered as the nutrient arteries of the lungs; but whether they are the sole nutrient arteries of the lungs, is matter of considerable doubt; when Ruysch first discovered these arteries, he naturally enough said, how could people be so absurd as to say, What! the arterious blood afford nutriment to the lungs! Is it not venous blood? Are there not the bronchial vessels, of which I, Frederick Ruysch, am the discoverer? But it was said they were not large enough to afford nutriment to the lungs; he, however, said they were. What creates the greatest doubt is, that you may inject the adhesions between the *pleura-pulmonalis* and the *pleura-costalis*, with subtile injection through the pulmonary artery; and these adhesions are formed by a gelatinous sort of matter. In short, I leave the matter in a state of uncertainty; but the argument that the blood of the pulmonary artery is unfit for nourishment, is, I think, not a very valid one, inasmuch as though it went for the venous blood, it became arterious blood in its branch through the lungs. Well, so much for the bronchial vessels.

Absorbents.—Now the lungs contain numberless absorbent vessels; these are deep-seated absorbents; they are very numerous, and they are not valvular. All these absorbents pass through a number of absorbent glands, which are seated about the primary ramifications of the bronchia. For a long time the nature of these glands was unknown; they were thought to be mucous glands for some time, but skilful injections have clearly proved that they are absorbent glands; the absorbents pass through them; and these are the bronchial absorbent glands. They have thin *capsules*, and when you cut them open they appear to be cellular, and you find a fluid in them that is

black, and will soil any thing it touches. It is found that this black fluid is owing to *carbon;* and the question is, how it got there. Then it has been generally thought that it must be from the dirt of the atmosphere which we inhale. I know that there is a great deal of dirt gets into our lungs, we who live in this town at least; for those who are in the habit of spitting in a morning, spit up stuff as black as soot, so that it must get into the lungs from the atmosphere, in London at least. But I observe that, in London, if I open a child, I find no carbon in these glands; it is red fluid I find in them. Now I do not see why a child's absorbents should not be as active as an adult's; and I find that if I open an old man in the country, (and many have I opened there) I find this carbon in their lungs, just the same as I find it in the lungs of those who live and die in London; therefore I am puzzled about it, and I cannot tell you any more respecting it than that we find this black dirty stuff in them.

Functions.—Now, with regard to the functions of the lungs, I have very little to say indeed; but, formerly, a great deal was to be said, and I may affirm, that one of the most beautiful theories that ever was erected by human ingenuity has been completely overthrown, and that there is none other erected in its stead.

Physiology.—In speaking of the physiology of the lungs, there are certain facts we have to advert to, and one is, the change wrought upon the air inspired. Now what we draw in by inspiration, we know very well contains twenty-seven parts of pure air, oxygen gas; seventy-two of azote, and one, or so, of carbon. Then we know that what we expire abounds with carbonic acid gas, and that a certain portion of oxygen is taken away from it; this is the change wrought in the air we inspire. And what is the change wrought in the blood by respiration? That which went forth by the pulmonary artery purple—dark purple blood—returns by the veins scarlet-coloured blood. Then it is believed now that the change is wrought principally by parting with the carbon. I cannot, however, believe that that is the sole cause of the change, because it is such a striking difference. It is said that the blood is carbonized by the action of the air in the lungs. Now be good enough to be aware that what takes place in the lungs takes place also in a basin. You bleed your patient, and let the blood cool; you find the crassamentum with a bright scarlet coat on the sur-

face; turn the clot upside down; O, you see the bottom of a black purple; but wait a little, and that black purple will change to scarlet. Well, all this is as it were by the exposition of the blood to air, through the medium of the serum. It seems to be a chemical change that takes place—an action or a re-action of a chemical nature which takes place upon the blood in consequence of an exposure to the air; and a certain degree of medium does not prevent this. Then it is said that the carbon goes off from the blood, and makes the carbonic acid gas. Now that may be; those who are believed to be the most accurate experimentalists affirm, that there is no more carbonic acid gas thrown out in expiration than can be made by the diminished quantity of the oxygen which has been drawn in by inspiration; that there is no more oxygen consumed than what is adequate to produce the exact quantity of carbonic acid gas that has been given out. This is the opinion of the best experimentalists, and therefore it is the general opinion of the profession with regard to that subject. Formerly, it was supposed that oxygen was taken into the blood, and that thus it obtained its carmine colour; but all now say it is not so. Those who suppose oxygen to be taken into the blood thought that the lungs were a source of animal heat, and that the oxygen meeting with the carbon in its round, the carbon united with it, and thus did they account for the oxygen in the body; but if you deny the ingress of oxygen, all this falls to the ground. Again, there seems to be another circumstance which gives a death blow to the theory of oxygen being imbibed into the blood, and these are the theories of Mr. Brodie. You know that circulation will be continued if respiration be continued, even though the brain has no influence upon the body.

Le Gallois took away the brain of an animal, and continuing respiration, circulation was continued; that is, he did not take away the base of the brain, but removing the upper part of the brain to the base, circulation was continued. But Mr. Brodie divided the *medulla spinalis*; he pithed an animal, and by that means, as I may say, killed it. Afterwards he kept up artificial respiration for three hours, and the blood circulated all that time, and it underwent the change from scarlet to purple successively; yet that animal cooled quicker than another animal, which was killed in the same manner, where no inflation of the lungs had taken place: so that inflation of the lungs can-

not be the cause of animal heat. But still people cannot get the idea out of their heads, that there may be some source of animal heat here, but requiring the action of the nervous system to render it efficient; that there may be some change wrought in the blood, which, under the influence of the nervous system, may be productive of augmented temperature. But we will say that is opinion.

Now you know I profess myself to be a great admirer of JOHN HUNTER, and of all JOHN HUNTER'S opinions; for I know not any man who ever considered the different subjects of physiology with the same depth of reflection, and with so unprejudiced a mind, as he did. Never did I hear Mr. HUNTER say a thing but as an inference from facts; he had no opinion that was not an inference from facts; he has given no opinion about the uses of the bile. What does he say of respiration? He says this; that it seems to impart life to the blood, which life becomes distributed to all parts of the body. What is the fact on which he grounds this opinion? Why, that if an animal did not respire, he would die; that is all; that life, was some subtile principle, which he believed pervaded every part, and without which the body could not be supported. HALLER came to the conclusion that this principle of life originates in, and is supported by our food; now, if HALLER had gone a little further, he would have completely come up to my ideas; for I do believe that a principle of vitality is imparted to digested aliment, and that it is, as it were, resuscitated and kept up during life by respiration; that is the notion I have of it. No animals will live without the exposure—without the ventilation, as I may call it, of the circulating fluids. You may drown a fish, by depriving water of all the pure air—boiling it will deprive it of all the pure air; cork it up in a bottle, and cool it to the temperature that a fish would delight to live in; then immerse the fish, take up the gills, and you will see that the blood in the bronchia will become purple, and the fish will be drowned, but not quickly. Then it is as you please; you may account for respiration—for the use of respiration—by saying that it decarbonates the blood, that it takes from it a principle which is noxious; or you may be inclined to believe that some principle useful to the body is superadded at that time, but a principle that we cannot define. You may think as you please on this subject, you are welcome to do so; I have told you all I have to say respecting it.

Actions of the Heart.—Now, with regard to the actions of the heart, Mr. HUNTER says—for he made these experiments: he wrote upon the cause of death in drowned animals, and made numerous experiments—and Mr. HUNTER says, that the chief dependence of the heart for its action is upon the lungs. He contrived a pair of bellows, by which he could continue a sort of artificial life in an animal; and he says, so long as I continued to blow with my bellows, so long was the life continued; but when I ceased blowing, the life seemed gradually to decline; when it had nearly ceased, resuming the operation by the bellows again, life seemed to renew; feebly, indeed, at first, but gradually increasing till it was brought up to its full action. Now that is JOHN HUNTER. There certainly must be considerable sympathy between the heart and the lungs. The eighth pair of nerves communicate branches to the heart, and there can be no doubt of this sympathy existing. Mr. HUNTER, could not find any case in which it was equally probable that a sympathy existed between the lungs and the heart; it seems to be difficult to meet with such a case, but that sympathy is reciprocal between these two organs, nobody can doubt. The old physiologists believed, when a man died suffocated, that he died because the blood could not pass from the right side of his heart to the left. Now a doctor, who published in Edinburgh, and afterwards went to the West Indies, affirmed, that this was not a true view of the subject. I forget the name of the doctor; I am very forgetful of names; but the way he took to show it was this: he put a cord round the windpipe of an animal, and stopped the ingress of air into the lungs; he then opened the chest of the animal, and he found that the blood did circulate through the lungs, though the windpipe was tied, and that the animal died with the left cavities of the heart full of the purple blood—blood that had not undergone the change by the reciprocal action that exists between blood and the air—and therefore says, he, this is not the cause, but the cause is, that the purple blood is not a proper stimulus to the left side of the heart. Now I am inclined to think, that this gentleman omitted to notice the circumstance relative to the tenacity of irritability in the different parts of the heart, namely, that let an animal die when it will, the left cavities of the heart soonest lose their irritability. But BICHAT has put all this in a clear point of view; he has shown that the animal dies from the circulation of purple blood, and that if purple blood is distributed to the brain, it is prejudicial to life.

He injected purple blood into the pulmonary vein, and it immediately killed. The true exposition is, then, that life cannot be supported without this pulmonary blood; the circulation of purple blood destroys life. But then, persons getting this into their heads, think it is not necessary for the circulation of blood in the lungs: now, to me, it appears to be necessary. If I inspire, and hold my breath in, I can remain sometime easy; if I expire, and cease breathing, I immediately feel uncomfortable, and begin to look pale in the face. You may say—aye, but that is because your lungs contain less air. Now I observe, in that case, that the very veins of the forehead swell; and that seems to me to show that there is an impediment produced to the circulation of the blood by expiration; and that inspiration produces a facility in the influx of blood through the lungs, I believe, will be generally admitted now. Inspiration creates a vacuum, or would create a vacuum, did not the air rush in to fill up the parts: it is an indraught to air; and the question is, whether it be not an indraught to blood also? Now observations have been made to this effect repeatedly; but of late a Dr. Barry has made experiments, and suggested a theory of his own; respecting respiration, and the main experiment is curious and interesting: he immerses a tube in fluid, and inserts the tube into the jugular vein; he then observes, that when the animal draws in a deep chest-full of air, the fluid is taken up from the tube; that there is an indraught to the blood, both in the larger veins leading to the heart, and in all probability in the vessels of the lungs, so as to facilitate the circulation. And also, if that is of use, we may infer that pressure is also of use in urging on the circulating blood. That there is pressure in inspiration, seems undoubted, from what we observe in the head: if a person is trephined, we see the arterial pulsation. There may also be a pressure on the venæ cavæ, in a way to impede the ready return of blood, or to propel it, in a degree, into the jugular vein, for we see it very much distended.

Well, then, this pressure equally acts on the veins before they arrive at the heart, and upon the pulmonary arteries too; therefore I think it extremely probable, that inspiration does facilitate the progress of the blood through the lungs, and that deep inspiration does relieve the head. Dr. Wollaston, who is a very thinking man, has proposed, as an account of one good derived by sighing, that it relieves the vessels of the brain; we

make a very great indraught of air, and as a good effected by it, the vessels of the brain are relieved. But when done, you see how uncertain we are of all these matters.

Again, I should beg of you not to think of the lungs simply as effecting the changes which I have been alluding to; they produce many other good effects. There is agitation in the abdominal viscera, you know, which seems also to contribute to the circulation of blood in these organs. But what complicated uses do the lungs serve? Is it not by the indraught of air, that we draw in our first food by suction? Is it not by a kind of suction at the back of the mouth, that we swallow? Is it not by the air passing up through the nose that we possess the sense of smelling? Is it not by the means of air, that we possess the voice? Those animals who have no respiratory powers, are very much put to it to communicate their feelings to one another: the *fly* makes a sort of drumming noise with its wings; the *crickets*, also make a sort of chirping noise. And they say the *death watch*, which produces so much alarm to many persons, is but an annunciation of the *amorous* propensities of a kind of *beetle*, which strikes against the table. Well, then, there are many purposes served by the powers of respiration.

LECTURE XXXVI.

ON THE HEART.

Malformations of the heart.—I have now to speak of the morbid anatomy of the heart; but before I do this I may mention that there are *malformations* of that organ. Dr. BAILLIE has mentioned a case in which there was no *pericardium*; therefore that is not an essential part, though it might be useful for connecting the heart to the *diaphragm*. Then, again, there are a great number of malformations with regard to the vessels; but one of the most frequent, perhaps, and one most explanatory of all the others, I shall describe. I remember the time when Dr. SANDIFORT's description of a *puer cæruleus* was considered as something miraculous; but these cases are now exceedingly common. I happened to examine a body of this

kind; a lad, who was about seven or eight years old, had had occasional fits of suspended, or very much impaired respiration, and at such times purple blood was circulated throughout the body. The lad was slim, of the usual height, and from an early period of his life he was subject to these attacks. When the fits attacked him, he threw himself on his breast, and uttered a scream; and there he lay for such a length of time, that the by-standers thought he would never have breathed more: he became purple, and very cold; then drawing in another deep breath, he gave another scream, and again remained silent. Those fits sometimes held him down for a quarter of an hour, and sometimes longer. At last he died in one of those fits, and I was asked to examine the body. On cutting open the heart in the way I usually proceed, and having cut through the right auricle into the right ventricle, I went on from the right ventricle, and, to my utter astonishment, I cut into the aorta. Here is the heart laid open; the preparation is here, and many others of the same kind. The left side of the heart was much smaller than common, and there was a perforation in the left *septum ventriculorum*. To make short of the business, I may say, at each action of the heart an equal quantity of arterious and venous blood seemed to be propelled into the organ.

I also examined the body of a girl, of seventeen or eighteen years of age, who had this malformation. I opened the body of a child likewise, who, in the early period of its life, had had the distribution of purple blood throughout the body. I was asked to see this boy, he was then seven or eight years of age, and as soon as I saw him, I said to his mother, you must be aware, Madam, that this is the result of some malformation in the heart. She said, you do not seem to recollect the little boy? and I said, certainly I do not. Now this was a boy I had myself attended at a very early age, and whose case I immediately recollected. He had had one of his arms just like a skeleton; it was wasted away, in short, to a perfect skeleton, and entirely owing to the state of his alimentary organs. As his bowels were put to rights, the arm became as well as the other; and I own to you, I was at this time rather more intent upon examining the arm, which I found to be as sound and muscular as the other, than the then state of the child's health. However, he died, and I opened him, and there seemed to have been a gradual diminution of the pulmonary artery from the

child's youth. First there seemed to have been a sufficient flow of blood throughout the artery, to have prevented the purple appearance. Blood of this kind may pass, but then, when imperfect respiration occurs, the blood becomes purple and cold. Well, this is the state of those who have that purple appearance from there occasionally being more venous blood than there ought to be passing through the vessels.

Morbid anatomy of the heart.—Now I have to speak of the morbid anatomy of the heart, and my arrangement of doing this is, first to speak of the morbid anatomy of the coverings; and here I begin with the *pericardium.*

Dropsy of the pericardium.—I have said you are not to confound a *dropsy* of the pericardium with serum, or even bloody serum, since that may be nothing more than the transudation of the blood through the vessels.

But there may be dropsy of the pericardium, and that too to a great extent. What we call dropsy, is an increase of fluid without inflammation; and there is that to the extent that the diaphragm is very considerably pressed down. Some have speculated upon opening the pericardium in this case. O, such an operation is perhaps one that we are not warranted to undertake, because it is not likely to be ultimately successful. However, I say it is a speculation; it is no regular operation, and for my own part, I am not one of those who are desirous of recommending or discussing novel projects of this sort.

Then I have to tell you, that in any case of extreme dropsy in the pericardium that I have met with, I have constantly found a very small, and a very quick pulse; and this is just what I should think would occur. The pericardium is a strong membrane; yields reluctantly; and if there is great pressure in the pericardium, it must press on all the cavities of the heart, and occasion a small and frequent pulse; for if the quantity of blood is to be transmitted on by small portions, it will have to be by more frequent actions of the heart.

Inflammation of the pericardium.—Now the pericardium is liable to inflammation; and where there is inflammation of the pericardium, it sometimes produces a considerable effusion into the bag, of turbid liquor, and of a deposition of jelly upon the polished surface; the bag, too, becomes white and thick. You may have great distention of the pericardium from inflammatory action, but with a sort of resinous appearance on the

surface, and thickness of the bag. I show you a magnificent preparation, as I may call it, of that kind. This is a representation that Dr. BAILLIE has given as one effect of pericarditis; but it is a magnified view of the representation he has made; it is a grand specimen. But you have different effects produced from the same cause; sometimes you have an abolition of the bag of the pericardium; there is an adhesion between the two membranes, and here is a specimen to show that. In these cases, a great deal of gelatinous deposition has taken place on both surfaces.

Symptoms.—Now I have always, in this lecture, conceived some account of the symptoms which I myself have known in cases of pericarditis, useful to be mentioned. Many a man feels a pain in the heart, when there is no disease there; but I never knew any one who was labouring under an inflammation of the heart who complained of pain in the part actually affected; they either did not complain of pain at all, or referred it to some other part, not in the region of the heart. I have known them complain of pain in the region of the liver. But in those cases of acute pericarditis, it is not to be supposed that the disease which affects the heart will not affect the flesh of the heart, and will not render it irritable. I have known it to be extremely irritable, having the greatest pain, acting with the greatest force, and then, as if exhausted, it has gone on with its function in a very slow manner. Now, in describing this pericarditis, I tell the case of a physician, who had come from the East Indies, and that perhaps might have tended to mislead the minds of his medical advisers, for he died, and they really knew not what the nature of his disease was; but yet it was distinguished by this peculiar symptom that I am describing. He referred his pain to the region of the liver, as if immediately coming from the liver; he was treated as if he suffered from hepatic affection merely; but the most curious circumstance was, that at one time his pulse would beat so quickly as not to be numbered, and that then it would become exceedingly slow. Really, towards the last period of his life, his attendants have gone out of the room, not liking to witness his last agonies, feeling certain, that from the state of his pulse, he could not survive many minutes; and upon returning into the room again, they have found him in a languid state, with a pulse beating very low indeed, perhaps not more than forty in a minute; just as if the heart was thrown into fits of irri-

tability, in which fits it would act with the utmost vehemence, till it became exhausted, and then carry on its functions, as I have expressed it, in a very languid manner.

Ossification.—You have ossification in the pericardium; and again, ossification on the surface of the heart. And here are preparations showing these, but I have no remarks to make respecting them.

Lining Membrane.—Well, now having gone through those diseases that affect the *coverings* of the heart, I next speak of the appearances in the *lining membrane* of the heart. And here I have to tell you that when *inflammation* takes place in this part, that the lining membrane becomes white, loses its transparency, and acquires a roughness on its surfaces; and thus you will know there is inflammation. Now, under these circumstances, growths may take place from the membrane—there may be polypi. I have known them seemingly obstruct the current of circulation before the death of the patient. I have known cases where, upon cutting into them, some water has issued out; there were certain growths, but those are rare occurrences. I have put up here a specimen, but I will not swear that it is polypus; I have put up a specimen of a deposition in the left auricle of the heart: All I can say is, that membrane is white and rough, and such as might lead to a deposition; but whether it is an actual deposition, or something adherent to the auricle, is more than I would take upon myself to affirm. The auricle is also enlarged, and the ventricle is proportionably contracted.

Valves.—Well, then, when this membrane is in the state I have described, the *valves* become thickened, for they are duplicatures of the membrane; there is something deposited in the duplicatures. Then, after they have become white and thick from this newly deposited substance, osseous matter is secreted. This is what we see in blood vessels generally; but you cannot well discern osseous matter deposited in the valves of the heart, when they are put up in spirits—in common spirits; but here are valves in the heart, very much ossified, as you will see. Now this is a curious circumstance, that when they are much ossified, they are not capable of being reflected up so as to shut the opening between the ventricle and the auricle; and therefore when the ventricle retracts, part of the blood will be propelled into the auricle, and the collision will occasion a sort of bound; and there is a peculiar

sensation created by this. I have many times been able to say, by feeling the pulse, that there was ossification in the membrane of the heart, and the event has proved that I was right. I distinguished it by a sort of *bound* in the pulse. Now it is curious to know that people live for a considerable time with this degree of obstruction in the circulating organs; but here a very curious thing happens, in this irritable state of the heart, which is, that you have a contraction of the *sinus venosus*. Now this is a piece of anatomical knowledge not mentioned by Dr. Baillie; and I own, when I found it out, that it was to me a matter of very considerable surprise: I dare say it may be five-and-thirty years ago, since a young man was in this hospital who had been labouring under some affection of his heart, at that time, three years or more. He was then about nineteen years of age; a tall, bulky man, but his bulk was increased by general œdema. He had a cough: there was a watery fluid in all the cavities of his body; his veins were turbid; his colour purple, and there was evidently some obstruction in the circulation, connected with the circumstances of his case; there was an extremely rapid pulse, proving that the left side of the heart received but very small quantities of blood. Now before his death, without having any idea of *stricture* in the sinus venosus, I said, I am sure there is something here interrupting the circulation, and intervening between the left ventricle of the heart; some tumour, or something. Well, when I opened him, I found stricture in the sinus venosus; a thing I had never met with before, or even dreamed of. Here is the heart. However, after having once met with it, I found it often in other cases. And how can this happen?—that is the question. Why, if you think a little, you will understand it: the *carneæ columnæ* of the heart send their cords into the *tricuspid* valve, and if they are irritable—if an inflammatory action is going on in them—it will tend to narrow the area of the sinus venosus. I think that is a phenomenon which may explain it rationally enough, but still it must be considered a strange thing. That the carnea columna is capable of this, you may satisfy yourselves: take the heart of any animal just killed, lay open the right ventricle, put your finger into it, and you will find the sinus venosus considerably contracted. It is naturally a large ring, perhaps an inch and a half in diameter, but here it is contracted, and permanently contracted, so as to impede the passage of the blood

from the left auricle into the left ventricle. Now there has always been an exceedingly small pulse, in such a state of the heart as I have described. Well, but this disposition going on, a sort of disease takes place; a thinning, a thinning of the structure of the heart—sometimes of parts of it; and then the heart is liable to burst; at least, all the burst parts in hearts that I have seen, have been the effect of disease; and so also in burst bladders. In these cases, there has been some weakening and thinning of the organ, and then when the organ has been filled, and pressed upon, the thinned parts have been burst through. Here is a heart that was in that way, and the man died immediately.

Well, these are the principal morbid appearances; whitening and thickening of the membrane, and roughening of its polished surfaces, something like excrescence absolutely produced by the disease; but do not believe that this is common. And I tell you that the mere coagulum of blood will impress on your minds the presence of this disease; but let me inform you that the ossification of the valves, in disease that weakens the structure of the heart at particular parts, makes its appearance very frequently in the left cavities of the heart, and very, very rarely in the right.

Now the valves of the vessels may be ossified—the valves of the aorta may be ossified; and here is a most remarkable specimen of this kind. The ventricle, here, is laid open, and it is immensely large—immensely large. Now this heart was taken from a public character in this town, whose case excited considerable interest, and who was attended by many learned men. I actually filled the ventricle with water, which was very large, as I have said, and it dropped out of a little *chink* that you will observe in the preparation. Under these circumstances, I could not but ask, with great solicitude, what sort of pulse the patient had? All the answer I could possibly get was, that he had *a very peculiar one*; but I could not get out, in what that peculiarity consisted. I questioned how he could have had any pulse at all, when there was so narrow an aperture to project the blood through.

Now, here we see a reason why the left ventricle should become so enlarged; it would be difficult to discharge its contents, and it would become large, just like the bladder: and I own I was fool enough to think, for a great part of my life, that if the semilunar valves of the aorta became ossified, they

would, in consequence, become reflected towards the heart, and shut up the area of the aorta; but I now see no reason for this opinion. I have positively found the valves ossified, in cases where they did shut up the area of the aorta; and I have seen other cases, in which the left ventricle of the heart became excessively enlarged, without any ossification of the valves at all. Now this case I met with first in a man who was a patient in this hospital, where the left ventricle of the heart was so much an overmatch for the aortic system, that all the large vessels leading from the heart became exceedingly large; I may say, a third larger than usual. Really, nobody could walk along the ward without taking notice of him: the carotid arteries were seen beating—the clavicular arteries were seen pulsating. It was a circumstance that would have struck any body; he died. The left ventricle of the heart was found to be most enormously large; all the vessels leading from it greatly enlarged, but the valves were not ossified, nor could you find any thing to account for the largeness of the ventricle of the heart. I may tell you, there was a small abscess in one lung, but the lungs were otherwise sound: I may tell you that as a fact; but one cannot see how this could have had any concern with the enlargement of the ventricle. Now this enlargement of the left ventricle of the heart, is no uncommon occurrence: I find it very much the fashion to call it a *rheumatic affection,* and that certainly very much entertains me, who have long been a rheumatic subject. To think that the left ventricle of the heart would become enlarged from rheumatism, seems to me to be a strange sort of idea—such as would never enter into my mind. But the same thing that produces this, may produce other things; for I am a very rheumatic subject, and have a very irritable heart.

But I go on to tell you another case, and this happened to a medical man, a friend of mine, no great number of years ago. This was a man of a remarkably vigorous constitution; he was the talk of the whole county; he had a very great extent of medical practice; called up all the nights in the week attending midwifery cases, and riding more miles a day than a postboy; and never did any thing appear to make him sick or sorry, till he arrived at the age of about fifty years; then he seemed to be ill, and his *bowels* were not right. O, but he still went on in his old habits and active employment, till he was seized with a pain shooting through his chest to his back-

bone, having a feeling of suffocation, and he thought he would die. However, he was relieved; and after a time, the fit of pain came on at night—regularly at a particular hour of the night: People sat with him in his room, and he seemed comfortable enough for a time; but when the hand of the clock pointed to a certain hour, he awoke from his sleep in this pain. It was under these circumstances he asked my opinion. I went to see him, and when I felt his pulse, I was really astonished, it was so extremely large and strong. I did not like to alarm him; and I said, this is such a pulse as probably you country gentlemen are accustomed to have, but it is certainly too large for Londoners. You ought to be bled; at any rate, this pulse shows the necessity of abstinence; after a time he got gout or rheumatism in his foot, and then he was not disturbed with this suffocation or pain at night; nor did these attacks ever return upon him, in a manner to interfere with his professional avocations. When he died, he left it in his will, that his body should be examined by me. I went down and examined it, and all I can tell you about it is, that the left ventricle was enormously strong, that all the arteries leading from the heart were extremely large; that there was no particular ossification; he had tubercular liver and spleen. Now I have no doubt that this man's disorder began in his alimentary organs; I have no doubt of it. And here I go on prosecuting a subject, which subject, I think, belongs to Mr. HUNTER—*sympathies.*

Sympathy.—With regard to *sympathetic affections,* an organ may be made very irritable and fidgetty from sympathy with another organ, and the sympathetically affected organ may be getting into the worst state of the two. Now I am convinced that disorder of the digestive organs will materially disturb the heart. In short, there are two sets of organs with which I say the heart peculiarly sympathises; the one is, the *head,* and the other, the *alimentary organs.* Of how it is caused by the *cerebral* affection, I have already told you; I am now speaking to you upon the other. But if an organ is kept in a state of irritation, it will go into disease; it will lead to disease—to *organic disease.* These words seem to be objected to by medical men; and then they say, there is *functional* disease which leads to *structural* disease.

Well, then I say, we must endeavour to relieve this functional disease; and that which leads to structural disease in the heart may be liable to lead to rheumatism in other parts. I

can only tell you, I have had, all my life time, one of the most irritable hearts possible; and sometimes I have thought I should die, just as JOHN HUNTER did, of an affection of the heart; but that was, perhaps, only a hypochondriacal feeling. I remember when I was young, that my heart used to beat at such a rate, as to make me think that I had an aneurism; it did not get worse; it was always worse after dinner; however, by degrees I ceased to think about it, and perhaps it diminished—perhaps it did. I was first reminded of this again, by being excessively distressed and annoyed from attending upon a patient who died. When I laid down in bed of a night, my heart intermitted to such an extent, that I thought it never would beat again; then it would go on with the utmost vehemence for a time, but as the anxiety of my mind subsided, so this state went off. Then I was next reminded, that my heart might put my stomach out of its order, from my own observations; then my heart was at its vagaries again. I have known it to beat 160 in one minute, and not sixty in the next, intermitting in the strangest way possible. However, my stomach was wrong, and being interested, as you may be inclined to suppose, in this case, I resolved to consult a physician. I let him feel my pulse, and he said, you have got a touch of *angina pectoris*. Well, but, said I, telling me what I have got, is not of so much consequence as telling me what I am to do. He said, you are not to take wine—you are not to take this, that, and the other thing, all which I attended to. I was extremely hypochondriacal. He recommended me to sleep in the country; I did so; and I remember once, just by the friction of my shirt, I had produced a large blister over my heart, and when I was stripped, I found my shirt all over serum and blood. Now this irritability I have known in many other cases, and I felt, personally, that relief from taking the *blue pill*, which I never could have believed could have been produced, had I not felt it. An *Irishman* once said to me, "O, Sir, I shall be bound to pray for you as long as I live, and *ever afterwards*. I declare to God, before I took those pills, I was in that state which I should have been thankful to any body who would have come and blown my brains out; but ever since I took them, I have been happy."

Now I have seen plenty of cases of great affection of the heart having been relieved, by putting the bowels to rights. There was a *chere amie* of one of the pupils here, whom he

asked me to see; and, upon my word, I thought she had an organic affection; but recollecting these facts, incident to my own case and others, I said, "Pray, Madam, is there not any particular time at which you find your heart get worse?" "O, yes, always after breakfast." "Pray, what do you take to breakfast?" "Tea." "O do not take tea any more; I would never take into my stomach that which seemed to provoke the complaint. This led to a little lecture on diet, and the result was, that she was to take bread and milk; however, I thought it was a lost case. It was about a year after this time, that was going up a street, and just about to turn a corner, that I met a man: he took off his hat; I took off mine; he looked, and I stared. We gradually approached each other. He asked me how I did, and I hoped he was well. We talked a little about the *weather*, and, in short, I was almost lost: but, by this time, I recollected him, and not liking to enquire about the girl, but still thinking it better to say something, than to stand and say nothing, I did venture to say, "Pray, Sir, may I be allowed to ask how the young lady is?" "O, Sir, you have cured her, perfectly cured her, by causing her to take bread and milk to breakfast."

I remember a man who cried me up as a very great *doctor*; a *glutton* of a fellow, he was, and no doctor could ever cure him. I said to him one day, "Pray, Sir, do you think if a man was to take a new laid egg and a piece of bread and butter three times a-day, he would starve?" He considered it for a time, and at last he dragged out, "No, why no, Sir, I don't think he would." "Well, then," said I, "I would advise you to try that plan." After that, he sent me lots of patients, and perhaps I did not attend to them as he thought I ought to have done, and therefore he said, "O, I know how to attend to these patients myself, and I am quite sure, if they will swallow a hard boiled egg every six hours, they will do."

Then there is a state of the heart, the exact reverse of all this, where the flesh of the heart degenerates, where you can hardly recognise it as a muscular structure; and what are we to ascribe this to? Now this was the case with Mr. HUNTER; and as I am a person of a very prejudiced mind, I am persuaded that JOHN HUNTER'S heart was primarily affected, either from his head, or from the state of his bowels. I have no doubt that he had been long ill, from the state of his bowels; but his heart got into a state, first of functional, and next of organic

disease. To what are we to attribute all this? I say his heart at times scarcely circulated any blood at all. Occasionally he became as pale as a corpse; and when he was seized with this fit of feeble action, he had extreme pain darting through from his breast-bone to his back-bone: he stood before a mirror looking at himself, he appearing as a corpse, and in this way trying to feel his own pulse; but he could feel none, nor could any body else. Now it is curious, (and this shows that one should always suspect one's self,) that JOHN HUNTER ever afterwards thought that the state of his heart was a proof of the suspension of the involuntary powers, while the voluntary powers could continue in motion. He used to give that account of his own case. He said, as life was continued, and as circulation had discontinued, he had no need of respiring, but that he did occasionally respire, as he thought it did him good.

Dr. JENNER, who was a clear-headed man, said, if ever JOHN HUNTER dies, and is examined, I am sure you will find the *coronary vessels* of his heart ossified; and this certainly was the case; it is a fact, that the coronary vessels of Mr. HUNTER's heart were ossified. Now Dr. PARRY has singled out a set of cases from all the cases that are related of this nature, and which are worthy of your attention.

Now I have told you all I know as to the morbid anatomy of the heart. That is particularly my business. I have connected the account which I have given you, with a recital of those symptoms which I myself have been especially struck with; and I deem it right to impress on your minds the subject of sympathetic affection, because organs may be sympathetically affected, and those, kept in a state of sympathetic irritation, may be attended with more fatal consequences than the disease of the primarily affected organ will produce. I could tell you of cases till you were tired; you are tired already, and I could tire you out, and out, and out again. I know that people are pretty much disposed to laugh at the notions I entertain, and I do not want to weary you further by enforcing them upon you. I tell you them, however, and I hope you will remember them; for I am sure you will find them useful in practice. I cannot be wrong in this, considering that I am continually meeting with repeated proofs of this doctrine, which I have now been teaching during the greater period of my life.

ON THE VOICE, LUNGS, LARYNX, EPIGLOTTIS, ŒSOPHAGUS, AND BLOOD-VESSELS.

Physiology of the Voice.—It is questioned how the tone of the voice is produced by the muscles of the throat; that I have described; for that it is produced by those muscles is manifest, by the division of the recurrent nerve that supplies them. I said, in speaking of that nerve, that experiments had been made on that most noisy of all animals when under torture, namely, a pig, that when that nerve was divided, no sound was induced. Then, I say, how is this done? Does the tone depend upon the enlargement or diminution of the aperture, or does it depend upon the tension of the strings? Now, for my own part, I am not competent to say, and when I cannot make up my own mind on any subject, I always form my opinion from the opinions of those who are sound-headed, and inclined to labour; and I find that HALLER affirms that it entirely depends upon the tension of the strings, and not in the enlargement or diminution of the aperture. You know, on wind instruments, you blow an octave higher with the same aperture, only by shortening the tube; and this also takes place in the larynx, as I shall afterwards mention: but this relates to the note, and not to the tone of the voice. As to the tone, there is no tone if the muscles do not act; and the muscles seem to produce tone by tightening the strings. A two-stringed instrument—how can this produce tone? Now here, you know, you must attend to the construction of musical instruments in general. The strings are not the cause of the tone; the tone arises from the vibration of the wood by which those strings are surrounded. In the harpsichord, do the strings give the sound, or the vibration of the wood? Every one knows that it is the vibration of the wood. So it is also with the fiddle. It is the wood, then, that gives the vibration, and this communicates with the air, as the material which communicates sound to us. It is in this way, too, that the tone of the voice is produced; and it is very curious that such results should take place from such simple mechanism; but you are already apprised of how these results do take place. I know a musician, who swears that he could prick out a tune from Madame CATALANI's throat when she is at her highest stretch, and that when he had gone through the whole, he would not be found to be

half a note wrong. Now he may stretch a little in that statement, or perhaps he may not; but that is what he says. Then, again, the current of tone is split and subdivided, and so are particular sounds. With regard to this splitting and subdivision of the current of sound, necessary for articulation of words, and communications of feelings, I would have you study that, to a certain extent at any rate. It is a very curious thing, that from this study there has resulted an effect, which is the greatest that we can imagine to have been produced by human labour and observation, that of teaching the deaf to speak—teaching a man who never heard a sound to communicate his sentiments to another, and be capable of understanding merely by the observation of the lips. Now, then, the outlines of the study are formed in the following manner: All *vowels* seem to be but notes of the voice; they are all done in the larynx. Then *consonants* are divided into *labial, lingual, dental,* and so on, compound. The labial consonants called *b, p,* and *m,* are produced simply by the close of the lips; and this being a motion which a child might perform without volition, the first thing a child pronounces is *ba, ba, papa, papa.* Lingual, *d, t, l,* and so on, *ta, ta, la, la; g* is a lingual, it is done by the back part of the mouth, and is certainly the very first sound a child utters, *ga, ga, ga; s* and *z* are dental, or compound, and are pronounced by a sort of hissing through the teeth. Now I would have you think of this; read about it, if you like. I can tell you it has often served me, and been of essential good to me. There is a family that do not speak, and you are called in to see if you can be of any assistance to them. I remember a family where there was a gawkey child running about, of five years old; I asked it a question, and the mother said, "O, Sir, my child does not speak yet; none of them speak till they are seven years old." There was another family, where there was a thick lipped child, of considerable age, and who could not speak; I said to his mother, "Can you teach your child to shut his lips?" She said, Yes. She tried it; she taught him to do so, and then he said, *papa* and *mamma.* It was with some difficulty that he was thus taught, but he did learn to speak. Now you may correct this in your own family. I had myself a child who grew to a considerable age before it could utter *f* or *w,* and by showing her, with considerable pains, how these letters were to be pronounced, she did pronounce them, and as soon as she had done so, she ran about the house the whole of that

day, crying, *fire, fire, wood, wood.* Therefore I say there is a great deal of good to be got by attending to this.

MORBID ANATOMY OF THE LUNGS.

I have now to speak of the morbid anatomy of the lungs: This is one of the organs in which, when diseased action is produced, the disease is tubercular, as in the liver and spleen. I know the matter may not be aggregated, it may be diffused, it may make a sort of solid substance, but it is tubercular. This is what we call a consumption—tubercles in the lungs. Now these tubercles are of various kinds, and various sizes; they are of all sizes and diversities of texture, according to the constitution. Then we may question whether these tubercles are organized or not? We know that they suppurate and break into the lungs. You have them larger or smaller. Sometimes a great deal of matter is spit up after the tubercles break; and this is the state of the lungs in those who die consumptive. Certain it is, you do see abscesses in lungs not tubercular.

This morbid anatomy, which people dread so much, does not appear to me to be of primary importance in the study of our profession, there is such a great diversity of appearances; but organs of certain construction are liable to certain diseases, and those are easily recognised; the grand thing to be attended to is, that what produces a state of irritation, and brings on a state of vascular action, will cause those diseases.

Now, with regard to the lungs, what can produce an irritable disease of pulmonary irritation? I am very well convinced, that stomachic irritation will; and I am very well satisfied that this is the primary state of the disease. I know that many gentlemen who have been educated at this hospital, have obtained very considerable credit to themselves, in curing or relieving some consumptive people by putting their stomachs to rights. I cannot tell you all the cases that I have known of this kind, but one I will tell you of, and this was the very first thing communicated to me, which produced those crazy opinions that have predominated in my mind ever since: there was a gentleman who attended these lectures, just about the time I first began to give them; he was a young Highlander, and a more intellectual or honourable man I never met with;

he went to settle about five or six miles out of town, and both he and myself being young, I used to go to see him, and he used occasionally to come to see me. Once when I went to see him, he said, "O, good God, what would I give for your opportunities! When we came first to this country, our books were crowded with consumptive cases, and I am convinced that consumption begins in the liver; I wish you would send me down some consumptive people; I will pay for their board, and attend them for nothing." Now this being a clear headed man, I listened to what he said, and I did send him down some patients; but he could not cure them, though he certainly did relieve them.

I know that, in some cases, there may be diseases in both organs; but then, what does that prove? It does not prove which of the organs is the most diseased. However, that led me to consider how sympathy might affect either organ; and I say it was this circumstance which induced the crazy thoughts to enter into my noddle, that have ever since reigned predominant there. But while I was wandering on this subject, my coachman did not come at his proper time one morning, and I said to him, Why is this? this must not be. He said, O, Sir, I really do not know what I am doing; my wife is in the greatest distress, and dying at home. I said, why did you not tell me of that sooner? I could have got her medicine for nothing, and would have attended her. O, he said, she has been kindly attended by physicians. I said, shall I go and see her? He said, you may. I went, and I found her coughing up about a pint and a half of mucous blood; she was wasted to the utmost degree; she was like a skeleton, with a hectic flush on her face, and her pulse 140 or 150 in a minute, and, in short, dying. But with these notions in my head, I began to ask her some questions about her bowels, and she said, O, Sir, that is the greatest annoyance I have. I know I must die, and that thought does not affect me; but I have continual purging of such offensive black stuff, that every person is affected by it, and if that was stopped, I should care nothing about dying. I said, what was the former state of your bowels? O, they were costive. And many people have congratulated themselves on being better, upon their bowels becoming lax, after they have been in a long continued state of confinement, but that is only an aggravation of the complaint. I sent her some pills; I told her not to take too many of them;

I said, for God's sake do not take too many of them. I put a grain of calomel and opium into each, and told her to take one of them in the day; and ere she had taken a dozen of them, she felt much better; she went into the country, and, in the course of a very few weeks, became perfectly well. Now that proved to me that pulmonary irritation is kept up by stomachic irritation.

But there is another function of importance employed in this case—the function of the skin, in throwing off carbon and water from the blood. The skin being engaged in the same functions, as its functions are suppressed, the lungs will have to do a double duty. I would ask, who are those who are most subject to consumption? O, your thin skinned people of delicate texture—those who, by the thinness of their skin, are chilled by slight causes. Where does consumption thrive? Most of all in this climate—in this changeable climate, where the skin is easily affected. What is the relief? O, you must go to Holland, or to some southern climate. Great numbers of people have gone there; they have felt better; they have considered that they were well; they have returned again to this country, but here the disease has come back upon them again, and they have died.

Well, I say it is a grand thing to attend to the functions of the skin; do not let them be oppressed; and anything that would weaken the action of the heart, or arteries, must be attended to.

Head.—But I know the head also acts on the lungs. MORGAGNI has cases of this kind, and I have met with many of them. I have been called on to attend children—to attend people—children especially, where the chest has been blistered, where local irritation has been kept up, and so on, to no purpose, for the affection has been *cerebral*. But I say, we should look to these cases, if we want to cure such cases.

But can consumption be cured? Odd bless me, that is a question which a man who had lived in a dissecting room would laugh at: how many people do you examine who have lungs tubercular, but which are otherwise sound. What is consumption? It is tubercle of the lungs; then, if those tubercles were healed, and the lungs otherwise sound, the patients must get better. But if the inquirer shifts his ground, and says, it was the case I meant of tubercles over the whole of the lungs, why then he shifts his ground for no purpose; for

there is no case which, when it has proceeded to a certain extent, can be cured. Therefore I say consumption may be cured, according to surgical principles, by creating local irritation on the side of the chest, and so on.

Scrofula.—But then there is an opinion, that consumption is scrofula. Why, tubercles are certainly very various; I see, in some cases, that a propensity to scrofula prevails; but to make consumption out a simple scrofula is, in my opinion, giving a very wrong opinion of it. If a man was consumptive, I should expect to find the *bronchial glands* diseased; and I find tubercle in the lungs very frequently without disease of the bronchial glands; and the question is, do you ever see bronchial glands diseased without tubercles? O, yes. In what state are the lungs in those cases? Why, just such a state of the lungs as you will find to be the state of an arm, where there was a thorough disease in the *axilla;* it is loaded with fluid; it is œdematous; that is what it is; and people die of this.

Larynx.—Now this is all I have to say respecting the morbid anatomy of the lungs; and now with regard to the *larynx.* The larynx is liable to acute inflammation, called *croup,* that is *acute laryngitis,* of a specific kind. But then the larynx is liable to *chronic* laryngitis—a thickening of the membrane, and producing divers morbid appearances. Here are several preparations of chronic laryngitis, where people have died of it; many of them. And I know you will find ulceration, and seemingly loose cartilage, and thickening of the membrane, in some of those cases of morbid appearances; but it is to the *disease* that our attention should be called. In these cases of chronic laryngitis, people breathe moderately well, except occasionally, and then they are almost suffocated; but the spasm goes off; the irritability of the muscles subsides; hope is entertained; the attack comes on again, hope leaves them, and they die suddenly. Now, these are the cases which more particularly demand the operation of *tracheotomy* —that particularly demand the operation of tracheotomy, for the larynx is an organ where the disease cannot be expected to cease, while the organ is continuing its functions. I say, if people could breathe through the *trachea,* and give the larynx repose, the disease of the larynx might get well. It has been an operation often performed abroad, but rarely in this country; though of late, it has been several times performed here.

Epiglottis.—The epiglottis is liable to become thick in cases of irritation about the larynx. It seems to be red; it seems altered in form; you see it, by looking into the throat, just rising up above the *dorsum* of the tongue. O, the case is very distressing. I heard of a man who thought that this was a *fungus*, and he was going to snip it off; he was an old practitioner, and residing in the West Indies. But the patient hearing that he was going to perform an operation, chose to consult a *new comer;* and this new comer was a man who had been educated in an anatomical school; when he saw it, he said it was an epiglottis, and that it must not be removed. The operator came, and this young gentleman remonstrated with him. O, well, said he to the patient, if you choose to have it remain, you will see what it will come to at last. The new comer had to remonstrate with him very warmly, and at last the operation was prevented.

I have met with cases where ulceration was formed here, and where ulceration has detached the epiglottis. I have known two cases in which the patients have swallowed their own epiglottis; one was that of a man who was in this hospital: for some time he could hardly speak, and yet just before his death, he spoke aloud, to the astonishment of all the persons in the ward; after he died, the epiglottis was found in the stomach.

Strictures of the Œsophagus.—You have stricture just at the commencement of the œsophagus—just behind the *cricoid cartilage;* this is where you generally meet with it, but you may have it in other parts of the œsophagus. You may have simple stricture, or you may have *carcinomatous* stricture, just as in other places.

Here are specimens of the diseases of the *thyroid gland* too; here is one considerably enlarged; that gland is often enlarged without the natural structure being altered.

Communications you meet with also between the œsophagus and the air tubes, of which I believe I spoke, when speaking of their relative situations.

Now as far as I know of these circumstances, the same thing prevails to induce diseases of the thyroid gland which causes them throughout the glands of the other parts of the body; and the best mode of tranquillizing a *bronchocele,* is to tranquillize the stomach, and I have seen many cases cured in that way.

Blood vessels.—The structure of the blood, and its circulation, was spoken of in the preliminary lectures, and therefore this subject I hold to be unnecessary to repeat; but as to the *morbid anatomy of the blood vessels*, that was not spoken of. And concerning that, I have to say, that I do verily believe the cause of morbid actions in the blood vessels to be, the too forcible pressure of the blood into the heart; and when that is the case, the internal coat of the aorta becomes strained; then there is a deposition between the different parts, and then the blood vessels become ossified. In this state, where the internal coats of the vessels are altered and liable to give way, they may perchance burst, and then the blood may escape out of the tube, under the elastic coat, and thus is an aneurism formed: you have the blood coagulated in a sort of pouch, in concentrated layers; and if the crack is very small, it is possible that it may close up again; but if it is large, O, then the inflammation takes place, the coats are inflamed, the blood is diffused and left in them without being absorbed; but still the blood does not escape. There is a preparation here of a very large case of this sort: I remember the woman in the hospital who had it, and she was told by one of Job's comforters, that when it burst she would die; it did burst; it bled a good deal, but the bleeding stopped, and when it did stop, the old woman thought that the doctors were mistaken, and that she would not die; but undoubtedly the doctors proved to be right, for eventually she did die. This is so often the way in which aneurisms are formed, that SCARPA says it is *always* the way in which they are formed; and that shows the impropriety of laying down rules without an exception. He says arteries will not dilate, but here are instances in which they have dilated. Now, if we examine these cases, we find that the valves are diseased, and that there is deposition of glutinous matter in them; but there is nothing very peculiar in them, and therefore I need not take up your time in talking further about them. You see these specimens are in the museum, and you may look over them at your leisure.

LECTURE XXXVII.

ON THE ABSORBENT VESSELS AND GLANDS.

With respect to the *absorbent vessels*, an account of them was given in the introductory lectures, which renders much more to be said about them, at this time, unnecessary. In *structure*, they are exceedingly like veins, as was then told you, they are exceedingly minute, and it is very difficult to fill them with injection; like veins, they communicate with one another very frequently; they anastomose; and there is a good in this; for, by it, the different kinds of matter or fluid which they imbibe, become all commixed together.

Glands.—But what is the structure into which these absorbents run, and from which they again shoot out? Why anatomists are in doubt upon this subject; some say they are *cellular*—some say they are merely contorted vessels; but this fact does not seem to me to be of the slightest interest. It is doubtful, in different parts of the body, whether, what we may consider a gland, may not be some sort of enlargement of vessels, and it can have no particular effect in relation to *functions*. We know that these glands do produce a *remora* to the ready emptying of the contents of the absorbents; that whatever was in the absorbents, is arrested for a time in the glands, and that the absorbents are capable of again going on with it. Now the glands are very vascular; if you inject them, you make them exceedingly red; and after a gland has been injected, I have found in it a mixture of red matter and quicksilver; therefore the belief is, that no secretion takes place from the *aorta;* that the contents of these glands are commixed with the blood from the absorbent vessels, and that there is a modification produced in these glands, by new animal juices added to them. We do not find them everywhere; we find them intervening between the vessels of the lower and upper extremities, and the trunk; such as in the *groin*, in the *axilla*, in the *neck*, and so on; and it is believed that they would stop any thing from going forward, that would be noxious if conveyed into the blood, and that they stay irritation of the

sanguiferous parts. There is an opinion of Mr. HUNTER's, that vessels do modify their contents, that the life of the vessels acts upon their contents, and that altered actions of blood vessels, produce altered states of the blood. Now it is clear that vessels have this property. *Digestion*, I may say, is performed in the absorbent vessels of many animals. What they imbibe is imparted into the nutritive fluid of the animals. It is so in *vegetables*; the roots of vegetables absorb fluid, which becomes converted into sap in the trunk. And there has been abundant proof of these vessels doing what we may fairly ascribe to the office of vessels in general, that they modify their contents; for, of late, great attention has been paid to the contents of the absorbents. The *chyle* seems to be made more and more to approximate to the relation of blood in proportion as you find it nearer and nearer to the *thoracic duct*; and if the contents of the thoracic duct be examined, they are as near to blood as possible; they separate spontaneously into *crassamentum* and *serum*. What is it that replenishes the absorbents, but the serum they drink up—the *lymph?* They were called *lymphatic vessels*; however, what you have in the thoracic duct, which is a vehicle for all, is not serum—it is a fluid very much approximating to the nature of blood.

I should have said that the vessels are valvular; that whatever gets into them, must go on, and that it cannot get back, from the number of their valves. *Pressure* will urge on the contents of the absorbents, as it does with respect to the veins; and *friction*, which is but pressure, will do the same thing. Then it is believed that these glands do, in a particular degree, modify the contents of the vessels, and this is supposed to be the use of them.

Absorbent vessels.—Now then, I go to give an account of the absorbent vessels: it is exceedingly difficult to inject them well, and if you have preparations of them, by moving them about, the vessels get cracked, and the contents get out of them. At the school in Windmill Street, where there was a great deal of pains bestowed on them, a chart of this kind, which I now present to your view, was published by Mr. CRUIKSHANK. No anatomists can pretend to say, they really can inject all the absorbent vessels, which exist in multitudes, in every part of the body—they are so minute. A Mr. FYFE, of Edinburgh, did inject the body of a malefactor; I mean the absorbents, and here is the view. Well, it certainly was a

shade of talent, but what idea does it give you of these multitudes of vessels?

Now, MORGAGNI hit upon a new mode of injecting absorbents, for till his time they were injected with steel pipes, which were apt to rust: he hit upon a new mode; he got tubes of *glass*, like barometer tubes, but made with very fusible glass—glass in which there was a good deal of *alkaline* matter; and therefore when he put it into the heat of a candle it would fuse, and when he heated it to a considerable degree, he drew it out to a very fine point, to the size of a hair, and then snapped it. Now this was a tube that never would become rusty, and he did inject the absorbents with it to a wonderful degree—to a degree to which it never could have been done unless this mode had been thought of. If the point of the tube broke, he had only to heat the glass again, then draw it out, and point it. He had the opportunity too of living in the pure air of Italy; and there are preparations thrown about there, considered as useless, which would be put up in bottles here, and preserved as very grand specimens of anatomical art. MORGAGNI even wished to find out the mouths of these vessels, and he absolutely put ink and fluids of different colours into the cavities of animals recently dead—into the abdominal cavity; for it does appear that absorption is a process going on longer than circulation: people who are about to die, and have *œdematous* limbs—in them, as the circulation falls off, the deposition diminishes, but the absorptions continue at work, and the œdema disappears. And MORGAGNI has drawn what he imagines to be the mouths of the absorbent vessels, but I have nothing to say of them; you may fancy that they are the mouths of them, or you may fancy that they are not—it is of no consequence. If there was any part of the body, where we had any hope of knowing any thing about the absorbent vessels, I should think it must be in the *villi* of the *intestines*, and yet we have no distinct knowledge of them there. I have told you what I think of the villi there; it is evidently vascular, and the chyle coagulated in it, distends it.

Well, then, MORGAGNI having, I say, laboured, has given us charts of the absorbents that he has been enabled to inject; and he has injected them, in the lower extremities, even from the toes, all the way up to the groin; but those we are to consider as trunks of the superficial absorbents. He has injected them on the outside, but he does not find that they go all the

way up. That there are absorbents every where, he demonstrates; but those vessels we are to consider as the *trunks*, and those trunks do not go over the superficial part of the thigh, but pass over so as to join the trunks of the absorbents on the inside of the thigh. You have likewise trunks of absorbents in company with the arteries—always in company with the blood vessels; he has injected them running in company with the different arteries. He has injected subcutaneous absorbents every where.

Now, then, the superficial absorbents of the thigh come to the groin, under a number of glands that are situated about POUPART's ligament; they are generally five in number, and are arranged, as it were, into two rows, one row above the ligament and one row below it. These are the glands in the groin which are so often diseased; there is an *inguinal phalanx*, as they call it, and a *femoral phalanx*, and this is said to deserve the attention of the surgeon, inasmuch as it may denote the cause of disease. If, for instance, you have one of the glands of the inguinal phalanx diseased, you may suppose it arises from irritation in the genitals, in the testicles, and so on; but if it be in the *lower* row, it may arise from *ulcers* in the leg. Often where there is tumefaction in the leg do you find disease in this part. Often when my opinion has been asked about a swelling in the groin of a lad, suspected to be a bubo, I have found it to be in the lower row, and then I have said immediately, no, it is not a *bubo*. Upon an examination I have found that the lad had some irritation, or sore about his heel, which had communicated irritation to these glands.

Here, again, are absorbents of the legs, which are traced on to absorbent glands in the *ham*. There are three or four absorbent glands in the ham which nobody ever thought of till MORGAGNI pointed them out; and I sometimes say, I would lay a wager that a gentleman may dissect a ham, pick away all the fat, and not find these glands. They are very small, but MORGAGNI has injected them. Now without injection, without anatomical artifice, any body might be convinced that they are there, for they often become swollen and suppurate. I have seen many cases where there has been enlargement of glands in the ham, and subsequent abscess. I do not think that MORGAGNI gives us that enlarged knowledge of the vessels from his injections which we are capable of deriving from disease in the parts.

Here, again, are the deep-seated absorbents; there are several glands as you approach to the groin. The deep seated absorbents of the thigh go under POUPART'S ligament; they form a trunk on the inside of the femoral artery. MORGAGNI has injected them, and shown that they are passing through the pelvis, which a person must be convinced of merely from what we meet with in disease. We find the glands of the groin all diseased, without any impediment to the absorption in the lower extremity in general, therefore there must be other passages by which the absorbed fluid can move on.

Then there is an immense plexus of absorbent glands in company with the *external iliac artery*, and another in company with the internal iliac artery; there are also many absorbent glands about the sacrum. I have seen all these very much diseased in cases of cancerous rectum, and in diseases about the pelvis; I have seen them exhibited far better than MORGAGNI has depicted them. They are exceedingly numerous, and I remember one patient in whom I tied the iliac artery, and who died in consequence of disease in these glands; the artery having been tied, irritation came on, the glands became diseased; one of them suppurated, and it lying on the artery, an aperture was made into the artery, ulceration took place, and the blood oozed out through the aperture into the gland, and it gradually sunk him; there was no great hæmorrhage, but a continual oozing of the blood.

Then MORGAGNI comes to show you the absorbents scattered all over the loins; it is astonishing to see the vast number of them; and, in disease, you find all the lungs covered with enlarged absorbents. You would think it scarcely possible that a man could inject absorbents in this manner, and yet there is no reason to doubt the validity of these charts. Now with regard to the absorbents of the bowels, supposing the glands were meant to modify their contents, we should expect that those would be very numerous where the chyle, or the scarcely animalized fluid, as we may say, is introduced into them, and actually a hundred and twenty, or more, absorbent glands have been counted in the *mesentery;* and the absorbents have gone through three or four tier of glands before they have got out of the mesentery; nay, a single absorbent has been traced through four rows of the glands ere it has got out of the root of the mesentery. Now MORGAGNI has shown the absorbents of the larger bowels, and he represents many

as connected with the colon, but still they go like that net-work which you find in the lungs; and this leads me to state what anatomists have said concerning the *formation* of the *thoracic duct*, that the trunks of the absorbents belonging to the right side of the body coming together, make one principal tube, and that those coming from the left side make another principal tube; that the two tubes converge, and that just by the root of the mesentery they become connected to the great trunk of absorbents which emerge from the mesentery, so that by this conjunction of the three trunks the thoracic duct is formed. Now we often see these vessels large at one end and not at the other, but we are often able to give a reason for this; for instance, in the thoracic duct, it may appear very large in one part, and then become suddenly small, and then large again; this may be owing to an absorbent going off from the vessel laterally and joining it higher up, so that there may be no disproportion between the size of the tubes and the contents. Well, you see our powers of injecting the vessels are so imperfect, that it is supposed nobody can confidently say whether there is a cavern into which the chyle is poured or not; but it is believed that the union of those three vessels makes the commencement of the thoracic duct, and then the thoracic duct goes through the diaphragm, (for it is not yet thoracic duct), it goes through the diaphragm at the aperture out of which the aorta passes—that it goes through the aortic orifice of the diaphragm, and afterwards you find it the thoracic duct.

There are multitudes of absorbents on the convex side of the liver, and so there are on the concave part of it; you make it appear a perfect net-work; those on the concave side descend and join the lumbar absorbents; those on the convex side penetrate the diaphragm, and get into the thorax.

In these charts, they are also injected, passing from the abdomen into the thorax, also in the interstitial cellular substance of the lungs in the bronchial glands; absorbents from the heart; absorbents about the jugular vein; absorbents of the arm, running up from the finger into the arm; absorbents of the jaw, and belonging to the head. I should state that all the absorbents MORGAGNI has shown in the arm, are traceable to the glands of the axilla; and when they become thoroughly diseased from cancer, you have an uncontrollable œdema from the extremity. Really this is a grand cause of suffering, in the

advanced stage of disease, to some individuals; the arm is so swollen, that we are obliged to puncture it, to let the fluid drain out, just as you do in *anasarca*.

No one, that I ever knew of, has attempted to demonstrate absorbents in the skull or brain, till MORGAGNI did, and he has asserted that he has injected them. There are quantities of absorbent glands in the course of the deep seated vessels of the neck, as I have seen in cases of general disease of those glands.

Now, notwithstanding such discovery as has been made by MORGAGNI, still I maintain that it is a very insufficient exhibition of these vessels. In HALLER's time, it was believed that the veins absorbed, because HALLER did not think that the number of vessels then demonstrated, were adequate to account for so great an absorption as was doubtless necessary. Secretion is allowed to be a constant process, and if there was not a proportional absorption, there would be an alteration in the parts, an alteration in the form of the parts. Mr. HUNTER, therefore, used to call the absorbent vessels, on that account, the modelling vessels, taking away what the arteries laid down, and preserving an uniformity. In HALLER's time, absorbents were not discovered in birds or fishes, but, since then, they have been frequently demonstrated. Mr. HUNTER, wishing to ascertain whether the veins did imbibe, was induced to introduce fluids into the intestines of animals, and they were absorbed by the *lacteals*; he introduced fluids coloured with indigo and saffron, and scented with musk; he opened the trunks of the lacteals and got the fluid, so coloured and scented; he opened at the same time the corresponding veins, and got the blood out of them, but it was not coloured with indigo or saffron, or scented with musk; therefore, said he, those are the only absorbing vessels. And believing them to be so, he spoke of what they did, which is, indeed, most surprising, that they should even absorb bones in the way they do; but it is more surprising to suppose that the veins do absorb. I know it is a very fashionable doctrine to maintain that the veins do absorb; but, as I told you in the preliminary lectures, I was not convinced that they did any thing of the kind; and here I repeat an observation of HALLER's, which I hold to be a very judicious one: "Surely," said HALLER, "Nature would not have created such multitudinous vessels, made them travel so long a course, and made them unite with the blood vessels in so curious a manner, if the same object could have been ef-

fected by the veins absorbing, or by those vessels being made to terminate directly in veins." But you see of how much importance it is, that whatever is taken into the vessels should be made, as soon as possible, to acquire the property of blood. Here we see a grand object to be answered by such complicated structures, and the course that these vessels are made to pursue.

White fluids are sometimes found in the blood vessels; that is known; the blood sometimes seems to lose its colour; that is known. White fluids may be seen in the vessels, but this does not show that the absorbents have poured it into them. You know MAGENDIE thought, by his grand experiment, to prove this beyond doubt; but I told you that it was no proof to my mind. MAGENDIE cuts off the limb of an animal, leaving no connexion between the limb and the trunk, but by means of the vessels, the arteries and the veins; he then wounds the foot of the animal, and introduces some deadly poison into it; if he stops the vein, the animal is not affected; but as the blood returns, the animal becomes affected, and dies. He says, he could even modify the effects of the poison, by letting the blood pass more freely or more slowly into the system. Then how is this? If the veins do not imbibe the poison (that is what MAGENDIE asks,) there is no absorbent, no channel by which it can be conveyed into the system, but by the veins, and if the veins do not imbibe the poison, how is it to be accounted for? But I say, to render this experiment satisfactory, it is first necessary to prove how poison operates: and FONTANA, who made a great many experiments with respect to poison, was led to entertain this opinion, that it had an influence on the blood itself, that it did not get into the system, but had an immediate influence on the blood itself. Now this may be; and then the blood being poisoned, as it were, will, of course, when it goes into the circulation, destroy the animal. I say it must be first proved that such poison as I am now speaking of, deadly poison, does not exert an influence in some direct manner upon the blood contained in the vessels, which FONTANA was led to suppose it did, before MAGENDIE's opinions can be upheld.

A great deal might be said about the mode of absorption, but really we know nothing at all about it; therefore it is talking to no purpose, and I am not very willing to do that, so that here I shall close.

LECTURE XXXVIII.

ON THE KIDNEY.

Function.—Now, with respect to the function of the kidney, its function is to secrete the urine; and then the question is, what is the urine? Every body knows that it is a watery liquor. But what is contained in water? Why, divers things; there is a great deal of animal matter—a great deal of it; and urine goes speedily into a state of putrefaction, and the product is, *ammonia*, as you must know. There is a great deal of animal matter in it, but then it is a kind of animal matter that is exceedingly soluble in water: you may evaporate the water of urine—of healthy urine, to a very considerable degree, without precipitating the animal matter; again, you may add water to a very considerable degree to it, and you may have this animal matter diffused throughout the whole of it; it is very soluble, and therefore it has been believed to be a peculiar kind of animal matter which has been distinguished; or which was at first distinguished by the term *urea*. It is supposed that nature has given the kidney the prerogative of combining and modifying all the old animal matter of the body—modifying it so as to convert it into something like matter; and it is this which gives the citron colour and peculiar odour to healthy urine.

I find that chemists of late apply the term urea to something they can crystallize from this matter; but the original use of the word was such as I have endeavoured to explain to you. Now this is exceedingly curious, how the *albumen*, and matter not soluble, is to be converted into that which is soluble; and it is very likely that nature has given the kidney this power of converting insoluble matter into one that is soluble.

But other difficulties present themselves to our notice: how is all the old earth and lime to get out of the circulation? How is that animal matter in the blood, which we take in, to get out of the blood? This is done in a curious manner too: nature has given to the kidney the power of making *phosphoric acid*; and the kidney has the power of forming

much more phosphoric acid than is necessary to dissolve all the lime that is secreted for a considerable time. If you drop oxalic acid into urine, you will precipitate lime in abundance from it, provided it is healthy. Well, if you get over this, you will not wonder how salts get into the urine, how salts are to be found in the kidney: we are continually taking in sea-salts with our food; we are, too, taking salts occasionally of another kind, as you all know; and if people take nitre, they will find it almost immediately in their urine. These things are met with in the urine, but really it is very curious how things sometimes are suspended in urine, which are readily precipitable; I allude to the morbid states of urine, where a great deal of albumen, or, as Dr. Brown would say, chyle is suspended in the urine. I say these are things which my chemical knowledge, at least, does not enable me to comprehend.

I have always said in these lectures, however, ever since I knew this fact, and I have observed the same fact in other cases, that many, many years ago I knew a female who voided urine of such a description, that if she had discharged it into a *blanc-mange* dish, and if she had allowed it to cool, and turned it up when solid, she might have served it up at the table. I gave the woman a lecture about her digestive organs; that was all I could say to her; in about a dozen years afterwards I saw her, she was then a fattish woman, and she said she then still occasionally voided the same sort of urine, but that she had got considerably better by the means I had suggested to her. There has been a case here lately, in the hospital, of this kind; but I am not now to speak of these things, it is the healthy function, it is the physiology that I have now to dwell upon, and the kidney is the great emunctory of the healthy circulation. What is it that is thrown off from the blood? *Saline juice.* What from the skin? *Carbon* and *water.* But how is the other improper stuff, that gets into the system, to get out of the different parts? O, the kidney is the organ—the kidney is the organ by which this is effected, in the healthy state, in a very wonderful manner.

Now, I take this opportunity, with regard to the secerning organs, to say what ought to be stated with regard to secretion, for you can observe the secretion going on in the kidney; and the first thing that is to be said is, that you perceive that the secretions are vicarious to one another: a man may be drinking diluent liquor in a warm room for a very considerable length

of time, and he may not want to make water during the whole of the evening; while he is there, and so engaged, his skin is perspiring, and so on, but he goes out of that room into the cold, and, on his way home, he has to turn to the wall every five minutes. There is a view which I have always laboured to make people attend to, and which is this—why does the blood circulate? Is it not that secretion should take place? Why the very building up of the body is secretion! Well, then, by secretion there will be a continual depletion of the blood itself; and how are the secretions to be supplied? By absorption. There is a sort of correspondence in these things, by which, in all probability, the quantity of blood is not only maintained, but the quality also, for if the blood is deprived of water, the absorption of water from any other part where it may be obtained, will be more ample, and vice versa. I spoke of this in the preliminary lectures, and I need not now tire you by repeating it. I wish, however, that you would consider this point, how important a thing it is that there should be a true balance between the quantity of circulating fluids and the powers that circulate those fluids. If perspiration were to be suppressed, why the blood would be redundant with water, but then in that case the kidney takes up the secretion. The kidney prepares phosphoric acid; and I suppose I need not try to convince any gentleman present that there is no phosphoric acid in the blood; if there was it would coagulate the blood; there is soda in it. What is there then to be said of this secretion? The albumen must be admitted to be there; it must be admitted there is phosphoric acid in the kidney; and what are we to say with respect to that? Why, that secretion is a new formation—that the phosphoric acid is a new production; and then the question is, how is this effected? This is the grand problem proposed a long time back, and perhaps not answered, you will say, at present—How, from one kind of blood, can such dissimilar substances be got? And the old anatomists were looking to the arrangement of the vessels, to the tortuous course of the arteries and their branches, to find this out; but I defy any man to conjecture what use can result from muscular formation, save that of filtration: it would spread that which was in it, but it could never make a new compound. Now Haller himself seemed to conceive that the result of these things was very much the result of vital feeling, for he has quoted the case of a hysterical female secreting nothing

but colourless urine, watery urine, but he has let this nervous fit be suspended, and the kidney has again performed its functions, and proper urine has been secreted ; more loaded too with animal substance and salts, on account of the temporary suspension during the hysterical fit, therefore vital action has a great deal to do with it. But it was JOHN HUNTER who proved that vital action was the cause of all this : it was proved with respect to what we know takes place with regard to poison ; a specific incision is made, a specific application of poison is made, and a specific secretion takes place. But if you were to say, how are these things prepared ? I think no man in his senses can deny, but that it is by electrical operation ; that electricity is the great agent of nature for the purpose of composition and decomposition of all surrounding substances, and we cannot doubt that the same agent inheres in animal bodies. A man who doubts of this, certainly could never have rubbed a black little kitten in the dark, or pulled off a black silk stocking in the dark ; there is no doubt of electricity being thus employed. But I say it would be absurd to doubt that this was the agent for producing composition and decomposition in animal bodies after what Sir HUMPHREY DAVY has told us. I cannot express my opinion on this subject now in any different way to what I am accustomed to do. If any man had told me, in the early period of my life, that I should live to see chemical action explained, I should have said, well, I shall certainly not die at a less age than that of METHUSALEM. But it has been done, and by Sir HUMPHREY DAVY; he showed that it depended on the matter it possessed. I was present at his lectures, when I saw performed what formed the ground of the papers that were given by him to the Royal Society, and which I had read, but the reading of them certainly did not produce that impression on my mind which the ocular demonstration of his performance did. I saw the thing done, and therefore I had conviction brought home to my mind. When I saw him make some potash pass through sulphuric acid, and come out potash on the other side of the dish, I said, it is wonderful ! it is wonderful indeed ! why, who would have thought it ? Who could have thought that a person could have mixed potash with sulphuric acid, and that it should have remained the same ? Why, it is counteracting nature by using her own agents. Who could have thought it ? Put potash into sulphuric acid, and what is the result ? Why, that the most violent actions take

place till they combine together, and then they are very sociable. On seeing this, I could not but think, that if I had been lecturing to the class Sir HUMPHRY DAVY was then lecturing to, which was principally composed of ladies, I should have said, now ladies, this is as wonderful as if I were to place a barrel of gunpowder before you, and, presenting you with some hot embers, you were to throw them into the powder, and they were to come through it without igniting it. Then I do not doubt but it is electrical, but electrical under the influence of the nervous system, and all the functions of the body are governed by that system.

Morbid Action.—Well, there is a morbid state of the kidney, which I think throws light upon the subject of secretion, and that is diabetes, where something like sugar abounds in the urine; they say it is positively sugar; you cannot crystallize it. I know, a long time ago, when I was a boy, I could not do it, but I thought that that was because I was not then *up* to the *sugar-making* business; but I do know this, that I have scraped as much crystallized sugar from a glazed vessel with a flaw in it, put under a patient's bed, where the urine transuded through it, I have scraped as much off as I could have sold to any one, in the morning, for half a pound of good Lisbon sugar! Positively I did. I wanted, as Dr. ROLLO did, to see whether, if I gave those people sugar, it would increase the quantity of sugar to be found in their urine, and it did. Then, having given them sugar, and kept them on vegetable food, I wanted to give them animal food, and to keep them on that, merely to try whether that would have the same effect, but they would not do it; they said I was trying experiments, and they would not submit to it. Egad, I half ruined myself in buying them oranges, and all the nice sweet things I could get hold of, but as soon as I had done that they turned their backs to me. But Dr. ROLLO met with a patient, a gentleman he was, and he found this, that there was no sugar in his urine when he did not live upon food containing sugar; and one day, after the Doctor had been directing this sort of diet, finding a little sugar in his urine, he said, "Sir, you have been taking some vegetables, or something of that sort;" and the patient, being a gentleman, said, "I have, indeed; I acknowledge that I ate half a savoy biscuit yesterday;" the other half of which was then upon the table. Now, please to mind, that I am convinced, that this is one grand cause of diabetes. They say,

feed your patients as you like, there will still be sugar in the urine. I do not mean to say a morbid state of the kidney will not give it a sugary nature, but I am very much convinced in my own mind, that Dr. Rollo's theory is a very good one, and that it affords an ample explanation of the cause of diabetes, in the greatest number of cases; I will not say of them all.

Morbid Anatomy.—Having now done with the physiology of the kidney, I next come to speak of the morbid anatomy of the kidney, and here you have enlargement of the membranous part of it. In that membranous part there is every thing to be found that you have in the bladder, and therefore stones are as likely to form in the kidney as in the bladder; indeed it is reasonable to believe that many stones do pass through the ureter, and I shall call your attention to this point, that when a stone has passed through the ureter, you ought to tell the patient how he should endeavour to get rid of it. In the preparation I now show you, there is a stone completely obstructing the ureter, and just see how all the membranes are enlarged, how they are inflamed from adhesion, how rough the surfaces are, and how the glandular structure of the kidney is diminished in proportion; for if any thing prevents the function of an organ, so that it can be no longer useful in the animal economy, it is absorbed, and that is a very curious thing. Here is the case of an impervious ureter, and what has become of the kidney? The kidney is absorbed.

Then you have *abscesses* in the kidney; you have *hydatids* formed in the kidney, and various appearances of diseases, which are not in any degree remarkable.

With regard to abscesses in the kidney, they burst and discharge into the ureter; and here I should just like to tell you a case of that kind. A man whom I knew, not as a patient merely, but as a friend also, had diseased kidneys, and well he might, for he kept his digestive organs in a perpetual state of irritation. I am convinced that it is disorder of the digestive organs which generally produces disorder of the kidney; I am perfectly convinced of it. I should think there could be no reasonable ground to object to this proposition, for the kidney is supplied by the same ganglions, has the same nerves that supply the alimentary organs, and therefore it must sympathize with them. But, however, this man had had several times suppression of urine, and one morning he sent for me, saying,

"For God's sake come, do come, and bring with you a catheter; my bladder is bursting, and I am in the most horrible torture." I went to him; I felt above his pubes, and I said, "there is no urine there; no, you are entirely wrong;" and he said, "I am sure there is, and I must have a catheter introduced." I said, "I do not think there is; but, however, I will pass a bougie;" and I pushed a bougie into his bladder, and gave him a chamber pot that he might try to make water. Now he voided a pint of *pus*, as good pus as ever I should wish to see; and for some time there was pus voided by him, mixed with his urine, and it went off. Also, I have known considerable *hæmorrhage* from the kidney, but this is not surprising.

Now I do not see any thing remarkable in the morbid appearances; it is that knowledge which looks to function, and the sympathies of organs with one another, that I consider the most important parts of the kidneys; this is what I have endeavoured, as well as I could, to explain to you, and I have done.

ON THE FEMALE ORGANS OF GENERATION.

Having gone through those functions which are necessary for the support of life, I proceed, in the last place, to speak of that by which the continuance of our species is effected; and I have, to-day, to describe the *female organs of generation*, which are, indeed, very simple; you know a great deal about them already, having seen them in their natural situation.

Os Internum Uteri.—It is scarcely possible you can fail to ascertain disease in the os internum uteri. Some people dwell a great deal upon this; because it happens, sometimes, that tumours displace the os internum uteri, put it aside; so that, if you find it where you do not naturally expect to find it, you may take it for some scirrhous disease. Now, I really think, there is little difficulty in medical men being able to find the os internum uteri, wherever it may be placed. It is not always *oblong*, it is sometimes contracted in the middle; but as the midwife teacher tells you all this, I do not think it necessary to dwell upon it.

Fallopian Trumpets.—The fallopian trumpets are exceedingly vascular, and we must allow them to have a state of vital action calculated for the function they have to perform. And this seems to be the most rational idea you can entertain of vital

contractility throughout the body; that every part of it possesses that action, but in different degrees; and that its actions are suited to the part which it inhabits. Voluntary muscles act in a peculiar way, as they ought to do under the influence of the will; and involuntary muscles act in another way, suited to the part to which they belong. No doubt the action of the muscles, in this part, must be different from the common muscular action of the body; but, I think, a man must have a very strange mind, indeed, who will deny that blood vessels have a vital contractility to answer their own purpose.

Ovaries.—Then what have I to say of the ovaries? They are *oval* bodies; and they say, about the size of a pigeon's egg; and when you cut them open, a kind of fleshy substance is exhibited, but there is nothing peculiar in it. Here are some cut open to show it; but really nothing can be made out of these parts, in the unimpregnated state of the womb, which I am now speaking of. The appearances of the ovaries of the human species are exceedingly variable: you sometimes see *cells* in them, and sometimes you do not; in animals they are more uniform: in them, they seem to be *fleshy* substances; and they say, cells may be seen in them with magnifying glasses. They are supplied by the spermatic arteries, and the blood returns by the veins. That is all I have to say of them.

Physiology.—Having described the structure of the female organs of generation, which seem to be as simple as can be, I now go to the physiology; and the first thing to be announced to you, and it need not be told, is, that the genitals of both male and female remain in a dwarfish state, until the animal has attained nearly its full growth; that they then enlarge very considerably; that hairs grow about the pubes. From the follicles, leading off from the fundus uteri, a secretion takes place at this time in the female, which, when it is first poured forth, appears to be mere serum; but afterwards it becomes of the colour of blood, and is of as deep a red colour; and as the discharge diminishes, in the advanced period of life, it becomes serous again, and ceases. This discharge, at first, is irregular, but it soon acquires a very perfect degree of regularity, occurring at the interval of a lunar revolution, which has given the name *menstruation* to it, and which has led some foolish people to surmise that it must be influenced by the *moon*; but a more ridiculous opinion never was broached; for if menstruation depended on that planet, it would be as regular as the *tides*,

which are under the influence of the moon. Now, contrariwise, we know, that in every day of the eight and twenty, some woman is menstruating, and that there is no regularity in that respect. Then, what is to be said concerning this discharge? Why, I believe, I may venture to say, that it is almost peculiar to the human race. It has been said, that Nature, ever provident, knowing that females must at times support an infant, has prepared a surplus of nutriment, which, when it is not wanted, is periodically discharged. Now this appears to be nonsense; because the human female is *moniferous*—at least, she seldom produces more than *two* at a time; whereas, a *sow* will produce a *dozen*, and yet she does not menstruate; so that Nature would seem to have acted in contradiction to herself. The only rational physiology of it is, as appears to me, that it is a means of relieving uterine irritation, and preventing that strong degree of desire which would render the party *indecent*. You know how extremely urgent this desire is in the *brute* creation; there is no relief; and, of course, if a woman had no relief from this desire, she would go seeking the male as a *goat* does. This, then, is a discharge which relieves uterine irritation—a discharge which is capable of reducing the vigour of the body. O, people have been examining this menstrual discharge, and declaring that it is *not blood*; and that it does not contain this, that, and the other thing: however, as far as I know, it does contain coagulable matter; and I believe the red colour to be the colour of blood. It is notorious that women are weakened by it; there is that state induced, which tends to take away from venereal desire; there is a degree of weakness induced, and thus is the extreme of sexual desire mitigated in the female. I could produce many reasons for thinking in this manner: those females who have been educated without much attention to decency, generally menstruate early; and others who have been educated more decently, and in a more refined manner, do not menstruate so early. That uterine irritation, however, will bring it on, is proved, because females soon marry after it does come on; and, in the course of time, it will cease again. Now, it has always pleased me much, when I was able to show any thing relating to the human bodies, that seemed to have a reference to those social compacts which are established between us—any reference to morals. Animals, of course, have lawful impediments to premature copulation—lawful bounds; but there is no animal has

such a bound, that I know of, as the human, the human being; and it seems to be part of the same ordinance, if I may call it so, of Nature; it is something that seems to relate to the moral obligations which belong to the human race. Now having told you what takes place at *puberty*, I need not go on to tell you, that when women are no longer able to have children, this discharge ceases.

Conception—I next have to take notice of the changes which take place at the period of conception: but here I have always been accustomed to state what has been said relative to *generation* a little historically, because it is a subject that does excite the curiosity of scientific men; and there are some subjects relative to general science, that would be well if medical men would study, merely from *policy:* as, for instance, if a man of science found a medical man ignorant of the eye, ignorant of its operation as a *camera obscura*—wholly ignorant of optics, why, he would despise him, and the medical man would get into great disrepute. If a lady found him ignorant of chemistry, that lady would never take any medicine of his compounding again; and ladies are critics on that point.

Well, then, I say, it is good to know something relative to the multiplication of the species. And I need not tell you, that the ancients had no distinct notions on the subject: they must have had some; they thought a seminal fluid was mixed by both sexes, and that by some process a young *turk* was produced. But HARVEY, the discoverer of the circulation, was the very first man who led the way to those speculations which have illustrated the process of the continuance of the species. HARVEY was, indeed, excessively puzzled by what he perceived: and it is curious that HARVEY wrote a book upon the generation of *insects*, and so on, and in which he had noted down all the facts he had collected on the subject, and that that book was destroyed. But HARVEY was exceedingly puzzled about the generation of *oviparous* animals: he knew that a *cock*, by one *strut* on a hen, would impregnate, perhaps, a dozen of eggs; and he knew that the *oviducts* of a hen lay high up in the loins, but how the seminal fluid could penetrate these ova in the hen, he was at a loss to know. HARVEY could not discover the penis of a cock; and he thought that what was projected, if any thing, seemed to be projected upon the common *cloaca* of the hen—that part which exudes the fæces, eggs and all; besides, the whole was such a *touch-and-*

go sort of business, that he could not satisfactorily account for the process. Now late experiments have shown what is the real fact, that in oviparous animals the semen is projected into a cell, that it is lodged there, and that as it descends into the duct, each ovum gets a touch of the seminal fluid, so that each egg is impregnated as long as there is any semen in the little reservoir. So that that mystery is now, in a certain degree, explained. BUFFON has a theory of generation, which is a very strange one. I should say, however, that LEUWENHOECK, when he first began to use the microscope, found abundance of *animalculi* in the semen; he found that there were more of these animalculi in a single cod fish than there were inhabitants on the globe. Now such a man would seem to be a man with *pudding* for his brains! A strange diversity of intellect he must indeed have. He thought these things were not all like the animals that contained them; but he supposed that one of them, more like the animal than the rest, might get lodged in the ovaries, and become more large than the rest. He said it was so in every thing, and that thus *seeds* were produced. But he seemed to know very well, that they did not resemble the animals; that they were something like *tadpoles*; and, moreover, that they did themselves propagate in the seminal fluid, so as to become multiplied. Now, I pause here, that you may think of this point, the absurdity, the positive absurdity of supposing that the *fœtus* can be formed by either parent. How can it be? What is the *testicle*; for that, undoubtedly, secretes the fecundating liquor? Is it not a congregation of vessels? Why, then, it must be formed by aggregation of parts after the secreting process has taken place. And what is the ovary but a vascular part? I say it is impossible, therefore, to suppose that a young animal can be secreted; it *must be formed*; and then the only question is, where is it formed, and how is it formed? That is a question which I shall have to discuss afterwards.

LEUWENHOECK met with these animalculæ; and BUFFON met with others. In all liquors that are exposed, you have animalculæ. There is really an invisible world of vegetable animalculæ, the nature of which is as well understood by those who are in the habit of seeing them through the microscope, as we understand the nature of things which we see distinctly with the naked eye. There are indeed a great number of them. BUFFON invented a theory respecting them, which some

praised, but which I cannot praise; I take it to be all nonsense, and therefore I cannot speak about it. His theory was, that there were things having power to conduct themselves so as to build up a body. Now, just after that, DE GRAAF was the proposer of that theory which is now generally admitted; every one knows, there are some animals that are oviparous; but DE GRAAF asserted, that the viviparous animals are in fact oviparous—that every female has the power of preparing an *ovum*, containing parts, which, under certain circumstances, will be led into a state of action which will produce young. Every one knows that an oviparous animal will bring forth her ovum, without any connexion with the opposite sex: shut up a young hen, and she will begin to lay eggs, though she never saw a cock in her life. So, in fishes: the male fish impregnates the spawn, without any connexion with the female. DE GRAAF says that it has taken place in the female, in the ovary; that it escapes from it through the fallopian trumpet into the uterus, and lives and grows there. But whoever saw this? DE GRAAF affirms that he has seen it. Now every one can see that there is good ground for DE GRAAF's theory to a certain extent, for there are little cells formed in the ovary, which gradually approach to the ovula, and as they grow, they burst: this, he affirms, he has seen, and therefore they have ever since been called *ovula Graafiana*. When these cells break they are filled up with another growth, which are called *corpora lutea*. When these cells are burst open, an inflammatory process takes place. Now, in the human subject we cannot make much of this. If in any animal, in a *virgin rabbit*, for instance, after she had taken the buck, you found four or five young ones, you would find four or five *corpora lutea*. But as to the ovula Graafiana, who can affirm that they have seen it? I really do not know that any one ever did, till Mr. CRUIKSHANK affirmed that he did in its passage through the fallopian trumpet. And he said he found it in this way, by slitting open the fallopian trumpet, and washing it with distilled vinegar. He says, that he concluded this was it. Now, I doubt all these things; I doubt whether he can see the works of nature, so as to speak of them positively. I am sure, in this state, it must be the merest atom imaginable, for JOHN HUNTER himself took home an uterus three weeks or a month after copulation, and though he slit it open and examined it, he could not find any ovula Graafiana. The history of that case

wast hat a young woman, supposing herself pregnant, had poisoned herself; that he was employed to examine the body respecting its condition with regard to the poison, and that he took home the uterus, but he could not find the ovula Graafiana. And we have some testimony from HARVEY, to support the statement that you cannot see it for some time; so that I doubt whether any body ever saw the ovula Graafiana. But that all this happens, you may be as certain of as if you saw it; if you cannot see it with the eye of sense, you may see it with the eye of intellect. Nay, nay, you see it with the eye of sense, for sometimes the ovum grows in the ovarium; sometimes it escapes into the cavity of the abdomen, and sometimes it grows to such a size in the trumpet, that it cannot get through it, and then you have a fallopian fœtus. Well, I say all this is proved, and you cannot doubt it.

Well, then, DE GRAAF was the proposer of that theory, of that theory of generation which is now universally admitted.

Now, SPALLANZANI wrote on the subject, but I do not see that he has thrown any light on it. He has told a number of things, ridiculous enough. You know that when a frog spawns, it is seen swimming about with the male on its back; and it is curious enough that at this time the male has some sort of things that grow upon its hands, by which it attaches itself firmly to the female, and she is constantly obliged to swim about with him on her back, and as she gives off the ovaries, he besprinkles them with the spermatic fluid. SPALLANZANI put *breeches* on the male frog, and the spawn was not impregnated. SPALLANZANI speaks of little *newts*, something like little *lizards*, with their heads placed together and their tails apart; so that they are like a pair of compasses: now the female newt passes tapes of eggs, and the male impregnates them; the male puts the fluid into the water, and the newts are formed from it; but SPALLANZANI gets the fluid in the neat state, touches the eggs with it, and they are not impregnated, but he puts it into a bucket of water, and they are formed. I say, therefore, you must admit that the fluid has a specific stimulus, that it has a specific excitability. That is clear, because the fecundating fluid of one animal will not fecundate in another genus of animals. Animals of a different species do fecundate, as the *horse* and *ass*, but it is curious that there is even a provision against the mixture of the animals: the mules are all formed with imperfect

genitals, and cannot continue their species; as if Nature had said, thus far shall ye go, and no further. If the seminal fluid of one animal were capable of fecundating in another, there would be a great confusion of animals, there would be no distinct genera. I say you must admit that this fluid has a specific excitability.

Well, now, that is all I have to say about it. I do not condescend to mention some beastly experiments of SPALLANZANI. Dr. JOHNSON said of a certain nobleman, "he may be a wit among lords, but he is no more than a lord among wits;" so I take it, that this happy SPALLANZANI might be a philosopher among the ignorant, but he was not a philosopher among philosophers; he was not a philosopher among anatomists. He has told some curious facts, but I cannot think highly of SPALLANZANI, who nevertheless is praised for his experiments of production, reproduction, and I do not know what all.

Well, now I have given you this account historically, but we come to this after all—in what degree of perfection is this ovum, which the female has the power to prepare—to what degree of perfection can it be brought, without this stimulus imparted by the male? Now, I should be ready to believe that it might be brought to various degrees of perfection. I am more inclined to suppose that, from what I see in plants; but still I know nothing about it. I do not know how it is to be tried. When you take away the male organs, even in seeds, I can never see any thing of the *plumula*; at the same time I know nothing about it. But how are we to know any thing of it with regard to animals? Why, we must look, and observe; and then what do you see; for instance, in the eggs of frogs? I am sure I do not know; there is something black in the middle of them, and that is all I can say about them. Then, when you get eggs upon a large scale, *crustaceous* ova—the eggs of geese, and such birds as those, one would think if ever there was an opportunity of examining any thing, with respect to this subject, you would find it in them, but that opportunity has not yet been sufficiently afforded; and therefore Mr. HUNTER, who bestowed great pains and labour in making out what constituted the embryo in the eggs, it is curious enough to know, after having put up two preparations, after having had the best drawings by the best artists, after satisfying every body as to what he had done, it is curious enough to know, that that man was still, at the time of his death, working at the subject.

I believe, as this subject will occupy three lectures, for I cannot get through it in two, I shall stop here to-day, and go on with the subject at our next meeting.

LECTURE XXXIX.

ON THE FEMALE ORGANS OF GENERATION.

(Continued.)

We left off yesterday, by saying, that those birds laying the largest eggs, afforded us the best opportunity of examining the formation of the embryo. Now what do you see in the goose's egg? Why, it is a white speck you behold in it; it is a membrane; it is called the *cicatricula;* the vulgar people call it *the tread of the cock;* but then it is there, though the cock has never trod the hen. I kept pullets in a garret all the winter, and in the spring of the year following, they began to lay eggs; I gave the eggs to Mr. Clift, to see if he could find any difference in the cicatricula, but he said he could not; it is a small speck of a whitish colour. Then, upon incubation, this speck enlarges; and it is curious to observe how it is contrived to expose this speck to the warmth of incubation. There are spiral cords to go from either end of the egg, to connect the yolk to it; and put the egg down in any way you please, the cicatricula will be always uppermost, and of course always nearest to the breast of the hen.

Well, this speck enlarges, that is the first thing to be observed; then, another circumstance is, that a *zone* of dots appears round the circumference of the cicatricula, a zone of bloody points, as Mr. Hunter said. He had a curious sort of whim in his head, for he thought the blood was the first thing that was made, and the vessels afterwards. Now these points enlarge, and eventually seem to connect themselves with a point in the middle of the cicatricula, which beats, and which is called the *vesicula pulsans;* the circular vessels turn out to be those sort of things which serve the embryo as the lungs, and the *vesicula pulsans* eventually turns out to be the heart. What Mr. Hunter says goes to prove this, that various things

are formed in various parts of the cicatricula at the same time; that there is no primary fibre from which all the rest proceed; that this is a part endowed with life, and that all parts are formed simultaneously. There are some who think there must be some elementary and primary fibre, and some get that sort of notion into their heads, that they cannot believe there is any thing like formation—that it must be development—that it must be evolution. Now, I say we have no sort of thing like this; I say the power of a female in forming an ovum is clear, but that, however decidedly it may be formed, the actions will not go on, nor will a young plant or animal be produced, unless this part is stimulated by the semen of the male; if this be done, actions go on, and a young one is formed, resembling both its parents, and that is a most marvellous circumstance; every thing relating to this subject is marvellous.

Well, then, as the process of incubation goes on, you find the vesicula pulsans seems to be contained in a kind of circular or oval surface; this eventually turns out to be the body of the chick. There is another little sort of spiral part, which turns out to be the head, as it proceeds, of the chick; and the wings and limbs seem to grow from it. It is altogether not like evolution—it is like formation.

Well, I have done with that subject; those who are curious in tracing the history of formation of the young, of course attend to this, that there are some animals that are between the oviparous and viviparous kind. One cannot express the thing plainly; they are oviparous, but the egg is hatched in the body of the parent animal. This is common in the lower class of animals: and it is even so in reptiles. The viper is an ovoviparous animal, as its name implies; but this is even met with in quadrupeds. Mr. HUNTER took a great deal of pains to get the ostrich to breed in this country, but he could not. We have become acquainted with a monstrous animal, the *kangaroo;* it is as big as a large sheep; and this great animal produces a young kangaroo that does not weigh above twenty grains. I have seen it when it did not weigh a scruple. It seems to be an ovum hatched in the uterus; and then, when it is hatched, it is excluded from the uterus, and received into a sort of pouch at the bottom of the abdomen; then grasping hold of the nipple of its mother, it sucks when it pleases. Here, too, how curious are all the works of Nature! you

know, the old kangaroo is an animal with a monstrous pair of legs, with which it can leap the length of this room, and with fore paws which only serve it to scratch or feel itself; but how different is the young one; it is a thing with most monstrous fore paws, but scarcely any hind legs at all: with those fore paws it clings to its mother, sucks nourishment, and then, having attained to a certain state, it pops out of the bag; and this gives almost the first intimation to observe, that the female kangaroo has been in the family way.

Now, I say, whether you can see it or not, you must admit that all that De Graaf said relative to the power of generation, is true; whether you can see this process or not, as it is ordinarily carried on; certain it is that it is carried on; and sometimes the ovula Graafiana will grow to perfection in the ovary. O, plenty of cases are on record to prove this, but it is gratifying to refer to cases which I myself have seen; and the first case I mention, was the case of a woman who died in the Middlesex Hospital, when a Dr. Croome, one of the midwives, was one of the physicians that attended her. The body being opened, the appearance of a sac was perceived with the broad ligament of the womb about it. Here it is; and here is the uterus enlarged. Now then, this being laid open, it was found to contain a full grown child. This was the left ovarium. How can you prove that, you may say? Because it was included in the broad ligament; because the fallopian trumpet was attached to it in the vicinity of the bag. Now if I tell you that Mr. Hunter examined this particularly, and that he was convinced it was the ovary, even if there were no other facts on record, but there are, it would be sufficient. Very well, this is the fecundation of the ovula Graafiana in the ovarium. But suppose the fallopian trumpet should prematurely quit its hold of the ovarium; suppose a kind of ulcerated process takes place, and an opening is made, and this ovula Graafiana gets loose, but that the fallopian trumpet attracts new powers in the uterus, what then? Why this little atom of vital matter endowed with seed, is like a seed thrown loose into the earth; it sends forth roots, attaches itself to the earth, and lives and grows there. Now this is the case in ventral or abdominal fœtation. About the same time that one case occurred in London, another likewise happened: I have a drawing of it here, but it is horribly badly done: a woman, who was pregnant, died, and being de-

livered, she was opened, and the appearance I now represent to you was exhibited: the bowels being drawn aside, a full grown fœtus was found attached, by a sort of mock placenta, to the mesentery. They did not make out exactly whether there were membranes or not; but here, as in the other case, you will observe the enlargement of the uterus; that goes on simultaneously, and it is a thing curious and deserving of your consideration. As to vital actions arising out of necessity, you may say, that provided the ovula Graafiana gets into the uterus, there is a necessity for a supply of blood, and therefore it enlarges, but without the ovula Graafiana getting into the uterus, the uterus enlarges, and the parts are all removed from one another; there being a child in the uterus, the mammæ enlarge, and the secretion of milk begins just when it is wanted, and not before.

Then with respect to the ovarial fœtus; this takes place. I did not bring down the drawing of it with me; it is in the transactions of a society for the promotion of medical and chirurgical knowledge; the drawing and relation of the case is given by Dr. CLARKE, and a very curious case unquestionably it was:—A woman had an enlargement of her abdomen; it was a very firm kind of enlargement; she gradually got faint, and feeble, and she died. The nature of the case was by no means understood, and therefore they opened the body. The abdomen was found distended with coagulable blood; they cleaned away all the blood; they concluded that the woman had had some aneurism of some principal vessel of the abdomen, and that it had burst. They removed all the peritoneal coverings; sought through all the branches of the aortic system, but found nothing of what they had expected to find. They were going to relinquish the prosecution of the examination, ignorant of how this blood had got into this situation, when some one suggested, or thought, that it was reasonable to believe the woman was newly with child; this excited Dr. CLARKE'S attention, and he thought that if this were so, he might at least see the ovula Graafiana; he examined and he found it; he found that the fallopian trumpet had burst, and that that was the source of all this hæmorrhage. I mentioned to you, that the membranes of this part became so vascular that they underwent erection from desire; but though very vascular, it is impossible to account for this in any other way than this, namely, that there was something exciting irritation

in the part. This was an ovarial fœtus, the fœtus being of a size that it could not pass through the ends of the fallopian trumpet, and which was big enough to be distinguished manifestly as an ovum. I myself gave Dr. CLARKE a preparation of a fallopian fœtus, which had attained a very considerable size; he was the midwifery lecturer at the hospital at that time, and I thought it was more interesting to him than to me, so I gave him the preparation.

Well, there is no doubt then that the ovum is formed in the ovary, and that it passes through the fallopian trumpet into the uterus: all this is proved by results in the course of nature. Now suppose this ovula Graafiana to have got into the uterus, what takes place? Why, the uterus enlarges; but it is the *fundus uteri* that enlarges, and really three weeks may elapse and scarcely any thing be seen but a swollen and vascular state of the fundus uteri; but, after a little time, you see a little membranous bag, with *floccular fibres*, shooting from every part. This is the first thing you see in the human ovum; and what do you afterwards see if you open it? Why, you see a little speck of matter, which is the rudiments of the fœtus; it is a little speck indeed, and hangs from the bag; however, as the ovum advances to maturity you distinguish certain parts in it. Now Mr. HUNTER's plates were considered as masterpieces, in point of engraving, and yet SOEMMERING seems to have surpassed him; he has found artists that seem to have surpassed all others.

Now then, I say, you see certain things in the ovum; you see that it is a membranous bag, composed of two membranes, the outer one is called the *chorion*, and the inner one is called the *amnion*, containing the fluid in which the child floats. There is another membrane here reflected, that is produced by the uterus; this is exceedingly abundant in the young brute, and is called the *membrana decidua*. When the ovum gets into the uterus, the uterus throws out a sort of fibres, which are produced in every direction; subsequently these fibres become thick, form the placenta, and the whole communication between the ovum and the uterus is effected by the placenta. In one part of the chorion you have the placenta, but at first the attachment seems to be of a more general kind. Well, then, the human ovum is a bag of water, composed of two membranes, the external of which, called the chorion, is evidently vascular; the internal, the amnion, does not appear to be vascular, but

you cannot doubt of its vascularity, since it probably secretes the water in which the child floats. Then the uterus throws out a membrane, the processes of which seem to meet processes thrown out by the chorion; but this general attachment, by degrees, subsides, and an especial attachment is made, between the uterus and the ovum, by means of the placenta.

Then what is the placenta? O, you know it very well; I need not particularly describe it. If you take a placenta, and inject it with wax in a child, you will find that the umbilical vein spreads its branches all over the placenta, and so do the arteries. The branches of the umbilical vein coming from the placenta, may be said to be the roots of that vein, being the blood which is carried to the child, and probably its nourishment; but the blood returning from the child goes again to the placenta. There do not appear to be communications large enough to let wax further pass; but there is no doubt that there is a communication, for if you inject with subtile injection, and throw it in by the umbilical vein, it returns by the umbilical arteries, and vice versa. It is undoubted that there is a connexion between the placenta and the child; that blood goes from the placenta to the child, and that it returns from the child to the placenta again to have some office wrought in it; and it is reasonable to suppose that the placenta serves the child in some such way as the lungs serve the adult subject; that is, that it effects some change essential to vitality.

Now the ground of this opinion? Why there are children born under circumstances in which the placental circulation has been obstructed. The funis umbilicalis has been known to slip down before the head of the child, and then the head of the child, pressing down, squeezes the funis umbilicalis against the pelvis, and no circulation goes on; under these circumstances, the child is always born dead. The placenta is not less necessary to the child than the lungs are to the adult.

But this is one view of the matter. The mind is at once set upon an inquiry, and it proceeds to ask, how is all this blood got together—where does it come from? Be aware I am only describing one portion of the placenta, and that is the fœtal portion; for there is another placenta to the uterus, the maternal portion. You inject the fœtal part as subtilely, but the

uterine part is not injected; nay, the maternal part of the placenta, this uterine part, may be injected from the vessels of the uterus. Throw injection into the uterine arteries, you inject this and make it as red as possible, but the fœtal portion may be taken off from it, and it will not appear as if injection had been used. I say they are separate; you can inject them separately, and make each as red as the injection you use, without injecting the other; so that there appears to be a distinct circulation between the uteral and the fœtal parts of the placenta; but doubtless the fœtal part is supplied with blood from the uterine part, but how this supply takes place I believe no one knows. In animals the uterine part of the placenta—in many animals the uterine part of the placenta is of considerable substance, and it forms little cavities, into which nodules belonging to the fœtal part of the placenta enter; but in the human subject the maternal part is of a very flimsy texture, and you may, if you plunge a pipe into the placenta, where it is attached to the uterus, throw in an injection which will pass into the uterus. O, there is no doubt that there is a communication, but the communication is intricate, and, as yet, unexplained.

Now the question is, Why, why is this intricacy? Can any good reason be assigned for it? The answer is Yes; seemingly a very good reason. If the vessels of the mother had led into the child; if the blood of the mother had directly communicated with the child, the child would have participated in all the illnesses of the mother. Sickly mothers would have always borne sickly children. It is of great consequence that a child should be healthy; and sickly mothers often bear healthy children. The child has an independent life; the mother furnishes it with nourishment, but the functions are so far unconnected with the circulation and general health of the mother, that the child does not suffer by the maladies of the mother, at least in any considerable degree. Mr. Hunter, thinking on this subject, was led at first to suppose, that the diseases of the mother could never be communicated to the child; but he lived long enough to see his error, for infectious diseases are communicated to the child after a certain period. If a mother has *measles*, *hooping-cough*, or *small-pox*, the diesease is not communicated to the child till it is about six months old; after that period, the child is liable to participate in the disease. Children generally do participate in such diseases, and prema-

ture labour coming on, children have been born covered with small-pox; but this does not happen till perhaps the seventh or eighth month. I knew an instance in my own acquaintance, of a mother who had the hooping-cough, and who produced a child with the hooping-cough at the time it first breathed.

Well, then, I have told you as much as I believe, physiologically, need be told, relative to the human ovum. Some have bothered themselves about the use of the waters of the amnion; some have entertained an idea that the child might drink them, and be nourished by them; but this is very absurd, because children are supported by blood from the placenta; and children have been born without the power of deglutition, who, nevertheless, have been well formed and strong. The waters of the amnion exceed by thirty times the young fœtus at first; and, in the case of the old fœtus, the bulk of the fœtus exceeds by seven or eight times the waters of the amnion You cannot imagine any better defence for the delicacy of the young, than that of having it suspended in a bag of water: any action of the uterus cannot injure it; but as the texture of the embryo becomes firm and capable of bearing pressure without being injured, so do the waters of the amnion go away. You know the growth of the ovum is entirely in the fundus uteri for a certain length of time; at about half the period of utero-gestation, the uterus rises up out of the pelvis, at that time do uterine motions take place in the child, and, as the phrase is, the woman quickens. Now, at this time, the whole of the cervix uteri is perfect, but by degrees the cervix uteri becomes extended, and made part of the bag that contains the fœtus. The obliteration of the cervix uteri, therefore, denotes the progress of pregnancy, and it is not until the ninth month that it is completely evolved, so as to make a part of the bag that contains the ovum.

Now this, no doubt, is very curious, and is the consequence of the upright figure of the human female. If it had happened otherwise, the weight of the ovum would have gravitated against the *os uteri;* as it is, it can never gravitate till the uterus has obtained its full dimensions, and till the cervix uteri has expanded, so as to make part of the general bag that contains the child. The uterus gradually comes up in front of the bowels, and the progress of its ascent is noted; by the eighth month, it has got above the umbilicus, or as high as the umbilicus; it gets higher still, and seems to fill the whole front

of the abdomen; but, towards the ninth month, a very considerable change takes place in the situation of the uterus. Actions preparatory to parturition take place, which bring the *os internum uteri* and *os tincæ*, into the very middle of the superior aperture of the pelvis. These are those adjusting actions preparatory for parturition which take place. Then, at the ninth month, more efficient actions of the uterus take place, and the ovum is protruded against the os tincæ; it yields, but yet it is injured, and forced away, so that blood is discharged from it, and it is made gradually to enlarge. There have been some who have puzzled themselves about inquiring why those actions of the uterus should take place at the generally allotted time; and some have held, that the burthen is kept as long as it can, and therefore that the uterus is glad to get rid of its contents. But it is held only the same time, whether there is a dwarfish fœtus, or a large one; whether there is much or little of the water of the amnion; and, in short, we cannot tell the nature of these things—it is a sort of ordinance of Nature. Then, when the uterus begins to act, it is aided by the expiratory powers; it is first pushed down by that which is free to move, that is, the waters of the amnion; then by the weight of the ovum, and there is carried forward a portion of membrane, which gradually tends to enlarge the os tincæ. Then it is natural to expect that these membranes will burst in their most extended part; but this does not always happen, for sometimes they burst in the uterus, and then the child carries forth a part of the uterus on its head; but generally it is the other way, and then what happens? Why, then, the head passes down; and what further happens has been explained in these lectures, as far as physiology is concerned. Well, the child is protruded, and the uterus is contracted until it has expelled the whole of its contents. The child is protruded out of the vagina, and the uterus is contracted so as to hold nothing but the placenta and membrane of the ovum: resting from its labour a little, it resumes it again, and it expels the ovum. Having contracted, so as to expel the ovum, it must have reduced itself to so small a size, that the vessels will be closed, or will pour forth but a very small quantity of blood. There is no hæmorrhage where the uterus is allowed to expel the ovum after what Nature has designed.

Now, really, there is good in studying these powers of Nature, which we should always endeavour to imitate, and not

interrupt—always endeavour to aid, and not to interrupt. A child does not grope its way into the world, and get through, just as a *chimney-sweeper* gets out of a chimney, by any efforts of his own; it is expelled, and the parts must be perfectly contracted when it is expelled from the ovum. But suppose a midwife helps the child out, draws it forth, and so on; the uterus containing the placenta and ovum is not contracted; and suppose those become detached; O, then you have uterine hæmorrhage; and an inclination to get away the *secundines* may induce the midwife, improperly, to pull. I once heard, what I thought was a good story; a medical man was talking to some comrades in his profession, and he told them that which I dare say he would not have told to others: he said, O Sirs, I was quite astonished to find, and I was not at all aware of, the very little force required for *inverting* the uterus! Now I took it for granted he had helped the child into the world. You know that the uterus has been inverted, and that the inversion of it has afforded the means of showing the menstrual discharge.

Now, I did not speak of the changes the uterus undergoes, and yet I should do that. When the uterus is enlarged, O, we see the veins very considerably enlarged; and when we cut through them, they appear as if they were composed of cells; we see the arteries of the uterus also very considerably enlarged. The arteries of the womb have a very curious tortuous arrangement; Mr. HUNTER calls them the curling arteries of the womb: a very curious tortuous arrangement. And then, on examining the womb, you have appearances which Mr. HUNTER, and others, have believed to be muscular, and some have denied that they are muscular; but no one can doubt the power of forcible contraction of the uterus, who has had his hand in the cavity of it. I need only appeal to the sense of accoucheurs for confirmation on this point. And now, then, I have done with the history of fecundation, and the growth of the ovum, as far as seems to me to be necessary, speaking physiologically.

The *secretion of milk* begins at this time—at the time when the child is able to suck. Now, with respect to the *mammary gland;* it is a gland that one can hardly describe as conglomerate; it is smooth when you cut into it, very firm in its texture, and much intermixed with fat; it is flat where it lies on the peritoneal muscle, and somewhat convex on the other side.

It is a gland sending forth pores, and that is what every surgeon should remember: often have I seen a surgeon take away a diseased lump from the breast; then wiping the breast he has seen a portion of the gland remaining on the woman, and looking white, he has said, O, I see something scirrhous still. Now, a surgeon should never look there to see whether his operation be complete or not; he should divide the substance he has endeavoured to remove, and see whether there are any ramifications of white bands extending further than what he has taken away; see whether he has taken away all the disease, and all the parts in the vicinity of the disease. It is what he has removed that is to be examined, and not the aspect or situation of the bleeding wound. Well, what can we say of the mammary gland? Why, that it is supplied with arteries and veins, as every other part is; that it secretes *milk*, and that the substance is made of *lactiferous tubes*. You may inject those tubes from the *nipple*. The gland seems to be a concretion of lactiferous tubes; those tubes collect together, and make one tube, which ascends up the nipple, and terminates there. Now the *skin*, as it approaches the nipple, suddenly changes its nature; it becomes of a *greyish* aspect; this is the *areola;* and this dusky skin mounts over the top of the nipple. The gland is, therefore, composed of lactiferous tubes of vascular substance, and covered over with skin. But there is so much vascular matter in the nipple that it is capable of *erection;* and when it is erected, the flow of the milk is allowed, by the tubes being kept straight, and it runs out; but when it is not in a state of erection, there is an obstacle preventing the flowing of the milk. There are controversies about the number of the lactiferous tubes, which, as I believe, can never be decided, because there are only a certain number that become enlarged to give passage to the milk, and others remain in a contracted state. You may put bristles into them; but, as for seeing what are their exact numbers, I think it is more than any individual can determine.

Well, now, such is its structure, and now as to its use: it secretes milk, the sustenance of young animals. And what is milk? It is *serum* and *crassamentum:* but the serum contains a certain quantity of *sugar*. Now this is curious, that the milk, both of young animals and vegetables, should have a *saccharine vegetable* in them. And what is the crassamentum—the curd of the milk? I cannot tell you; it is an insoluble matter;

it is something analogous to blood, as I should say. But besides this, there is oil—pure oil in milk; and the propensity of this oil to rise, and carry the curd of the milk with it, is what they call cream. If you separate the curd from the oil, you will find the oil a very pure oil. There are various opinions in France upon this subject; and we have different proportions of sugar, and different stuff, contained in the milk handed to us, which you may search into, if you are desirous of making further inquiry upon this point; but for my own part, it is sufficient for me to know that it is a very nourishing food for young animals.

There are two animal substances, which I say contain, and must contain the greatest nourishment; this is *milk* and *egg*. You know a calf drinks nothing but milk, and the milk is converted into blood, flesh, brain, and bone, with most surprising celerity; *ergo*, it must contain the elements of all these subjects; but I believe it requires a calf's stomach to digest it. Milk is the food of young animals; but I do not think the stomachs of old, or of sick persons can digest it. And out of an egg comes a chick; the egg must therefore, contain all the rudiments of the materials of that chick. I say, suffice it for me to know, that this is the most nourishing food for young animals.

But, physiologically, I feel much interested as to the function of this mammary gland, for here is a gland where you have an opportunity of seeing how much of glandular secretion depends upon nervous action. It is really very curious that such a complicated fluid as milk can be prepared by this secreting gland, with such surprising celerity: a cow will yield a pailful of milk, and which must be secreted almost as fast as the blood is formed; and is this not wonderful, with respect to what the vital powers are competent to effect. But suppose the cow had had the pet: suppose the calf had been taken away from her; or suppose that somebody had come to milk her she did not like, would she secrete any milk? O, none at all. You might stroke out of the lactiferous tubes that which was in them, but the devil a drop more would you get. Therefore you see it requires a tranquil state of mind, and a distribution of nervous energy, for the secretion of milk from the secreting fluid; it is not because the blood secretes, but there must be vital action co-operating, in order to produce the secretion. The effect of those vital actions, in *com-*

pounding fluid, as it were, with such celerity, is, indeed, a matter which cannot but excite our surprise; and though it may excite our wonder, yet it is a fact we cannot deny.

Morbid Anatomy.—I now speak of the morbid anatomy of the female organs of generation; and, beginning with the *womb*, I have to say, that there are diseases in the vicinity of the womb, under its peritoneal coat, under its internal lining, in the broad ligaments, and in the substance of the womb, too, which are similar to one another.—They are tumours of a peculiar texture, like *scirrhus*. Here is one, exposed, to show the texture; this is in the cavity of the womb, formed in the internal lining of it; there are firm fibres, and substances deposited in the intervals, so as to give to the whole a firm degree of hardness, not that extreme hardness, however, that cancerous scirrhous possesses. Yet these tumours never ulcerate; they are not at all of a cancerous nature, they merely increase in growth, and seem, as it were, to tease people to death, by the irritation they occasion in the surrounding parts. Here is a preparation in which there is a number of them in the broad ligament. Some of these tumours are of a looser texture than others; all this Dr. Baillie has remarked. There are something like cells in them, but they all agree in this particular, that they increase in size, have no malignity in their nature, have no tendency to suppurate or ulcer, are inclined, simply to increase in bulk, but they are exceedingly distressing as to their consequences. By pressing on the *ovaries*, they create uterine irritation, irregular and painful menstruation; by pressing on the bladder, they create a frequent desire to void the urine; and so with regard to pressing on the rectum, they produce *tenesmus*, and prevent the discharge of fœtal matter. Persons having these diseases labour under circumtances which are exceedingly perplexing, and it is not till a very advanced period, when the swelling has called for the patient's attention, and the examination of the medical attendant, that the real nature of the disease has been known. There are people who have been hardy enough to poke instruments into them, for bringing about, as they say, a change, and, they also state, successfully too; but this is a practice which I know nothing of. To pass an instrument into the womb, where you are uncertain what you may wound, is certainly a kind of practice that should not be promulgated, until the beneficial effect of it has been substantiated by the relation of a

number of cases, and by the concurrent opinion of the profession. Now when these things, growing beneath the lining of the womb, increase to a certain size, they are protruded out of the *os internum uteri* into the *vagina*. Here is a very large specimen of that sort; and then this is often accounted a polypus of the uterus. They have absolutely become so large, that they have projected out of the *pudendum*, and descended half way down the thigh. I saw a preparation, which Dr. Coombe possessed, where the bladder and part of the pelvic viscera were actually drawn a considerable way down. Being sometimes of a pyriform shape, they have been mistaken for an inverted uterus. These are tumours which you may tie, if you can get a thread or wire over the neck of them; when that is done, the tumour drops off, and the patients are as well as if they had never had any such disease. These are tumours which Leveret invented a sort of wire to tie; he put the wire round the neck of them, and twisted it daily. There are various modes of tying them. Well, then, these tumours, when they present in the uterus, are esteemed, perhaps, polypi of the uterus; but there are polypous growths from the uterus; and here is a preparation of one, it has a thick base. Here is one of those injected, but still it seems to be a tumour with a thickened base.

What I want to tell you is this, that I know very well, that where there is a tumour carrying down with it a process of the membrane of the womb, there is no doubt that that may be tied, and that no harm will result from it; but even those with thickened bases may also be tied, and no harm result from it. But I have met with many cases in the course of my life, where medical practitioners, tying these *polypi*, as they are called, (for they all go by that name,) with thick necks, have included a portion of the uterus. Those tumours sometimes form within the *parietes* of the uterus, and they may drag down a portion of it; but if the uterus is included in the ligature, fatal irritation generally succeeds. Many times have I been called upon to open the bodies of people who have died after they had had what was called polypi tied; and I have found, that a portion of the uterus was included in the ligature. Now this should make us cautious in our proceedings.

Well, then, so much for those diseases, and now, with regard to the diseases of the womb itself: you find the womb

chronically enlarged and diseased; in some cases, the whole substance of the womb is thus situated, and it is known, that in these diseases there is sometimes morbid, and often fœtid secretions, from the linings of the womb. Now this I deem right to mention, because I would not, if I found a case of this kind get well, report it as a case of cure of cancer of the uterus. Here is a specimen showing a uterus chronically diseased; the whole substance of it is thickened. But in the cancer of the uterus, the disease always begins in the cervix uterus; and there are two sets of these diseases; one that begins with scirrhosity, and then goes into a state of ulceration, which is a fatal disease. Here are a number of specimens of them. But the *fundus uteri* remain sound to the last. It begins in scirrhosity in the cervix uteri; and this scirrhosity, which goes into cancerous ulceration, begins generally where there is most mucous secretion, as I may say, about the pylorus, and so on; it extends progressively, and of course the fundus uteri is most remote, and remains sound to the latest period. But there are other cases in which the ulceration in the cervix of the womb throws out a *fungus*—a queer sort of fungus. Here is a specimen of that kind, but it equally shows the fundus uteri, remaining sound, till the disease has so spread as to destroy the life the patient.

Then I must speak of *morbid appearances*, as circumstances contained in the womb itself: and here I have to mention that there are *hydatids*. Here is a specimen of uterine hydatids; they are of an oval form; they have necks, and they seem to be attached to one another. The uterus becomes distended with them; the weight of extension expels them; and the uterus being enlarged to a considerable size, when they are discharged, there is a great deal of blood discharged from the uterus. I have known many people, as I may say, *delivered* of hydatids, and they have done very well; I have known some, who have been delivered more than once of such a burthen. Ruysch had an opinion, that if there were hydatids, there was an *ovum*; but I know not, if any one has had an opportunity of making remarks on this subject, because the woman discharged the hydatids, and then we see no more of them. I say that I know of none, but Sir Everard Home says that he examined the uterus of a woman distended with hydatids, in a workhouse in this town; and he seems to think as Ruysch did, that there was an ovum, but he has produced no voucher

that I can see for this opinion. He says there were membranes, but he only supposes they were the membranes of an ovum. Now, what do we know of hydatids? Why, that whenever they form, they are either in a natural cavity, or in some membranous cyst; so that the finding of a membrane, or a something thrown out from the lining of the uterus, resembling a membrane, would not be an appearance like what you would expect where there is an ovum; and it does not identify that that is the membrane of an ovum.

I may here say, that the ovum itself often goes into a diseased state, and thus the child does not grow; that there are diseases of the *placenta*. Here is a specimen of one—of a diseased state of the ovum, such as prohibited nourishment being conveyed to the *fœtus*; the little fœtus is hanging to the morbid ovum; sufficient nourishment has been conveyed to maintain the life of it, but not sufficient to enable it to grow. When people have an ovum of this kind, in a diseased state, after a time it is expelled, and they are delivered of a very strange thing—of a *mole*, as they call it. And a case occurring near this hospital, of this kind, I went to see it, and I brought away the preparation I now show you, which I found to be the case of a placenta, from its being covered with membranes.

Ovarian disease.—Well, I can say no more relative to the disease of the uterus; and that there are such things told in books, though I have not seen them, as dropsy of the *fallopian trumpet*. But diseases of the *ovary*, which is the next subject I have to call your attention to, are exceedingly common indeed; and it is curious to know that a little body like the ovary, not so big as a pigeon's egg, will swell to an enormous size. Now when it enlarges, it generally has cells in it, and those cells are filled with gelatinous fluid. I show you here a portion of an immensely large ovary, which has been injected. Sometimes, however, a single cell fills and enlarges, and grows till it occupies the whole of the abdomen, and then this is what we call a dropsy of the ovarium. Sometimes the whole enlarges together—there is a congeries of cells, and thus all are enlarged, having a kind of sarcomatous structure, and sometimes the case is the compound of these two; one cell enlarges, or two cells enlarge, and you are obliged to tap them on opposite sides of the body; and there is a certain portion still sarcomatous. Now these are the circumstances you may meet with in diseased ovaries. I say, it is curious that a little part like the

ovary should acquire such a great magnitude. O, the cells often become immensely large, as I shall tell you. You can do nothing in the way of operation, where the ovary is of this cellular structure which I have exhibited to you; you cannot tap a thing of that sort; if you did, you can let out nothing, and it is very dangerous to do it. I have seen, where people thought there was dropsy in the ovarium, that they plunged a *trocar* into some solid substance, but the patient has died. I have known many cases of solid substance of the ovary, where it has attained a certain magnitude and then ceased to grow. I have therefore to declare to you, as my opinion, that where patients have a filling up of one cell of the ovary, or of many cells of the ovary, that the case ought to be treated on purely surgical principles: that when you find the body rising up on one or other side, and painful, you should keep your patients in a horizontal situation; apply leeches, blisters, and produce irritation, for I can assure you that I have seen many enlarged ovaries arrested by this treatment. Of course you would attend to the state of the patient's general health, as I imagine you would do in every case, in which a patient was affected; but I say this should be treated on surgical principles, just as you would an enlargement of any other part of the body.

When the belly is distended, and where there is one great cyst, forming the principal bulk, the question is, about tapping it. Now I should say to you, you must tap where it becomes inconveniently large to the patient. It is a cruel thing for a female to be subject to periodical tappings, but there is no help for it. I shall tell you cases of postponement of this operation, in order to show you the consequence of such postponement. It is a curious thing too, that sometimes when the ovary increases in this manner, the urinary secretion is suppressed. This is one of the circumstances that probably has led to the treatment of the disease as if it were a dropsical disease, as it is called—a dropsy of the ovarium. It is not in every case there is suppression of the urine, or rather diminution, for it is not an actual suppression, but a *diminution* of the quantity; but where such an occurrence takes place, it has been observed that the secretion of urine becomes renewed, and the fluid of the ovary is dissipated. A lady, who had this happen to her several times, went on, in hopes that it would not again occur, till her ovary became most immensely

enlarged indeed, and then she was obliged to consent to the dreaded operation of tapping—dreaded by her; and I let out from the ovary a washing-tub full of fluid, a pail, and many wash-hand basins full. I did not measure the quantity, but it was very great indeed. I closed the aperture, and bandaged her up. Now the consequence? Her abdominal muscles had been so over-stretched, that she could make no use of them; she could not cough: the following night she had some irritation in her throat, and she wished to cough, but she was unable to do it; she poked her finger down her throat, with a view to assist in removing the irritation, but this only made bad worse, and her sufferings during the night were almost intolerable. This was overstraining the abdominal muscles. Now another case: A young lady, who was exceedingly vain of her fine form, had it spoiled by dropsy of the ovary. She was so annoyed by this, that she secluded herself from the view of every one; she went into the country and left her family: she became very large in the abdomen indeed; she was miserable, and, altogether, it produced a great decline in her health; but she shrunk from this operation of tapping, till at last she was reduced to extremities, and was brought back to the residence of her parents. A physician was sent for; he had seen her in the early stage of the disease; he said it was a diseased ovary; he said it must be tapped; and I was sent for to perform the operation, but I could feel nothing like undulation, and I said, I do not think there is fluid in it. O, said he, that is quite nonsense; you must perform the operation; I have known the case from the beginning, and why should you be afraid to perform the operation? I said, it may be a sarcomatous ovary, but if you demand it, I am ready to introduce a trocar, and I did. What were the contents? It had been jelly; it was now almost a solid substance, but there did ooze out a sort of thing that would scarcely pass through the aperture, which the *Doctor* took hold of, and dragged out to a considerable length; but really there was no good done by the operation. Well, you must go on with the practice of your profession, according to those rules that experience has established to be right. But this is so deplorable a thing, for a person, apparently otherwise healthy, to submit to this periodical tapping, that almost every medical man has been devising means to cure the disease, or to prevent the necessity of a recurrence to this operation. Now we are not the arbi-

trators of the life and death of people; if they die in consequence of disease, why it cannot be helped, and people submit to it, for they know it is inevitable; but we had better not be making experiments that probably may destroy them, and bring disgrace on our profession. What has been done? Why, people have passed setons, have thrown in injections into diseased ovaries, and so on. I have known many of those pranks played in the course of my life time, and they have all been fatal. Certainly, in common sense, no one would treat a diseased ovary as they would do the common *hydrocele* of the *tunica vaginalis*. Le Dran did puncture the ovary, so as to have a fistulous opening, out of which the water could be passed; but I do not know that the patient's state was much bettered by that. I have seen this, however, practised, where there were divers cysts in one ovary and a good deal of sarcomatous swelling, in order to prevent the repetition of the tapping. The lady, whose case I have mentioned, said this to me, "What, must I submit to this periodically? Is there no mode that can be adopted to obviate such a course?" And a very rational lady she was. I told her that there was no mode, save this one, namely, putting in a canula, and leaving it in, but that I thought that as bad a doom as the other. She said, "I shall have that done." She was a woman of great determination, and of great intellect; she decided that she would have it done, and that if I did not choose to do it, some other person should. Well, then, I tapped the abdomen the second time, or rather the third or fourth time, for it had been tapped several times before, and put in, through the canula of the trocar, a catheter, and fixing the tube, I dressed the part up, and put on a bandage. I thought this was a very good way of proceeding, but the case terminated fatally. Now I know Dr. Gooch was much inclined to recommend that operation; I have only, however, to tell you what I know myself respecting these cases; and I say, I should be more disposed to let disease destroy the patients, than to incur the blame of having at all aided in their destruction.

I omited to say this—I am convinced that an enlarged ovary, treated on surgical principles, even subsequent to these operations, may do good. And here I have to tell you what has come within my own knowledge in treating the disease in this manner. I have tapped many ovaries, and, by producing counter irritation with blisters and so on, I have kept them from

filling again. I could tell you a very considerable number of cases, but the case of one old woman shall suffice. This old woman I kept nine months in the hospital, and there was no filling of the ovary; I could not detain her any longer; it would not have been right to have done so, and I let her go; but I said to her, if ever this should fill again, do apply early, and she said she would. However, I saw not the old woman again for two years, and then she was a patient in the house, with a sloughing sore of her leg, and her abdomen was full. I asked the old woman why she did not come early, according to her promise; she said she did not like to be blistered. She told me, she had not filled for a year after leaving this hospital; that she had gone to a dispensary, and got tapped; and that this was the second time of filling since she had been in this hospital before.

Now, the diminution of urine is a curious thing. Some would be inclined to say it was the pressure of the ovary upon the kidneys that caused it; but that opinion cannot be maintained, for the ovary will fill, and the secretion will come on when the ovary is at its greatest magnitude; just as I have observed in the case I have mentioned; for several times that lady was relieved in this way, and which induced her to postpone the tapping.

I remember, when I was quite a boy, hearing this case: that an old woman, or an oldish woman, with an exceedingly large belly, was waddling over London Bridge on a cattle day, and there arose behind her a cry of mad ox! mad ox! She was terribly afraid; she waddled into one of the recesses on the bridge; the ox passed her; and from the state of fear into which she was thrown, she immediately began to piddle, and the water continued to flow from her, until she was as light as a greyhound.

Now this suppression or diminution of urine cannot be owing to pressure; and what is it owing to? O, no doubt, it is a sympathy; no doubt that the kidneys are sympathetically affected with the ovary. I would not tell this to those who choose to be incredulous, but I tell it you, as my opinion of the only solution of it; and if you can keep the ovary exempt from irritation, you will keep the kidneys from this sympathetic affection. Thus it has happened, when I have treated the disease of the ovary by surgical measures, after the depletion of their contents, that I have prolonged the secretion of urine for

an indefinite period. I do not remember whether, in the case of the lady I have mentioned to you, there was a diminution of urine; I do not remember whether there was or not, but I remember many patients in this hospital in whom it was; one woman in particular—an old *stager*, who used to go into all the hospitals in London, to get tapped; I tapped that woman; afterwards, I put on the bandages, leaving a little sort of trap-door, where I could apply a blister; by doing so, keeping her in a recumbent position, and attending to the state of her bowels, she has got round, but her urinary secretion used to stop on the third day.

I may tell you, that whenever the water is discharged, in general there is a flow of urine at first—a considerable flow of urine from the relaxation induced in the cyst of the ovary; but, on the third day, the ovary begins to fill again, and the urinary secretion is diminished. It is not in every case of diseased ovary you find this; sometimes you find it, and sometimes you do not: and this is the case of all sympathetic affections that I know anything of: the stomach and the head sympathize; and there are people, too, of diseased stomachs, without cerebral affection.

I have a preparation here of the state of an ovary, where, instead of jelly, or a sort of gelatinous fluid, or a bloody fluid being found, there was stuff like water-gruel in it; this stuff became solid from residence, and was afterwards rolled up into balls like gun-bullets. The surgeon who examined the body, and who gave me the preparation, was much surprised at this form of appearance. There was also a great ball of hair rolled up, and plastered over with this water-gruel sort of stuff.

Yes, you find hair grow from the sides of the ovaries, and afterwards become detached; but it is curious that this hair has no bulb to it, as hairs in common have. Also oil is found in these cysts; oil which becomes like suet when cold. If you open the body when warm, you will find it to be oil, but if you allow the body to get cold, this becomes of a suety sort of consistency.

Also, in the ovary, you sometimes meet with *bones;* and whenever bones have been met with, the medical man, who has examined the case, has been apt to suspect that there was a fœtus. This was the opinion formerly, that there was an ovarial fœtus in such a case, and that the rest of the fœtus had

decayed and been absorbed, but that these bones had been left: now Doctor BAILLIE invalidated this opinion, by the examination of a child, who must have been a virgin, and in whose ovary he found a bone. Then I dare say those who are disposed to speculate, might say, O, well, I do not wonder at that; the ovary is kept down with very peculiar and elastic powers; it may form bone; it is intended to form bone, which might be the rudiments of the embryo, and so on. Now that does not at all seem to me to be sound physiology;, and it is invalidated by this case—a case which occurred in an animal that was neither male nor female, to wit, a *gelding*. One of the horses of the artillery at Woolwich died; Mr. COLEMAN had him opened, and he found below his kidney a kind of cyst, which had in it a tumour, of a fleshy hardness; he tried to cut it through, but he could not, and he was obliged to saw it. It was a fleshy tumour in a sort of cyst, but containing *horse-teeth*—large horse-teeth, with processes of enamel descending between the bones of the teeth. Now these could not have been formed, but upon the bulbs, and upon compound capsules. Well, then, the vessels of a part seem competent to do any thing, and to build up any kind of structure. Now this is what I have to say about the disease of the ovary.

Superfœtation.—A great deal of fuss has been made of late where appearances have been met with, that have led to the idea of the existence of what is called *superfœtation*, as if there was an embryo within an embryo. You have heard of those cases, doubtless. A person has some swelling in the abdomen, the nature of which is unintelligible; and, subsequently, the patient dying, on being examined, it is found that this swelling is something like a fœtus; that there is a head, and a trunk, and limbs to it. But, in general, nothing has been made out, as to the supply of such a fœtus; I have no doubt it is supplied, if the case was made out at all, by the aortic system of the subject. But it is supposed to be a sort of second conception—a fœtus within a fœtus, and looked upon as exceedingly marvellous. Now, if the same thing had grown upon the back of a patient, why there would have been no marvel in it: do we not find children formed with superfluous membranes, and yet consider them as no matter of surprise at all? I may be wrong, you see, in these opinions, but I tell you my notions about them. To me there does not seem to be anything very remarkable.

Monstrosities.—I have not talked of *monstrosities;* the subject is endless; but I wish to tell you, what seems to me, physiologically speaking, the most curious thing with respect to monstrosities, as related in a case by Dr. CLARKE: a woman was delivered, who had two ova; out of one ovum was formed a healthy well-formed child, but there seemed to be more in the uterus; and out of the other ovum there was delivered a mass of flesh, about seven or eight inches long, three or four inches in breadth, and one and a half, or two inches, in thickness; in one part of it there was inserted the funis, and the other end was attached to the ovum; it had neither head nor extremities. Dr. CLARKE took it home, and injected it from the funis; he made it red throughout with injection; it was a fleshy mass; it was cut asunder, and what do you think he found in it—heart, lungs, or digestive organs? Not one of them; it was a vascular mass throughout; but nearly extending the whole length of it, there was a gigantic thigh-bone, much bigger than could have belonged to any child born in the natural way. You will say, what vessels were there to supply this bone, when there was neither heart nor anything of that kind found in the mass? Did the vessels emanate from the placenta, which produced a mass of flesh, and a well modelled thigh-bone, differing in nothing from a natural thigh-bone, saving its gigantic size?

Well, I say, in a physiological point of view, this is curious. In general, it is said, where products come out from a second ovum, that there is a communication between the vessels of the placenta of the two ovums, so that this accounts for a sort of mode of conveying nourishment.

Well, this is all I have to say on the subject of the morbid anatomy, or its appearances in the examination of the female genitals.

ON THE BONES OF THE HEAD.

The cranium is of very different degrees of thickness: some there are, whose crania are very thin, mere paper skulls; and some there are, whose skulls are as thick as possible. No surgeon, therefore, by looking at the outside of a man's skull, can tell whether it is thick or thin; and if he had been trephining a thick skull, and immediately afterwards came to work on a thin skull, he certainly would have the instrument into the

brain, unless he took very great care. In trephining the skull, you are always to go on as if you were trephining the thinnest skull possible. They say you may go on till you come to the *diploe;* now this diploe is not much to be depended upon; there are thick skulls with a very little diploe, and there are thin ones with much; in short, the diploe appears to be most abundant in the middle period of life; it diminishes in old age, and there is but very little of it to be found in a child. I say, therefore, you must trephine as if you were trephining upon the thinnest skull possible, and as if that skull had pits in it, for there are pits in the skull, more especially on each side of the longitudinal sinus; and suppose a person were trephining upon a skull where there was a pit? why, if care was not taken, the trephine would be cutting the membrane of the brain where the pit was, before it had penetrated the bone in the other parts.

Now there is no caution laid down in surgical books with regard to the operation of the trephine, which I believe to be necessary. You must frequently take out the instrument, and examine with a searcher, to know whether you have penetrated the skull; you may penetrate it at one part and not at another, and you should let the teeth bite on that part which is the least likely to be penetrated; in short, it is better to break out the part, when you have once made the slightest penetration, than to run the risk of going through upon the brain. I say it is an unpardonable thing to do so; it is like cutting a hole in a man's belly; if you divide the *dura mater* the blood will flow out, and you cannot sew up the wound again. I have further to say to you, though you may trephine—though you may use Hey's saw, as they call it, and all this, with very great skill, you sometimes do meet with a difficulty in raising up a depressed part of the bone, owing to the internal table, which I believe to be a more frangible table than the outer one, being broken to a greater extent than the outer table. The outer table is the larger, and the internal table is the smaller portion of bone, and when the external table is broken, there is often a considerably larger portion of the internal table broken, which may be wedged in under the external table, and create a very considerable difficulty. It is necessary that this should be borne in mind, in order perfectly to understand the events which occur in operative surgery.

Now there are two states of the skull to which you should

particularly direct your attention; one is, the state in which you meet with it in the fœtus. In the fœtus it is made up of many pieces of bone. There is the *pericranium*, for so the *periosteum* of the skull is technically called, externally, and the dura mater internally, and the bone forms between those two membranes. Points of ossification are set up with some irregularity, but they join together and make plates of bone. There are many pieces of bone in the head of the fœtus, but they soon coalesce and leave only a certain number of principal distinctions. The bones are by no means in contact with one another in the fœtus, but they afterwards become united.

The *bregma*, or *fontanella*, which is an opening, or large interstice, between the *parietal* bones and the middle of the *os frontis*, is deserving of your attention. All your midwife teachers call your attention to this, for you can distinguish whether the child is dead or alive in passing through the vagina, by putting your finger on this part of its head; if it be alive, you will feel the pulsation, but if it be dead, the brain will not pulsate. You may say, why is the head of a child, when born, composed of so many pieces? Why, it allows of a moulding of the head into the figure of the cavity it has to pass through —the cavity of the pelvis; if it were formed of solid bone, it would hitch in passing through that cavity.

Well then, I say you must fix your attention to the head as you find it in the fœtus, and as you find it in the adult subject. There, it is really one piece of bone. In very old life you see it made of one piece of bone; there is no vestige of the connexions of the bones. The sutures, of which I shall speak presently, are formed intermediately between those two states; but you will always find the sutures beginning to form, first in the inner and last in the outer table of the skull; and you will always find the sutures first obliterating on the inside and last on the outer side.

I would not give two pins' points for a man who would take up the skull and describe the proper bones and sutures of it; for what I want to know is, how you would find them out on the living skull? The pericranium adheres firmly to the crack of a suture, but it does not do so to that of a fracture. To say the truth, in old worn out sutures, as I may call them, there is a great difficulty, and the parts are very frequently mistaken; but how are you to find the sutures? About an inch behind the *angular process* of the *os frontis*, you find the commence-

ment of the *coronal* suture; the coronal suture ascends upwards to the very vertex of the head, touching a perpendicular plane; this is not very decisive, but generally it is correct, and to be relied upon. From the middle of the coronal there proceeds the *sagittal,* which is as straight as an arrow; but where does it terminate? Upon a line drawn horizontally round the cranium from the commencement of the coronal. Then the termination of the sagittal gives you the commencement of the *lambdoidal.* Where does it end? I will say no more about it at present, than that it terminates behind certain bumps. Now a person who had turned his attention to this might draw out a sketch of the sutures upon a shaven living head, and make oath to the chart. At the same time you must not put entire dependance upon this; if you presume that the sutures are always to be found exactly as I have stated them you will be in error, because sometimes there is a suture down the os frontis; it has been continued through life, as in the fœtus. Always the os frontis of the fœtus is to be found in two pieces, and the sagittal suture (for that is the expression used) is sometimes continued down the os frontis.

Form of the Head.—With regard to the *form* of the head, that is more a matter of curiosity than of useful knowledge. Mr. Hunter and Camper were turning their attention to this subject at the same time, without apparently knowing what each other was about; and they seemed to think that there was a descending series in the head of every animal from man. Camper represents that the Greeks delineated the head with a wonderful degree of accuracy. The Greeks were very intellectual men, undoubtedly; and I have often heard artists wonder what there was in the statues of the Greeks which could so fascinate the beholders. To me it seemed to be perfectly plain: they have exaggerated every thing in which the human head differs from the brute, but then with a delicacy that left the exaggeration hardly perceptible. What have they done? They have raised the human *forehead* almost beyond the facial line; elevated it exceedingly, expanding from the orbits, rather than shelving off from them. They have wrought up the *eyebrow* in a very surprising manner. Now the eyebrow is a feature peculiar to man; no animal has it; there can be very little doubt of its being an index of the mind—a part formed for expression. What have they done with respect to the *eyes?* Made them to look straight forwards; the eyes of brutes look

to the side. A hare, for instance, will run up against a post, if it is right before it, and break its neck; it will run against its direct foe, provided that foe is directly before it. No animal has a *bridge* to its nose; they have all *snouts*. The bridge is peculiar to man; and they gave him an ample one, raising it high above the sockets. The Greeks thought the *nose* looked handsomest when it came off *straight* from the head; the Romans liked it *arched* best, and they made it in that form. As to the *nostrils*, they made them as thin, and as unlike a snout as possible. And with regard to the *mouth*, they made it as little like a devouring aperture as they could. The *lips* they made particularly muscular, strong, and expressive. And the *chin*, which belongs to the human species, and not at all to animals, they made broad and expressive; and upon the whole they managed to produce a grand effect. Now only sketch but this, and you will have the very front of *Jove* himself. But I say the exaggeration of every part of the human head, in which it differs from that of a brute, was done with such delicacy, that it required knowledge and discernment to detect the exaggeration. Well, so much with regard to the form of the head, which I do not go into at length, for I say it is rather a matter of curiosity than anything else. GALL and SPURZHEIM have gone much further; but if what they state was to be considered as referring to the brain at all, it would have to be considered whether the convolutions of the brain are organs producing the faculties which those gentlemen assign to them.

Orbitar Plate.—The orbitar plate is *exceedingly thin*; there is but a very slight partition in this part, separating the eye from the brain; and often have I seen *disease* begin on one side of this plate, and work its way through to the other; the partition is so thin as to admit of disease doing that, on whichever side it begins. A man has been working in a hay-field; he has slipped and fallen; the prong of a fork has entered his eye; he has got up, drawn it out, thrown it from him, and instantly fallen down dead, apparently without any cause. A speck of blood has been observed on the eye; it has been wiped away; an aperture has been observed; and upon examining the part, the fork has been found to have penetrated into the brain.

Now, why do we learn all the particulars of the different bones of the head by rote? For the practice of our profession;

and without such learning we could not practise it. Where may you trephine? Draw a horizontal line three parts above the orbit of the eye, and you may trephine where you list, save and except you may not do it precisely on the middle, because then you would come on the *frontal spine*. If you were to trephine lower down, at the sides, you might penetrate into the orbit of the eye. A person may say, surely I may trephine a little lower than this; but, I ask, why should you wish it? Are you not almost as low as the bottom of the skull? I say, making general rules, we ought, if we err at all, to err on the side of safety: it is foolish to walk on the very brink of a precipice, if the same good can be effected by keeping at a little distance from it. This, then, is the rule: *do not* trephine on the *middle* of the os frontis, for you do not know where this frontal spine, or *crista interna*, is to be found. They say, if the sagittal suture is seen continued down the os frontis, this spine will not be found; but this is not true, for I have seen it in such a skull.

You ought not to trephine over the *arteria meningia media*, if it is possible to avoid it. Neither would a man trephine, so as to open the *longitudinal sinus*, if he could help it. There is such inequality in the bone at this part, that there is great risk in opening it, if the trephine is applied here. I know it has been often opened. I remember a *madman*—and, upon my word, his madness was of a curious kind, for he would just do what he liked; he applied to me to give some opinion about his case, and, you may be very sure, I told him to *keep his bowels regular*. But he was persuaded that there was something wrong about his head. He told a rhodomontade story about having been pitched off a horse, and that he knew there was something wrong; but he pointed to the precise part which GALL and SPURZHEIM make the organ of *obstinacy*. He wished very much to be trephined; I would not do it; I said the man who would do it would be as mad as himself. However, a surgeon did do it; and he might do it if he thought there would be no harm in doing it; but, *egad*, in doing it, he laid open the longitudinal sinus, and the man lost a great quantity of blood. Whether that loss of blood was the cause of it, or not, I don't know, but the man became exceedingly quiet and rational afterwards; and he did not rejoice so much for the relief he experienced, as at the *triumph* he had obtained over myself; that seemed to be his great source of

delight. However, shortly subsequent, he became as mad as ever. The surgeon was induced to do it, for sometimes there is a speck of bone growing out or into the brain, which creates great irritation. The surgeon was a well-informed man, and no fault was to be found with him; perhaps the man deceived him. But, I say, when you come to the longitudinal sinus, ascending half way up the perpendicular ridge, there are such irregularities, that I defy any body to trephine without wounding the sinus.

Mr. Pott speaks of this wound of the longitudinal sinus; he says he has opened it with a lancet, to take blood, as it were, from the vessels of the head immediately. All this may be done where the wound is made upon the upper part of the head; but what are you to do where the sinus is wounded in the lower part, when people are delirious, and are turning and twisting about, from injuries received on the head? You must lay them on their face; and this really cannot be done; I can only tell you, that I have seen this sinus wounded two or three times in my life, and though I do not believe it killed the patients, yet there was always a dripping of blood till they died.

Now, what can you do *surgically* to the *os occipitis?* You may trephine it on either side of the perpendicular ridge; but if you have anything to do with the under part, you must cut off a man's neck to get to it. I know there are cases on record, where bones have been taken away, and so on, from this part; but I speak generally; and if I err, I give you admonitions on the side of caution.

ON THE PANCREAS AND SPLEEN.

Physiology of the Pancreas.—The pancreas is circumstanced like all the other salivary glands; it is situated in the vicinity of some large vessel, such as the splenic artery; it is supplied from the splenic artery with numerous branches. We have every reason to believe that the blood moves with force and celerity through the glands of this structure. These glands are all situated where occasional pressure may be made: and the very agitation of the food to and fro in the stomach might create a pressure alternately on either end of the *pancreas*, and certainly this pressure seems to excite the *secretion* which takes place there. People sleep, perhaps, with their mouths open, and when they awake their mouths are quite dry; but the

making only of a few motions of the jaw produces a flow of saliva into the mouth. They are also liable to excitement by anything that acts as a stimulant on the system. Food sometimes put into the mouth acts as a stimulant, and even the very sight or odour of the food produces a flow of saliva. Now this I say, in order to show you that it is probable the pancreatic liquor is not secreted at all times, but is liable to be poured more copiously into the intestines at the time digestion is taking place than at other times. It is notorious that the bile flows more abundantly at that time than at any other time into the *duodenum*. I have told you what has been remarked respecting it, but I wish you to understand now, that there is reason to believe that the pancreatic liquor is discharged at the same time; in short, however, we are very unacquainted with the office of the pancreatic liquor. The *structure* of the gland would lead one to believe that it was a watery liquor, like the saliva: and as far as it has been noted, that opinion has been confirmed by observation: but I am persuaded it must be a very difficult thing to obtain any pancreatic liquor for examination. Sir ASTLEY COOPER could not obtain any, though I am sure he must have taken considerable pains to do it. I say, of the physiology of the liver we know but little; and I say, I may affirm of the pancreas the same thing. The digested aliment, the *chyme*, is viscid, and it may require some sort of dilution, and this may be a source of watery fluid, a proper diluent for it. That may be the case, or it may not; in short we do not thoroughly understand the use of this gland. That is all I have to say as to its physiology, and I go, in the next place, to speak of its

Morbid Affections.—That glands of this nature are subject to morbid affections must be confessed, but I think they are not so subject to frequent disease as other glands in the body; and for this, it appears to me, there is a good physiological reason accounting for the fact, that is, the facility with which they secrete. What is the great cause of that increased vascular action which builds up the disease in different organs of the body? It is the great flow of blood exciting vascular action; it is the turgescence of vessels which frequently occasions disease; but if those vessels can disburthen themselves easily of secretion, then there is less likelihood of such structures going into disease.

The pancreas is liable to *cancer*. I have seen the pancreas

undiseased in the midst of disease; that is, where there has been disease surrounding it in all parts. The pancreas is a gland of much looser texture in some subjects than it is in others; and if a person had met with several pancreas of loose texture, and should afterwards meet with one of a very firm texture, he would say, "O, I am sure this is *scirrhus*;" but I am sure it is not, it is only a firmer pancreas than usual. However, the pancreas does sometimes become cancerous, and the scirrhosity which precedes cancer may be easily distinguished. There is always a whitish induration of matter, consisting of a sort of strong membranous bands. Of the structure of cancer I have spoken: Dr. BAILLIE has described it, and I know not any better description that can be given of it than that which has already been given.

I shall now tell you the history of a cancerous pancreas, and as those histories are not very commonly recorded, I have some satisfaction in doing this, inasmuch as it seems to be elucidating the physiology of the pancreas, and, in a degree, showing that its *function* is not of very grand importance. There was a medical man in London, a most respectable man, a man of considerable knowledge and judgment in his profession, but especially in the *pharmaceutic* department of medicine; he was, in short, a very old, and a very respectable apothecary, known, of course, to all the physicians in this town; and they were at one time very much, I may say, dependant upon such practitioners as I have described, and paid great court to them. I mean to tell you that this man was attended by the best physicians and best physiologists in London, but not a soul got at the real nature of his malady; it was not ascertained till he was dead. It was observed that he took aperient medicines out of his own shop at the commencement of his illness. His bowels, therefore, seemed to be in some degree constipated; but that was not wonderful. At the commencement of his illness, he looked ill; I mean like a person that had some serious disease within him. Well, I never saw any one who was in that state, but where it was betrayed in his physiognomy. This is a common observation. I say to patients every day, "O, you will not die yet—you do not look as if you were going to take your departure." This is a common observation. We go along the streets, and seeing some poor fellow pass us, we say, "Ah, poor fellow, he has not long to live in this world, I am sure." However, the individual I allude to looked as if he had some important disease

within him; nevertheless he ate his food, and I suppose he kept his bowels regular. When the pancreas began to ulcerate, it produced no effect but that of local pain; he complained of pain in the pit of his stomach, which gradually extended in its dimensions; but what was the cause of that pain no one could find out. As it advanced, he was constantly stooping forwards; towards the latter part of his life he lay in bed, propped up with pillows, to lessen the pressure that might have been made on the pancreas. He still had an appetite, and his bowels were kept regular. He found that when he ate more than he should have done, that that aggravated his pain; so much so, that he learned to relieve himself by poking his finger down his throat, and pushing forward some of the food through the stomach. The pressure of the food on the pancreas increased the pain. He also found that a *clyster* relieved him, probably by unloading the *colon*, and lessening the pressure that might previously have taken place on the pancreas. Thus he went on, gradually becoming worse and worse till he died, and then his pancreas was found to be in a horrible state of ulceration from end to end.

Stones.—There are stones sometimes formed in the pancreas; they are carbonate of lime. Dr. BAILLIE has given a drawing of stones in the pancreas. Well, these are the chief diseases in the pancreas that I have ever known of.

THE SPLEEN.

Now I come to the spleen, and to this viscus goes the splenic artery. If you inject this part with wax, you will find it dividing and subdividing at acute angles, after the manner of an artery. This you ascertain by corrosion; and when the tubes become so minute that the injection will penetrate no further, there of course it rests.

RUYSCH himself has made it a question, whether you can inject the cells of the spleen; but certainly, if you inject the artery going into it, the injection will come back through the vein, or if you throw water into the vein, it will come out of the artery.

Is there any *excretory duct?* O, it has been sought for, but none has been discovered; sought for with all possible diligence, but there is none.

The spleen appears to be like a sponge; and if you inject the splenic vein, even with coarse wax, the wax escapes into

some sort of cells, and is modelled by those cells. The vein readily communicates with the cells; the artery communicates with them, but with difficulty. Now late anatomists have been making it out, that those cells are *not* cells, *properly* so speaking, but that they are the commencement of veins in a varicose state. Well now, suppose they are; what difference does it make as to function? None at all; therefore, whichever opinion is true, it is not important. The blood that goes through the splenic arteries, which become very small, and then terminate in veins which may be in a varicose state, or, if you please, in cells communicating with the veins, for I do not think it makes any difference, but the blood readily passes into the venous system. So then, the spleen has been considered as a sponge, enveloped in a sort of capsule, and eventually covered by the *peritoneum;* with this sponge, the arteries have a communication; but the communication is difficult; the veins have also a communication with it, but the communication is easy. It is a viscus which varies in size at different periods in the same animal. It often becomes very large, in ill health. You know it is the *ague cake,* which attacks people living in marshy countries. It becomes very large, sometimes without being diseased, and then subsides again. I say it is a sort of sponge, contained in a capsule; and the capsule of the spleen is thicker and firmer than the capsule of most other parts. It often becomes thickened in the diseased state of the organ; I have seen it greatly thickened, and almost like cartilage.

Absorbents.—There are abundance of absorbents about the spleen, and it is perfectly easy to inject them in animals, but not so much so in the human subject; still Mascagni has shown that there are multitudes of absorbents belonging to the human spleen; and every thing relating to the anatomy of the part, the function of which we are ignorant of, should not be unattended to.

Physiology.—Now for the physiology of the spleen; and it was known in ancient times that if a man was wounded in the wars, and had his spleen slip out of his belly; it came away, perished, and yet the man did well. Well, upon the revival of philosophical inquiry, Malpighi took away the spleen of a dog, and he has given a history of the proceeding: according to his account, the dog has rather benefited by the operation than otherwise, or the dog had more appetite, and became

more salacious than he was before the operation. Now when MALPHIGI has led the way, why every person who could get a dog, was cutting out the spleen and publishing the history of the case. HALLER has made a general review of the histories of those *unsplenic* dogs, and the summary is just what we would naturally expect it to be, namely, that though some of the dogs did well for a time, that they became ill, pined away, and died. This is just what we would naturally expect, because no one could suppose that the spleen was made in vain and for no purpose. HALLER thought that they died, because they had not got a preparation of bile; for in HALLER's time it was thought that all the abdominal viscera were but organs for the preparation of bile—that they wrought a change in the blood, that fitted it for the secretion of bile; and every anatomist has observed that the blood of the splenic vein does not coagulate like the blood of other veins. This is an observation which should be attended to, for we really do not understand the physiology of the spleen. Then HALLER, you know, had a theory to support; and, therefore, probably he might consider this as a stronger proof than he otherwise would have done. But the opinion that bile could alone be prepared from venous blood and from the *vena portæ* which contained blood of a peculiar quality, is now, I consider, refuted. Then, why should so large an artery go to the spleen, to have the blood simply to pass through it, without preparing any thing, and to return by the splenic vein? The only rational explanation that has been given of this is, that if the circulation through the spleen can at any time be impeded, it must throw an abundance of blood into the other branches of the *cæliac artery*, so that there would be a most inordinate quantity of blood poured upon the stomach and the duodenum at the time digestion was taking place. It is supposed that the pressure of the great end of the stomach upon the spleen must impede the blood through the spleen, and cause it to flow in abundance in those parts where it is most wanted. I say this is the only rational explanation that has been given of it; and also the only solution of the problem of the unsplenic dogs; for if the spleen could be taken out and the wounds healed up without injury, then there would be an inordinate quantity of blood thrown to the stomach and other parts; so that MALPIGHI's dogs might well have a better appetite and be more salacious, than before they took leave of their spleens. I say this is the only rational theory,

but still it may not be true for all that; and there is one staggering fact which I hear of, which is, that the spleen seems to be largest when the stomach is full. That is a thing I never examined for myself, and I can tell you no more about it. Dr. Haighton took a great deal of pains with this theory, and examined the blood of the splenic vein; he did not find it different to other blood. But he seems to me to have left off where he should have begun; it was his business to have shown that a solid pressure upon the spongy part would impede the circulation; and then to have shown that the stomach was adequate to produce that pressure, which he did not show.

Morbid Analomy.—Now I say the spleen may be greatly swollen, and yet not diseased. When diseased, I see nothing in it but tubercles—a sort of interstitial deposit. I have seen abscesses in it, and I have seen the capsule of the spleen excessively thickened, and almost cartilaginous. It is right to mention that there are subjects in which you have two or three small spleens, besides the large one, each receiving the branch of an artery, and having a returning vein. In some cases there are eight or nine small spleens instead of one large one; I have myself seen such an instance, but those are exceedingly rare instances; however, it is not uncommon to meet with one or two small ones besides the large spleen.

Now I have finished all I have to say on this subject.

LECTURE XL.

ON THE BLOOD VESSELS AND BLOOD.

Blood vessels.—The blood vessels are of an exceedingly elastic nature. If you take a *brachial artery* out of the arm, it will not be half the length it was when in the arm. If you cut it, it shrinks up immediately. Now there is both a benefit and disadvantage arising from this circumstance: the benefit is, that when it is cut it recedes, contracts at the lips, and, in fact, becomes quite closed; and the disadvantage is, that a once cut artery has always a tendency to open again. When what I may call the plug is removed, the wound is again opened, and hæmorrhage ensues.

Structure of Arteries.—Slit an artery open, and what do you see on the inside of it? It is a *membranous tube*; in this particular all the blood vessels agree. Nothing can be imagined better, for the progress of the contents of the vessel, than gliding along this finely polished surface. But if you take an artery and cut it open, you may tear or pull off a very thin pellicle, which is, in some degree, elastic; and I believe this induced Mr. HUNTER to call the internal coat, the *cuticular coat* of an artery. But is this all of the internal coat of an artery? Nay, for certain it is not: you may turn down the internal coat, and what you so turn down is perfectly unyielding, and excessively strong. It is this which causes the artery to have regularly a circular form. Inject an artery as much as you please, and you will never alter the form of it; but inject it too much, and crack it will go. This internal coat must be strong and unyielding. It is so much so, that SCARPA has attributed the formation of every aneurism to the cracking of the internal coat of an artery. Mr. HUNTER found that a force of 90lbs. was necessary to burst the lining of an artery when injected.

Then, surrounding this, there is a reddish substance. Mr. HUNTER says, if you make a section of a healthy artery that is near to the interior, there is a reddish substance, which he believes to be muscular. BICHAT describes it in the same way, but calls it the *arterious tissue,* and will not allow it to be muscular. I do not pretend to say, that Mr. HUNTER's description of an artery is a clear account; I cannot give a clear account of one; but, to be sure, I cannot understand BICHAT's account, any more than any body else. For my own part I think, if I cut up small arteries, that I can see muscular fibres: I see the internal coat in ridges, as if contracted by muscular fibres. But what convinced Mr. HUNTER was experiment: he attended the shambles where animals were bled to death; and, of course, the arteries of those animals would contract themselves, upon the gradual diminishing of the blood in the vessels. He measured the *area* of the arteries, as he found them in animals killed in this manner. He then introduced something into them to intervene, and do away with the effect that had been thus produced; and then he measured the area, they being restored as near to their natural size as their elastic property would admit of. Mr. HUNTER, under these circumstances, found that the aorta had contracted one-twentieth; that arte-

ries corresponding with the celiac artery, had contracted one-sixth; that those corresponding with the size of the radial and ulnar, had contracted one-half their ordinary size; and, in short, that the small arteries were closed altogether. I wonder that any surgeon, who had ever attended an operation, ever doubted that small arteries close themselves: you are taking away the breast of a patient, and out gushes a stream of blood in your face; you put your finger on the part, and stop the flow of blood; the operation is proceeded in; then, when you come to look for the cut arteries, you wash away the blood; you sponge the part from which the stream proceeded, and not a drop of blood more is seen to flow; you may see the artery, but, with its mouth closed, it is a perfect chord. You may twitch the end of an artery that has been thus closed, until you open it again, and then the blood will flow once more.

I have cut through the ulnar and radial arteries to get blood from them, and found that they were perfectly solid. But BICHAT himself allows the greatest degree of contractility to the minute arteries. To the extremities of all the minute arteries, the greatest possible irritability is admitted by all. Now, I say, how strange is this! Where is it to begin? Is it not more rational to suppose, that all arteries are endowed with a contractile power, and that that power increases in proportion as the artery recedes from the heart? I can only say, that such were the opinions of Mr. HUNTER, and they appear to me to be the most rational.

Now, then, for the elastic coat of the artery; this, which we strip off. They speak of it as a condensed substance. What it is, I do not know. I have heard some say, it is more like the *ligamentum nuchæ* than any thing else; but it is certainly very elastic, affording just that resistance to the power of the heart which is necessary for that part. If it had not this quality, there would be such a percussion on the heart, that vitality would be destroyed. And it was left to the *Argus-eyed* Mr. JOHN HUNTER to observe, that where the most elastic power was wanted, there you find it; that there was more elastic power on the convex, than on the concave side of an artery.

Well, but as arteries become minute, they gradually lose their elastic coat, till at last they have none at all; and where there is a contrivance to break the propelling force of the

blood, they lose it altogether: as, for instance, in the brain, there is no elastic coat to be found in the arteries there; there is such a winding of the arteries through bony channels, as that the force of the blood must be broken before it gets to the brain, so that no elastic coat is required. Now these are things worthy of your attention.

Distribution of Arteries.—The next thing I have to observe upon is, the distribution of arteries throughout the body. If an artery sends off a branch, it is generally sent off at a *half right angle.* Some say, that some branches of arteries go off at *right angles;* I do not believe it, but you may, if you like. But then they say, this is where peculiar secretion is wanted, such as in the venal arteries. O, it is not worth making a puzzle about this; the belief was, that arteries would receive the propelling force of the heart most in proportion as they ran parallel to the trunk. That is the physiological belief, and it seems to me to be very well founded.

Then the angles of the extreme vessels are perhaps more various; but I do not think there is much to be said on this subject.

Then another thing: it was formerly taught that arteries were *conical tubes,* from observing the state of the artery when it was taken out. Now if the blood was thrown into a conical tube, I apprehend there would be a continual stoppage presented to it. HALLER says, that if you examine the area of the minute arteries, you will find it to be larger in proportion than that of the aorta. He comes to this conclusion at last, that the area of the branches of the minute arteries is one-third larger than that of the large ones. You know that Mr. HUNTER examined the *carotid,* and other arteries, upon this subject; and he found, that out of several small arteries, the area was larger as it receded from the heart. It is rational to believe this, and therefore I try to enforce it.

Now no arteries are perfectly straight; they are all zig-zag, in some degree. But there are arteries excessively tortuous, such as the *splenic* artery. And what physiological notion are you to form from this? Will this not prevent the extreme force of the blood? O yes; and we believe that the turnings and windings of arteries tend to impede the velocity of the blood. I shall ever believe it as long as I live; you may believe it or not, just as you like; but you see I keep to the old physiology.

Then we come to this, that in observing the branches of arteries, we have presented to our minds what we see taking place in trees, the division and subdivision of the branches from the trunk. But here is a circumstance in which the simile does not hold good, namely, that the branches of the arteries coalesce; that two branches will join, will have their mouths communicate with each other, and become one conjoined tube. I mention that, for the sake of explaining the name that is given to this; if you take the Greek word, it is called the *anastomosing*, or *inosculation* of arteries. Now what will the effect of this be? Why, I say, that when two arteries join in this way, it must tend greatly to impede the force of the blood. There is a current of blood flowing in each, and when they come dashing against each other, they must mutually retard the force; and this I take to be a fair reason why we so seldom meet with anastomoses in the larger arteries; but we have plenty of them in the smaller ones.

You know it is very well understood now, that diseases occur in those inosculations. Now what is the good of the communications? O, that is evident; if one artery did not communicate with another, whenever a trunk were cut off, all the parts to which the branches were distributed would want nourishment. Well, then, you must be aware that the same end is answered by numerous communications that would be answered by one. In the case of the brachial artery being tied, though there are but few vessels as principal channels by which the blood can be circulated to the different parts, yet there are myriads through which it can be sent, and the blood does pass from above to below with a surprising speed. Now these things happen only rarely in the large, but constantly in the small arteries; and the more subtile the injection that is thrown into the small arteries is, the greater number of anastomoses appear. And those anastomoses among the minute vessels seem to retard the flow of the blood, and such retardation was, in all probability, requisite for those changes to take place in the fluid which fits it for the secretion and nourishment of the body.

But how do arteries ultimately end?—Ultimately they end in veins. An artery becomes reflected to the heart, and becomes a vein. This is seen by the microscope; nay, it may be seen by injections. But I do not believe that, till of late years, people were acquainted with the size of the arteries that

terminate in veins. I take what I now show you to be the first preparation that was made of this kind; I never heard of such a one in this town, and it has been made, I should think, about fifty years. The arteries were injected with wax, and the wax is returned by the veins, and it shows the large arteries that receive the wax returning and becoming veins. Now every one knows that this can be done. I remember that Mr. Liston, of Edinburgh, thought it could be done, and I wanted him to make a preparation of the kind, but he did not do it. We have had the veins injected in this place, but the extremities were fat, and gentlemen did not take pains to make preparations of them. People have got lazy since I was young. However, it can be done. Well, then, how do arteries ultimately terminate? In veins, to be sure. You may prove this by quicksilver: throw quicksilver into the arteries, and the whole will return by the veins; so that there is, in reality, to speak in a speculating sort of way, something like a descending series of vessels. But if this were the whole, to what good would the circulation be carried on? There must be some other termination; and so there is, for a fine artery goes off from the part where the vessel is reflected, and terminates by an open mouth; this produces something from the circulating blood, and it is called the *secerning* artery. Can you see those secerning arteries! No. What makes you believe there are those arteries then? Why, when you inject arteries, you see something oozing out from the parts, which you conclude to be the secerning arteries. It is by analogy that you draw this conclusion. How can the parts lay down the atoms that fill up the body, but by open mouths? It is from analogy we say that every artery terminates in a venous extremity, and in a secerning extremity ultimately. But in glandular parts you do see the secerning extremities. Books have been written upon the subject, and therefore I need not dwell upon it. O, be assured of this, that the great mass of blood is speedily circulated; that it is driven from the heart with great velocity, and returns by the veins. It is but a small portion of the current that goes into those parts which are calculated for secretion and nutrition.

Structure of Veins.—Now I come to the veins: and what is the structure of veins? Internally, *membranous tubes*, surrounded with another substance; a very thin membrane it seems to be. You have no pellicle here. Thin as the membrane is,

however, it is stronger than the internal coat of the artery. Mr. HUNTER found that a greater force was necessary to burst the internal coat of the *vena cava* than that of the *aorta*. It is elastic in as great a degree as that of the artery. You might stretch a vein as far as you would stretch an artery, and it would recoil again; but it would require a smaller power to stretch a vein to its utmost, than to stretch an artery to its utmost. Well, the question is, whether they are muscular? O, they have a muscularity, no doubt, suited to their functions; this is a point I have before spoken of. It is absurd for us to suppose that this power of vital contraction operates in the same manner in all parts; it is a power suited to the different functions of the different parts. Then what do the veins do? They congregate together, and form tubes, and all the tubes eventually collect into the *venæ cavæ*. What is the consequence? That if the blood is to go through narrower and narrower channels, it must go with increased velocity. With respect to this, I might say, that if you were to open a small vein the blood would flow slowly from it, and soon coagulate; but you know how it springs from the arm; and then that it gushes from the vein near to the heart with as much celerity as it does from an artery. There is always plenty of room in the veins; the veins are not filled with blood; there is room for the blood to return by the deep-seated veins, or by the superficial veins. The superficial veins have valves to prevent the reflux of the blood. The blood is thrown out from the heart, passes rapidly through the large vessels, tardily through the small ones; it is thrown out through tubes which gradually augment in diameter; it returns through tubes gradually diminishing in diameter; and the current is accelerated as it goes towards the heart, but diminished as it recedes from it.

Some have set themselves about to inquire with what degree of force the heart projects its blood, but it is a very difficult thing to determine, and no great good results from determining it. Dr. HALES's is allowed to be the best method of determining it. He determined it by putting a pipe into the artery of an animal; and I think he makes the force of the heart equal to something more than seventy pounds. Mr. HUNTER found that a force of ninety pounds would burst the aorta; but I am willing to acknowledge that Mr. HUNTER was wrong there, because this was no proof that that would have burst it in the

living body. He had tied the artery, and thereby stopped the circulation, but in the living body the blood is free to move on.

I did not state to you that the elasticity in those parts tended to urge the blood on. It was an omission of mine, but it was not an important one. I may omit things, but I am not willing to tire you with the repetition of facts which I believe you all to be well acquainted with. It does not seem to me important, that you should have any precise knowledge of these facts. It is sufficient to know, that the heart propels the blood forth with great power, and that it returns with less force. Dr. HALES made an experiment which deserves your consideration, because people deny what I tell you, and they quote instances of experiments in hydraulics. Now Dr. HALES made an experiment of this nature; he put a vessel in the artery of a dog, with a column of water capable of making a pressure equal to what he estimated the force of the heart of that animal to be. Therefore there was a force in the vessel equal to the force in the heart. He took care to fill it up, as the water seemed to escape. He then laid open the abdomen of the animal, and, of course, the water oozed from the cut arteries; he divided the intestines opposite to where the mesentery was attached, and the water issued out; he made a cut in the parts opposite the mesentery, dividing arteries of a tolerable size, and the water flowed out, and where he cut a large artery, the water gushed out in a stream. Now there is an experiment with the very tubes themselves, all tending to convince you that this theory, if I may call it so, of the circulation, is a correct one. The view which is generally understood, I believe by the best physiologists, is a known accurate view of the subject.

OF THE QUALITIES OF THE BLOOD.

Now, having done with the description of the circulating vessels, and of the circulation, I have yet to speak of the qualities of the blood, before I can proceed to speak of its nutrition; and as the description of the blood cannot take long, I may as well proceed to speak of it now.

You all know what the blood is; it separates into *serum* and *crassamentum*, but the crassamentum is a compound. You may wash all the *colour* out of it, and leave it a *white cake*. I now show you a preparation of blood that has been thus

washed. You may bleed a person into water at the temperature of 120°; and if you stir it round with a whisk, all the cake will congregate in fibres around the whisk without colour. Well, this was called the *fibrous part* of the blood. Then it may be said, there are *three parts* evidently in the blood; the colour, the cake or *basis*, and the serum. Well, now suppose you examine the serum. If you put it into a cylindrical tube, and immerse the tube in water, at 180° Fahrenheit's thermometer, the fluid serum becomes *solid*, and you may turn it out of the tube in a solid cylindrical piece. If you take it out in this way, and put it into warm water, a good deal of that which was solid will become serum again, but a good deal will still remain solid; and if you examine that which still remains solid, you will find it to be like *white of egg*—albumen. If you immerse once more, that which was left a serum, you will find it *solid* again, and thus it was considered to be *gelatine*. Curious enough it is, that all the French writers state it to be gelatine; and it was not till lately that Dr. Bostock showed it was not gelatine at all; but if you ask the Doctor what it is, I do not believe he can find a name for it; and that is the predicament in which chemists may often be, with regard to many of the animal fluids, I am sure. We have, however, nothing to do with them, but with their sensible properties, they are so modified. At all events, this is something with respect to which that which may be evaporated from it becomes a tremulous solid. Dr. Bostock is inclined to call it *mucosity*; but it is not mucus. Besides this, you have *soda* in the blood; and you may coagulate the serum by *acids*; and this led another doctor to speak of the serum as if it were a sort of *soap*, where the water was kept together by a sort of *alkali*. Likewise, you have a sort of yellow sticky stuff which is in small quantities in the blood, and which they call the *serosity* of the blood. Well, I have nothing to say about it, for it is hardly worth while to tell you what they have said about it; because, the end of it is, that they do not know what it is. And you have *salt*, which is formed sometimes in the round of circulation, and sometimes it is taken in what we eat. We eat a great deal of *sea* salt, and it is curious to see the crystallization of the serum of the blood through a microscope.

Now, having done with the serum, I go to the cake or crassamentum. Well, this is very queer stuff indeed. You may boil it for ever, but you will never make any broth of it, as I

can testify; for I know that a piece of what I now show you was boiled for a very great length of time. It is the same sort of stuff as that which the muscular fibres are composed of, and it has a disposition to become fibrous. If you pour nitric acid on it, you will get a kind of *azote* out of it. It has always been considered as a highly animalized part. That is all I need say about it.

With regard to the *colouring matter*—O, this was said to belong to globules in blood. There are *globules*, as I told you, in the blood, and most wonderfully numerous they are. They are so minute, that I cannot believe any reports of them, when they are said to be examined by the common microscope. HALLER says he saw them as big as peas, through the solar microscope; and I remember that I once looked through a solar microscope which was carried about the country by a *Dr. Catterfelto*, and I saw them as big as peas too. I am ready to swear it, but it is of no use, for no body would believe me, if I were to do so. They are said to be about the three thousand and two hundredth part of an inch in diameter, but that is of no great consequence.

Well, you see those things are of different specific gravities. The serum is a little heavier than water; it is, as 1050 to 1000, perhaps less than that; it may be as 1025 to 1000. I really forget, but it is not important. It is undoubted, that the crassamentum is heavier than the serum, for it sinks; and it is evident that the globules are heavier than both, for they subside at the bottom of the cake, when the cake is slow in forming, as is the case in inflammatory blood.

But as to those globules, are they *red?* No, they are not indeed. Now that is a queer thing; and it was only found out of late by Dr. YOUNG. He found out that the red colour of this blood will rise in water. It seems to have an affinity to water. If you put a piece of flesh into a bucket of water, the blood will rise up through the water and make it a transparent liquor, but the globules will fall to the bottom. Dr. YOUNG saw this; he saw that the globules sunk, and that the blood rose. Then what are we to think of it? Why, only that the colouring matter of the blood has a strong affinity to those globules; and the way to get to them is to wash the clot well; if you do that in water, the globules will subside. BRANDE has shown that the colouring of blood is susceptible of colouring other matters.

Now, with regard to those globules and the colouring matter: when they subside, upon chemical analysis, they are found to be like the rest of the blood. But they are found to contain *iron*, and that is a very curious thing. Is it not curious too, that BRANDE of late should deny this? But it is asserted by BERZELIUS, and by a great many others indeed, who have made experiments, and got iron in abundance from globules. This may perhaps be one of the causes of their greater specific gravity.

Now that is all I have to say on the subject, but I must add, that it is a very satisfactory examination, as it shows you that the blood contains the rudiments of all the body. What do you find that the body is composed of, but of *fibres?* Some insoluble, and some soluble. All this is to be found in the blood. You have also something in the blood which is susceptible of considerable mutations of colour, and you see such mutations take place throughout all the body. I say, we know but little, but that there is a sort of satisfactory conclusion come to from what we do know. And having gone so far, we shall not proceed further.

ON THE PHYSIOLOGY OF THE MUSCULAR SYSTEM.

As to the structure of the muscular and nervous system, those parts must be spoken of, because the physiology of the Course would be incomplete without it.

With regard to muscles, what we know of the *structure* of muscles is, that their ultimate structure is like the larger structure. The only question is, whether their threads are continued from end to end of the muscle? For my own part, I do not believe it. What a curious thing it would be if we could see a muscular fibre of the whole length of the human *sartorius*) Well, but muscles are *vascular*? they are liberally supplied with nerves, and that is all.

Function.—But their function—is their function not amazing? Is it not amazing that a little part of the flesh, such as may be found in the arm, would be torn by a few pounds weight hung to it in the dead body, and yet that it should be capable of lifting up and sustaining some hundreds of pounds in the living? The question is, in what is that power? Is it in the fleshy structure itself? Even the flesh is easily lacerated when in the living subject. Has no one of you known of the finger of a person being torn off

in a mill, the whole of it being lacerated and taken away? I have known many such instances myself. Now as to the *power* of the muscle; some persons say it is a chemical change that takes place, and some say it is a peculiar property of the muscle to contract; but all this I hold to be downright nonsense. I shall declare my opinion to you to be, and which is just JOHN HUNTER'S, that *life* is something superadded to the visible structure of bodies, that it builds up structures; that it is something superadded to it, and that it produces its peculiar phenomena, the vital phenomena, in the structure in which it appears. That it is something superadded to the structure, as magnetism is to iron, or as electricity may be to the different bodies on which it is to act. As magnetism is to iron: iron is iron, but let magnetism be added to it, and the particles of iron will contract immediately. Pith balls, are pith-balls, and if placed on the floor will never stir; but let them be on the floor when magnetism is there, and they will dance the hay and frisk about in every corner of the room. O, the most sturdy of those gentlemen whom I have known to deny this doctrine were obliged to admit, that there was a subtile kind of fluid engaged in producing divers fine phenomena. I say that that admission was made by the most sturdy champion I ever heard of in denying that electricity was somthing distinct, and something superadded to visible objects. But how this could be doubted, how it could be questioned that there was some subtile fluid permeating all nature and producing change, is to me quite astonishing.—Well then, I think we may believe that there is some vital energy inherent in the muscles; and I say the action of them is very like the action of electricity on other objects: and that is all I need say on the subject.

ON THE PHYSIOLOGY OF THE NERVOUS SYSTEM.

From one sort of blood, circulated throughout the body, is such a variety of structures not only prepared as we find to exist in the body, but those structures are endowed, according to nature, with a vitality which qualifies them for their functions.

I spoke of muscles, and now I go to speak of *nerves*.

Structure.—What do we know of the structure of nerves? Next to nothing. Ultimately, the body of nerves is fibrous.

Atoms of matter are laid down, according to some arrangement; and those atoms, so laid down, cohere and form fibres I say there can be no question but that the body must be either fibrous, laminous, or concrete. It is the first sentence in HALLER's physiology. I do not undertake to explain this just now, for I have talked of it before; but if you use your eyes, you see nothing but fibres, or plates of this arrangement. It is fair to suppose that the ultimate arrangement may be similar to the visible arrangement. What do they say about the fibres? That they are an ultimate mass. And what do they say about nerves? That they are fibrous. FONTANA calls them *waving cylinders*; they are transparent, and are surrounded by vascular matter, as that which surrounds the brain; they are again surrounded by a grosser protecting membrane, analogous to the dura mater. Well, then, when a nerve is divided, how does it appear? Are the fibres blended together in the nerve? No, every one of them appears to be distinct,and every nervous fibre has a communication with its origin, either directly in the brain, or in the *medulla spinalis*, save and except where a *ganglion* is formed in the nerves. No one, I apprehend, can unravel a ganglion; the fibres are either blended together there, or are inexplicably interwoven. And when there is a plexus of nerves, then there is a great complication. Bating those things, it is very possible to take a nerve from the toe, to follow it according to its thread, and to trace it to its very origin in the medulla spinalis, or in the brain. All this is but very little knowledge, but very little knowledge indeed, and perfectly insufficient to enable you to account for the functions of the organs—perfectly insufficient. It is most marvellous, no doubt, the celerity with which vital actions are propagated through the nerves. The first proposition, and it was a perplexing proposition indeed, with regard to the physiology of the nervous system is, that every thing is felt in the brain, and that volition proceeds from that organ. To persuade a man, that when he felt this piece of chalk, for instance, the perception was in his brain, was a very difficult matter. But if the nerves of his arm were divided, no sensation could be imparted by the contact of the substance of the hand; neither, if the nerves were divided, could any will be conveyed to the muscles.

Well, now, all this part of physiology, you know, is no longer to be argued upon; it is proved by direct experiment.

It is proved that the nerves are of two kinds, such as communicate transmitted pressure to the brain productive of sensation, and such as transmit volition to the nerves. The experiments to which I allude, are those of MAGENDIE. MAGENDIE slits up the *theca vertebralis*. By frequent practice, he has got a handiness in performing operations on living animals. He will take them, when their bones are soft, and lay the theca vertebralis bare from end to end. Immediately afterwards, a fluid will form in the theca vertebralis, and there is a distention of the theca vertebralis. He slits open the theca vertebralis. He will then divide the nerves that come from the posterior pillars of the medulla spinalis, and thus he deprives the animal of sensation. The animal will move about, but you may burn him, or do any thing with him you like, but he will not feel it. You may hack and cut him as you please, still he will not feel it. And contrariwise, if you divide the front nerves, all voluntary motions are put a stop to, but feeling remains. Now, you know that a great deal of argument is rendered unnecessary, because it is acknowledged that the *brain* is the organ by which we possess both feeling and volition—feeling and all its consequent faculties. And I say it is most wonderful, the celerity with which this is done: I put it thus—a man might be standing by the fire, and have a cloth shoe on his foot: suppose, also, that there was a bucket of water standing by the side of him, and that a hot coal fell upon his cloth shoe, and set it on fire; in an instant he plunges his foot in the water; it is done as quick as thought; and yet in that space of time an intimation must be given to him that his foot is burning from the shoe being on fire, and a direction to the muscles to act. Now how can you suppose that this can be done? I say, and say again, that HALLER's notion of there being a subtile fluid inherent in the nerves was a very sensible notion, according to his time. He says there must be some subtile fluid in them, for you cannot suppose that volition is propagated throughout the whole of the nerves with this celerity. But we are at no loss, according to the philosophy of our time, to know how it happens. There is something prompt to move, in consequence of external impressions made upon nerves, or internal volition excited. There is a distinct set of chords for the conveyance of sensation, and another distinct set of chords for the conveyance of volition. All this you must admit; and you may speculate upon it as much as you please,

but I say this is a proof to me, that there is some subtile fluid mixed up with the frame, capable of acting with amazing celerity, and in a manner capable of adapting parts for their different structures. It is not the impression—and that was the old physiology—that is propagated along the nerves, there must be vital action induced. But temporary suspension of actions occurs both in the muscles and in the nerves; both go to sleep and recruit themselves. A man may sleep with his eyes open, and a friend comes into the room with a lighted candle in his hand; the object of that friend, and his candle too, are both impressed on the *retina*—both depicted on the retina, but the sleeper sees them not. His "eye is open, but its senses are shut;" rouse the slumberer, put him into a state in which the actions of the nerves and muscles will take place, and he will immediately exclaim—God bless me, what are you doing here? What are you doing here at this time of night? Well, I say this is vital action; and Dr. Darwin has shown how those vital actions occur, and how perceptions take place without visible objects being before you—I mean the vital actions of the retina. You know that a man sees a *ghost*; why, now to suppose that a ghost was a palpable thing—that it was as sensible to feeling as to sight, is to suppose a downright absurdity; but still people may see objects before them which are not before them, and may hear sounds which do not exist. You must consider the subject for yourselves; and I believe, if you do consider the subject for yourselves, you will be as confident as I am, that vital actions must take place, in order to affect the brain and produce perception, and that the whole must operate upon the vital actions, so as to cause the motions of the obedient muscles.

I say, the physiology of the nervous system, the physiology of secretion, the physiology of muscular action—all tend to convince my mind that Mr. Hunter's notions of life are true, and that there is some subtile material invisibly commixed with the visible fabric of the body, operating on each part, so as to produce the phenomena of the vital functions.

The deduction that I draw from the whole of the productions of modern anatomy, is a very curious and a very simple one; we know no more, after all, than what common sense dictates; we know no more than what a man of sense, in the first ages, would have believed, upon considering the phenomena of life, which is, that there is in living beings an or-

ganized structure invisible in he body; and in the common material a principle of life pervading every part—a sentient and rational faculty connected with the brain, but each apparently distinct from the other, though they are all intimately connected, and operate one with the other. Here I go a step beyond JOHN HUNTER; I believe sensation to be as distinct from life, as I do life to be distinct from the materials of our body. I say, that is a step beyond JOHN HUNTER; but it is an opinion that the most intelligent of mankind have always entertained; it is no new opinion. Have you not life without sensation? Cannot I ask that question? Do you believe that *vegetables* feel? There are some people who have a propensity to take things up by the wrong ends; and I say, if they had a poker presented to them with one of its ends *red hot*, they would lay hold of the red hot part rather than the other end, and burn their fingers of course. Dr. DARWIN has done this; he begins to certify things, deducing inferences from the conduct of man. He says, man performs certain actions, becausehe *reasons*; the refore he infers, that *brutes* do the same thing, because they reason. This is common belief. Now I really do not know how people can believe it. I know not what kind of faith they possess, I am sure; but I do not believe that *instinct* is *reason*. I believe that it is *a blind impulse*, inducing actions far superior, in their effect, to what reason does; but still it is a blind impulse. A *hen* will sit as vigilantly upon so many pieces of chalk as she would upon eggs; she will peck the ends of the chalk, at the period when the young chick ought to come out of the shell; and that is instinctive, but not reason. Dr. DARWIN himself produces an instance which is a direct contradiction to his doctrine: he tells you that his kitten, observing a little water upon the marble slab in his room, went to it and scraped with its foot. Now that was instinct, but not reason; she could never have reasonably supposed that the water was urine, and that she could have covered it by scraping with her foot on the Doctor's marble slab. But it is natural you know for foolish people always to judge of others by themselves, and they mistake by so doing. You may judge in this way, if you please; but those who form this notion of reason, must grant the highest degree of reason to the vilest and lowest of animals.

Well, I say, can you believe that life may exist without rea-

son? I do believe it. I do not believe that vegetables feel, or that the lower order of animals feel in the degree in which we do; and as far as we know, all feeling is placed in one point in animals, which is a little *tubercle* at the top of the medulla spinalis. As soon as this tubercle begins to form, so soon we begin to find something sentient; and as this knob begins to grow, and becomes complex, so do animals have additional faculties and powers.

Now that is all I have to say upon the subject; for how any man, or set of men, can possibly believe that those faculties are the result of mere replications of the brain—for such the convolutions are—is what I cannot understand. I have no faith of that kind. The brain must be allowed to be the organ by which a diversity of perception is prone to act. We have divers perceptions, and divers faculties; but it is said that those are only the simple replications of the brain, which form the convolutions. Well, that is very strange. And how is it argued? Why, we have organs by which we see and hear, and smell and taste—giving us diversity of feelings: now, the eye does not see: the reason of the organization of the eye, you understand perfectly well, is to transmit the rays of light arranged in the same manner as they present themselves from the different visible bodies. The organization of the ear you understand. But the eye does not see, nor the ear feel: the notions we form must be dependent upon the peculiar properties of that which is perceptive; and it is wonderful: for what can we make of it, if we consider it intently? What is there around us, but matter in different states of motion and of rest? As subtile matter is reflected from objects, it is arranged on the eye in the same order in which it was produced from the surrounding objects. Well, there is the magnetism of the eye. Vibrations take place, and the agitation of the fluids in the labyrinth of the ear produces the sound. O, but it comes to this, that the vital actions of the nerves take place, and those are propagated to the brain; hence are the different sensations produced. All you can make of it is, a sort of vibratory motion; and how from one set of vibrations we can see, and from another we can smell, and from a third we can hear, is certainly very curious; but they are all vibrations, or tremors of the nerves. I say all this I can only attribute to the very curious properties of that which is per-

ceptive; and that that has divers faculties superadded, capable of being acted upon by the brain, a subject which no one can consider without wonder. But I cannot believe that the faculties are occasioned by the magnetism of the brain. Now here the subject becomes *metaphysical.* I do not want to enter into metaphysics in this course of lectures, but I believe if we were to go into metaphysics you would be of opinion, as I am, that there is an unity of all the senses with the brain. They put the organs all in different places; whereas, I ask, how can you account for it? Many of the actions which are performed are the effects of compound motives. There is no answer that can be given, but that it must be done by sub-committees of the different organs—by the board of control. Now I say, pho, pho, if you go to the board of control I am contented. However, every opinion has its day; but the opinions which I am in the habit of teaching here have endured from the first ages to the present time; therefore I think they will stand on; I think that all wonders—that all novelties of the day will decline, but that those opinions will flourish.

Morbid Anatomy.—Now we go to something more to the purpose, and that is the morbid anatomy of the nervous system. Here I first speak of morbid appearances of the dura mater; and yet I say, with regard to morbid anatomy, there is nothing in it that seems to me to be particularly interesting. The same diseases occur in other parts of the body. The dura mater will be *thickened;* it has an under secreting surface, and the secretions from that surface may be altered in consequence of inflammation. Here you will see actually a *fungous* membrane formed from a gelatinous substance that has been deposited. It is in some degree organized. It is what you see in the *larynx,* in the case of *croup.* Well, the dura mater may be thickened, it may adhere to the surface of the brain; and here is a specimen, a very curious specimen it is, in which the dura mater has been separated from the *arachnoid membrane,* to show that it is distinct. I know the history of this case very well. Here are specimens of growths from the under surface of the dura mater. The dura mater may be ossified. And yet, I say, these things are what any body would believe without witnessing the facts; and therefore what appears to me to be of most consequence to us is, that we should consider that the dura mater is the periosteum of the internal table of the skull; and that if there was any thorough

disease of the cranium, you might as rationally expect the cranium to adhere to the outer layers, as the dura mater to adhere to the under layers, or inner table of the skull. And if the dura mater separates from the under table of the skull, what will happen? There will be fluid collected between it and the bone; irritation between it and that part of the bone will be kept up; gelatinous matter will be deposited; the deposited matter will become organized; and then, if this irritation be continued, as you know how prone disease is to be propagated along continuing surfaces, the dura mater may become generally affected. I have seen excessive disease of the dura mater thus take place, the origin of it being local, and simply in consequence of its being detached from a diseased part of the bone. To show you what I mean, I may tell you what happened to a man in this hospital: he had a disease in the bones of his head; it was supposed to be venereal; he was salivated; he came here in a very reduced state; ultimately he had symptoms of cerebral irritation, and he died. Now upon taking off the top of the skull it was found that there was a part of the dura mater separated from the bone, and a sort of fungus growing from it. This was from one hemisphere. But upon turning the skull round, and cutting it fully open, we found upon the other hemisphere, under cover of the dura mater, a large quantity of good tenacious pus, which extended every where upon the hemisphere, and adhered to the under layer of the dura mater.

Now it is of great consequence that you should trepan in these circumstances. Why should you not take away a piece of the bone? It may be dead; the patient cannot feel the operation, and you give a discharge to any pus that may be under the dura mater, thereby exempting it from irritation. But under these circumstances, the dura mater may throw out a fungus; the fungus may come in contact with the under table of the bone, and what then? The bone may be absorbed; the fungus makes its way by progression through the bone, and rises up like a *mushroom* on the man's head, where you may have a violent pulsation. This was called a *cerebral hernia*; and Monsieur Louis, in the fifth volume of the Memoirs of the French Academy of Surgery, has described this case. I now show you a preparation of the case. Mons. Louis, when he described the case, told the proper treatment of it in a very few words: he says, in this case it is your duty

to remove the bony circle which surrounds the basis of the fungous growth, and afterwards to destroy the fungus by suitable remedies. But I say to you, if you do remove the circle of bone which surrounds the basis of the fungus, in all probability the fungus will cease to grow; for what occasions the fungus to grow, but irritation? Remove the cause of irritation, and the growth of the fungus will cease. There is one famous case of this kind on record, which was attended with complete success, described by HALLER; it was the case of a German officer. You know that the dura mater may undergo disease distinct from the brain, or the brain disease distinct from the dura mater.

I now come to the membrane of the brain, and here I may tell you, that you will find the *arachnoid membrane* white and thick; and that you will have water effused, and jelly effused, connecting it with the true *pia mater*. But all those appearances, which were spoken of during the demonstrations and the lectures, cannot well be exhibited in preparations. The morbid appearances fade, or the fluid escapes, or the jelly thickens, so that they are not exhibitable in preparations put up in spirits.

Then I come to the substance of the brain itself; and what do we here meet with? Why, we meet with tubercles; not unfrequently tubercles in the cineritious and medullary part of the brain. Of these tubercles Dr. BAILLIE speaks, and he has said some of them are very small. I show you preparations of strings of them. When cut asunder, they seem to be a little yellow, as if they were about to suppurate. Now I have very frequently found tubercles in people who died of epilepsy. But then, would you say that this was the cause of epilepsy? I should say, rather the effect of that cerebral irritation which produced the epileptic fits. Can epilepsy take place without *organic disease* of the brain? that is the question. The answer is, unquestionably. It is answered unquestionably by any man of extensive observation, but few people have that extensive observation which would qualify them to assert this opinion. I refer you to a case where a man died of epilepsy, which was published, I think, by a Dr. FRASER, and where Sir ASTLEY COOPER was employed to examine the brain. He did examine it with the utmost attention, and he found no organic disease of the brain whatever.

Every variety of nervous affection does occur without any

organic disease. But when people have epilepsy, without organic disease, they generally recover, or may recover. If the irritation which occasioned the epilepsy continues for a length of time, naturally enough, faulty vascular actions will be induced, so that in people who have been long subject to epileptic fits you generally find tubercles. But the subject is a most momentous and important one, and very much deserving of your attention. For my own part, I shall affirm that every kind of nervous malady may occur in the brain, as functional disorder, and not as organic disease. This is most important, because it leads to the consideration of madness? and why should they bleed, and purge, and the devil knows what, poor people who are afflicted with madness, as if they have all a disease of the organ, the brain itself? Now here I quote Morgagni; he has got a long list of diseases that he has published, where tubercles were found, and among that list are cases of persons who were mad from too hard a brain, from too soft a brain, from a fungus in the brain, from a plexus occurring in the brain, and so on; but here comes the epigram, the concluding sentence, "but all these morbid appearances have I seen without any appearance of tubercles at all, and I have seen tubercles where there have been no such morbid appearances." There is a body of evidence for you! Madness is no cause of these tubercles at all. I say the object we should bind our attention to is, to settle nervous inquietude; to tranquilize the vital actions of the cerebrum; to cut off any cause of irritation affecting it; and I take the putting into order the digestive organs to be one grand point towards accomplishing this end. I tell you honestly what I think is the cause of the complicated madness of the human race; it is their gormandizing and stuffing, and stimulating those organs to an excess, thereby producing nervous disorder and irritation. The state of their minds is another grand cause: the fidgetting and discontenting yourselves about that which cannot be helped; passions of all kinds—malignant passions, and worldly cares, pressing upon the mind, disturb the cerebral action, and do a great deal of harm. There is something of moral treatment in madness: you are to quiet the mind as well as the alimentary organs, and not to keep bleeding and blistering, as if you were to cure all the diseased structure by such measures. Of course, preventing too powerful an impulse of the blood is important, but every thing is to be conducted in moderation.

Now, what do you meet with here? You sometimes find the substance of the cerebrum itself undergoing a morbid deterioration, I have seen it like thick cream—not at all having its natural smoothness of surface, when divided. And I remember two apoplectic patients who were examined at this hospital, about the same time, where the morbid state of the medullary matter of the hemispheres of the brain was found thickened throughout the whole length of it: but I remember too—and that was a thing that particularly fixed itself on my mind, that one of those chaps was the happiest mortal alive; though he had those fits, still he thought he would do very well, and for every body who approached him he had a smile. The other was the most uneasy and desponding dog that could live; though he might be exempt from the fits, yet he had no hope; he felt sure he would die, and nothing was presented to him but what was a sort of misery to his mind. Well, that was a difference of character, I suppose, in the two individuals, but the morbid appearances were the same.

Abscesses.—Then you have abscesses in the brain. And when an abscess is formed in the brain, it is like an abscess in any other part of the body. Here is one, the sides of which are flocculent, and what I may call villous. Here is a preparation of one, which, I suppose, held an ounce and a half of matter; this was in the front hemisphere of the brain, and this case is a case I wish to tell you of; for, in considering the practice of surgery, it is a very important thing that you should understand, that there is a considerable sympathy between the contained and the containing parts; this man had his scalp torn off by a cart-wheel, which passed against his head; torn off and smashed all to pieces; it could not be replaced. He was not stunned: no other harm was done to him, than that the scalp was torn off. He was brought to this hospital, and here he lived for a considerable time. The *pericranium* having been stripped off the bone, there was a death of the bone, and the exfoliation of the bone was a very tedious process. There was not much wrong with him; and, to tell you the truth, he was not much attended to; he was left by the dresser, and he used to dress the injured part himself. I was going round the ward one morning, and I saw him standing at the end, staring in an extraordinary manner; I went up to him, and said, what is the matter with this man? I felt his pulse; it was very low,

and his hands were cold. I got into a sort of rage with the nurse, and desired to know why she had not been more attentive to the man. She said, he did not say he was ill, Sir. Why, he might not have said he was ill; but I am quite sure, I observed, that he could not have eat his food of late. No, he certainly did not eat his food well, but he had not made any complaints. However, he died very shortly afterwards. Now here, from mere irritation on the outer part of the head, an abscess formed in the brain; and when abscesses form there, people go off generally in the way I have told you. They have no warning, they have a failure of the vital powers, and they sink very speedily. O, there are many similar cases on record; but here I call your attention to it, as a case indicating the sympathy that exists between the contents of the cranium and the external parts.

You may have abscess in the medullary matter of the brain. And what does all this show? Does it not show that the organization of every part of the body is precisely the same, since every part of the body is capable of building up the same sort of disease, of converting the healthy structure into diseased structure? How is it done? Why, by the vessels arranging deposited matter from the secretions of those vessels being wrong, instead of preparing that which they ought to prepare.

Apoplexy.—Well, then, again you have *apoplectic cells.* Here are specimens of plenty of them. Blood vessels burst upon the brain; the blood gets effused; it clots, and forms cells in the brain, and that is called *sanguineous apoplexy.* Now with regard to sanguineous apoplexy, Dr. Baillie has said, you generally see some alteration in the natural structure of the principal arteries; that they are white, in some degree ossified, and that in this state they are more liable to burst than in the healthy state; that there is a sort of diseased structure in them, which renders them the more liable to burst. Well, now, I say that that is the morbid action of the blood vessels; that it proceeds from the too forcible action of the blood, and that therefore all this is likely to be observed in the principal arteries, where they go into the brain. Here are specimens of arteries which having penetrated the substance of the brain, ossified. This was the brain of a man who had been long ill, and had had very strong nervous symptoms. A surgeon being employed to examine the brain, he notched his knives, and he

had to go on haggling and scratching through the ossified arteries which ran through the substance of the brain. Well, of course the arteries of the brain are liable to the same sort of disease which the other arteries of the body are liable to; and of course diseased arteries are more liable to burst by a forcible action of the blood than sound arteries. Sanguineous apoplexy also is more common to the old than to the young, where probably some disease of the arteries has taken place, owing to the too forcible action of the blood vessels: But the young do likewise have sanguineous apoplexy, in consequence of diseased blood vessels in the brain. I have seen it many times, but still it is not common. The young seem to me to be more subject to nervous apoplexy than to apoplexia sanguinea.

Now what is to be done in these cases? O, to guard against the forcible action of the heart and arteries; that state of irritation which is the cause of inherent vascular action must be your object. You say, however, that a vessel gives way, and blood is shed; what does it do? Why it acts as pressure. It generally falls on some part of the body; but a very considerable quantity of blood may be shed, and yet the patient may recover. I may relate the case of a great effusion of blood, where the patient did recover; it was the case of Mr. Baddeley, the player. He fell down in a fit; there was a great effusion of blood; but he recovered, played again on the stage, and then he died in a second fit of apoplexy. I think you may easily know the state of an individual who is liable to sanguineous apoplexy, if you will be observant. To convey to you the state of my mind on this subject, I will tell you the case of an old man, with whom I was acquainted, who could get no better doctor to attend him than myself; he could not afford to give fees. He was a man of a vigorous constitution; he lived nearly to the age of seventy, without having had the slightest illness. He then had occasionally a fit of the gout, and subsequently he had a slight attack of apoplexy. The first fit he had affected his speech, and left him in a state unable to articulate. After some years he had another fit, and this affected one side of his face, but not more. Then again he had a third fit, which left him completely without any motion in one side of the body. It was one of the larger vessels that had burst. He was a man of great vigour, both of body and mind, but when I first knew him, it was just as he was recovering

from this third fit. He used sometimes to forget all about every thing that had transpired, and was quite unconscious of all around him. I admonished him about his diet, and about keeping his bowels regular; and I made him an issue in the neck, and his recovery was rapid beyond belief. He soon had all his wits about him; he became able to go forth and walk in his garden, to recount again with accuracy all the adventures and incidents of his past life, and he was very comfortable. As long as I knew him, he had a fit of gout in the winter; and every year, in the summer, he had a violent throbbing in the vessels of his head; but he kept his bowels open, and went on with tolerable comfort for five or six years. But I remember one summer time, for he lived in the country, on going into the house, I saw that he looked a little confused; and his wife said to me, I do not know what is the matter with him, I think it may be his stomach. I saw the temporal artery beating violently; I said, bring me a basin, and on puncturing the temporal artery, I got about fourteen ounces of blood from it, and closed the wound. Then I said to him, well sir, how do you find yourself now? says he, as well as ever I found myself in my life. He got up, threw himself down into his chair with a thump on his bottom; now, said he, before you took away that blood I was obliged to sit down very cautiously, otherwise it would have jarred my head, but now you see what I can do. I say, I think you will have forewarnings enough if you are observant. Coughing hurts the head most excessively where there is a tendency to inflammatory action, and the throbbing of the vessels seems to cause them all to go into a sort of sanguineous apoplexy.

As to the nervous apoplexy, you know that is very curious; but you have cases of nervous apoplexy. It cannot be said that that is occasioned so much from pressure, for no pressure seems to continue, and yet the effects go off. However, this is a subject of discussion which is foreign to our present purpose. I will tell you, that a little pressure will produce apoplectic symptoms in one person, and will not produce them in another preson; and that sometimes a person from pressure will be liable to torpor in the brain—that torpor will go off, the patient recover, and again have the same symptoms from the same degree of pressure.

Well, then, what more have I to say? O, there is *hydrocephalus*—effusion of fluid into the ventricles of the brain. But

I never thought it worth while to put up a preparation of this kind, for really there is nothing to be seen, but a great quantity of water in the ventricles. You have also disease of the *plexus choroides*; and it is curious, that if you have a disease in the plexus choroides on one side, you generally have the disease on the other side. This is a circumstance that Dr. BAILLIE has remarked, and here is an example of it.

Now I do not know that I have more to say relative to the facts which we observe in dissecting the brain, when it undergoes organic disease.

LECTURE XLI.

ON THE NERVES WHICH PASS THROUGH THE BASE OF THE CRANIUM.

The true notions of the system of nerves are these: that the nerves are parts produced in those portions of the body where we find them; that they are formed and maintained by the part of the vessels on which they lie; and simply, that they are connected with certain sources or centres, as we call them, of nervous action, connected with the spinal chord, the brain, and large ganglia. You know it is no extraordinary thing for a child to be born without brains, and yet for that child to have a regular nervous system and action, which is a demonstration of what I state.

Infra Orbital Nerve.—The infra orbital nerve has been made memorable from an operation performed upon it: Dr. HAIGHTON had a relation afflicted with tic douloureux, and he advised an operation; and a person having the tic douloureux will submit to any operation, for the pain is most excruciating. The Doctor advised the nerve to be divided, and that was done. Now this has led to the cutting of the nerve for that complaint; and, as I generally speak what I think, I must say, I wonder it has not entered into the wise head of some great medical man to cut the nerves of a man's leg for the purpose of curing the gout in his toe, for they are both eqnally diseases of the

constitution. There was that poor man, Dr. PEMBERTON, who had his face cut to such a degree that he could not feel the razor upon it when any person was shaving him, and yet I have seen him write prescriptions while suffering the most horrible pain. Now these things are wide subjects for consideration, and you may consider them till you are mad ; and, egad, if you were as mad as I am you would be mad indeed. There are cerebral actions of the nerves, and if you cut off the extreme parts, you do but little good, as the disease may remain behind the parts cut off ; although cutting is justifiable in some cases, I admit, with all my notions as to the absurdity of it, as I shall point out to you by and by.

Communication of Nerves.—Nerves do communicate with each other ; but they are, by Mr. BELL, supposed to serve one especial purpose ; for Mr. BELL believes that this *facial* nerve is not a nerve producing common sensation, but a nerve producing those actions which indicate the affections of the mind —those actions which we call expression. This he founded on an experiment ; because, he says, if he divides the facial nerve of an animal, it does not seem to feel it so severely as if he were to divide the fifth pair. He says he once cut the facial nerve of a terrier, and after he had done that, engaged it in combat with another dog ; and the terrier, being of a pugnacious nature, grinned and growled most terribly with one half of his face, but without the other half being moved at all.

Nerves passing through Muscles.—As to nerves running through muscles, that is a common trick enough with them. You do not see arteries doing so—at least, I do not see them doing so ; the arteries lie in the crevices between the muscles, but if a muscle lies in the way of a nerve, it goes right through it, and it does not seem to impair the action of the nerve at all.

Eighth Pair.—This is an extremely important nerve. We are authorized to believe that parts of nerves may sympathize with one another ; and if that be so, you have to consider whether *pharyngeal* and *laryngeal* irritation will not affect the stomach. Now, any man who doubts that, I think, does not seem to have studied his profession sufficiently, or paid quite enough of attention to the eighth pair. It is curious to know how many coughs arise from this pharyngeal irritation. I cannot tell you all that is in my mind about that, but I shall tell you one case, and you know I am always in the habit of telling a case

most likely to strike your minds, and to make such an impression as to remain there: there was a friend of mine very much subject to this choking sort of cough; and if he took any mustard to his dinner, or anything that happened to tickle his pharynx, or any part about his epiglottis, it would occasion such a coughing as almost to throw him into a fit; and he was so ill with it, that he never went out to dine, but always dined at home for a great many years. Now it appears to me that one nerve does certainly sympathize with another. If you put your finger into your throat, it will cause you to cough till your eyes almost start out of your head. Well, this man was supposed to be dying of an hepatic affection; a thorough break up: he was advanced in life; his liver was all wrong, and he was dying—at least, so his doctor said; but as he was dying, and as a drowning man will always catch at a straw, he expressed a particular desire to see me, to know if I could order anything that would do him good. I was sent for, and I went into the country to see him: I said to him, upon my life, sir, I should be most happy if I could suggest anything that was likely to do you good, but I am very ignorant of the medical profession, and if I were not, I really do not think I could suggest more judicious treatment than has been prescribed; but, sir, I shall give you a lecture on your *diet*, in the presence of your medical man. I knew the man before; he prided himself on his virtue; he drank no wine, but he did that which, for anything I know, is as bad—he ate most preposterously—was a perfect glutton. Now, said I, I know you like milk; I shall give you a cup of milk to breakfast, and you may put a piece of bread in it, but not one drop of the milk displaced by the bread must be replaced in the cup; you may take a new laid egg to dinner, and a piece of bread and butter; about four o'clock you may drink some soda-water, and then have done for the day. Continue that for some time, taking it at the distance of every six hours in the day. He did so, and, egad, the man got well; he seemed to have grown young again; he got quite active, and really it was quite astonishing to see him. About three months afterwards, he asked me to dine with him; I went, and I saw him just at his old trick, stuffing most enormous quantities of food into his mouth. After dinner, we walked in the garden; he was a merchant; and in the course of our walk, I said to him, "Pray, sir, what would you think of a man who from nothing had raised a small

capital, and who might, if he chose to go on, increase that into an immense fortune, but who did not choose to go on, and squandered that capital away; what would you think of him?" "Why," said he, "I would say he was a d——d fool;" then, said I, "what one may think of wealth, another may think of health, and 'thou art the man.'" I say health is like wealth, extremely difficult to get a little of; but when you have got it, if you take care of it, it will increase, and increase too with compound interest; but it is the nature of man that he will not do well unless he is compelled; and I believe you will find this to be the lesson of human life; if people will not take care of health, and do well from inclination, they will be obliged to do it from compulsion; but there are those who will even defy necessity, and those people go to the devil, of course.

SPINAL NERVES.

The nerves coming up from the two pillars of the *medulla spinalis*, was a subject that attracted Mr. Bell's attention; for he had got into his head, from examining the mechanism of the nervous system, that they must have different functions; and I told you the experiment he made. I remember, as an argument why the nerves of the medulla spinalis were distinct, yet joined together, he told me that he tried to divide the *fasciculi* going from one of the columns, without dividing the other, but that he could find no distinct result from the experiment; and the cruelty seemed to annoy him, and therefore he was deterred from pursuing it. But I do believe this same Mr. Bell, from his own cogitations and experiments, was the very first person who has led to broach an opinion, which now seems to have gained ground among the profession; an opinion which is absolutely verified by an experiment of Magendie. The first relation of the experiment was this; somebody had sent Magendie some puppies, and one morning after breakfast he did not know exactly what to do with himself, and therefore he laid open the *cervical* portion of the medulla spinalis, and he cut the posterior fasciculi of the nerves going off from it. The animal then seemed to have no feeling in the parts to which the nerves were distributed, but yet it moved its head. Well, a long time elapsed before he could manage the converse of the experiment, but at last he

contrived to divide the anterior column of the nerves in the loins of a puppy also, and that dog seemed to have sensation in his foot, but yet he could not move it. Well, he has contrived to make a great many experiments of this kind, which those who saw him performing here, were forced to acknowledge the truth of; and it is curious to see how immediately the theca vertebralis becomes distended with fluid. Then when it is distended with fluid he slits that up, and divides the posterior or anterior column, which he pleases. Say he divides the postreior column; why the animal loses all feeling, but it can command its muscles: say he divides the other; why the animal has no command of its muscles at all and is perfectly deprived of motion: the foot may be cut, or the toe taken off, and yet the animal not be conscious of it, but the animal can give a kick, and a pretty hard kick too; but it is a kick by the muscles, without volition. Well, now, this experiment, you see, is one that pleases me, because it seems to show, that feeling is above the origin of these nerves; for there is a total annihilation of feeling below them when they are cut. And now I have done with that part of the subject.

Sympathy of the Heart.—There is a curious thing with respect to the heart, showing its sympathies with the lungs. This is one of the curious experiments that have been made: If an animal be pithed, the medulla spinalis divided high up, he dies; because the lungs want energy, and the *diaphragmatic* nerve has its function abolished, and he dies for want of breath. But let him be pithed, and respiration kept up by mechanical means, the inflation kept up, and life will go on. Now this is the experiment of JOHN HUNTER, on inventing a pair of bellows for drowned animals; he says, the nearest dependence of the heart is upon the lungs, for "when I left off blowing my bellows, (said JOHN) the heart left off its action: and when I began again to blow into the lungs, the heart recovered its power of action, at first feebly, but after a little, more strongly." I use the words exactly as Mr. HUNTER uttered them. Now, I trust all that nonsense is completely abolished; people say the *cerebrum* has no influence upon the heart; but I say, unquestionably it has a great influence upon it, and I quote this instance to show it: suppose a timid person, or a delicate female, hears a noise in the middle of the night, which she supposes either to be a ghost or a robber? egad, her pulsation fails, she gets cold, and is so alarmed as to be almost

dead. Suppose a fellow calls another person a scoundrel? the person who is so called feels his spirit rising, his face grows red, and he raises his fist and knocks the other down. Now it is very strange that such a person as BICHAT should make the heart the seat of feeling, and the head merely the seat of thought? Why he should put his hand upon his heart to show that that is the place of feeling, and his other hand upon his head to denote that that is the place of thought, I do not know; but his head had no thought in it, I think, when he talked such nonsense as that. You know that pleasant feelings do produce a pleasant action, as SHAKSPEARE said, "My bosom's lord sits lightly on its throne," and that uneasiness of mind has directly the converse effect. It is therefore certainly the effect of the cerebrum that acts upon these organs. I wonder it had not entered into the wise head of some person to argue that the diaphragm was not the seat of all these motions. SHAKSPEARE seems to me to have most sensibly felt the seat of these emotions: "Canst thou not minister to a mind diseased? Pluck from the memory a rooted sorrow? Rase out the written troubles of the *brain?*" and so on; you know the rest, I dare say, but I do not. Well, now, so much with respect to that: and so much nonsense has been said with respect to it, that I hope it will never enter into your consideration.

Sympathy of Nerves.—The dissection of the nerves sometimes has rendered it reasonable to suppose, what we are justified in supposing from every other part, that all parts of the body sympathize with one another; they show us the cause of sympathetic affections in divers diseases, and throw light on other subjects; it is, however, yet an indistinct light, but as the sun has risen, I hope it will go on and illuminate the subject more completely.

PECULIARITIES OF THE FŒTUS.

We have been considering the structure of the body, as we find it in the adult subject, and now there must be striking *peculiarities in the structure of the fœtus,* for the fœtus is not supported by food—by the stomach, by the digestion of food—but it is supported by an influx of blood into its body from the *placenta* through the *vena umbilicalis.* The course of that vein you are already acquainted with, it is what is called the *round ligament* of the *liver.* In the adult, after passing through

the umbilicus, arriving at the surface of the peritoneum, and proceeding upwards, it descends down the front and through the notch of the liver, and then it sends off several branches that enter the substance of the liver, the transverse fissure of the liver, and those ramify through the liver after the manner of the vena portæ, and have many branches which penetrate into the liver; then it makes a considerable turn inwards to the vena portæ. I now show you the vena portæ, and the trunk which is continued on joining it, and of course the blood which is conveyed by it permeates the liver through the branches of the vena portæ. There is a small residue of blood which goes on and terminates either in the *rena cava*, or in the *vena cava hepatica*. So that all the blood coming from the placenta does eventually get into the vena cava, either by first circulating through the liver, and passing by the vena cava hepatica, in consequence of the vena umbilicalis giving off direct vessels to the vena portæ, or by the residue being continued on by a tube into the vena cava, or vena cava hepatica; this small tube is called the *ductus venosus*, and it is the direct communication of the umbilical chord with the venous system of the fœtus. But a small portion only of the blood goes directly into the venous system; by far the greater part is made to circulate through the liver, either by direct branches given off by the vena cava, or by the connexion which the vena umbilicalis has with the vena portæ. Well, then, all the blood must go into the vena cava either by this connexion or the direct branches given off by the vena cava; but why this should happen we know not. O, I am sure there is something which we do not understand in the physiology of the liver. Why all this should happen we do not know. Why the blood that is conveyed as nourishment to the child should be made to circulate here, we certainly are ignorant. They say it is to keep the vessels of the liver open, that the liver may be prepared for the function of secreting bile as soon as the child is born. If such suppositions satisfy you I am content, but I am sure they are very unsatisfactory to my mind. Then the blood gets to the *right auricle* of the heart, Now the right auricle of the heart bears the same proportion to the rest of the other cavities of the heart in the fœtus that it does in the adult subject; but all the blood returned from the fœtal circulation is conveyed to the right auricle of the heart, with this great surplus derived from the placenta. But there is too much for it to hold, and what becomes of the sur-

plus quantity? O, it passes through the *foramen ovale* into the left auricle of the heart. A pressure being made against the membranous part of the auricle, the folds are forced asunder, and there is a large oval foramen through which the surplus goes into the left auricle. They say that the *valvula Eustachii* is peculiarly calculated to give this determination to the current of blood that comes thus into the vena cava. Well, but both auricles act together; they contract simultaneously, and in contracting, drive the blood into their respective arteries; the left drives the blood into the aorta, and the right, into the pulmonary artery. But with respect to that blood which is driven into the pulmonary artery, the question is, can it circulate through the lungs in the collapsed state of those organs? No. The answer is, no, it cannot; but if it could, there would be no use in it, and therefore only so much blood passes through the vessels, the vessels of the lungs from the pulmonary artery, as those organs are capable of transmitting; and I need not tell you, that this blood returns to the left auricle of the heart by the pulmonary veins, but it is not sufficient to fill them. Then the trunk of the descending artery may be said to go into the aorta. You always see a very strong fibrous chord tying the pulmonary artery to the ascending aorta. Then the blood of the left ventricle is sent to the aorta, and as much blood from the right ventricle as cannot permeate the lungs. Now as there is a continual current of blood coming into the child, what is to become of it? It cannot be all expended in nourishment. The surplus, the redundant quantity, must be returned, and is returned by the two *iliac arteries*, which go out at the navel and become the umbilical arteries of the chord; and I told you the reason for believing that the placenta served the same function, with regard to the fœtus, that the lungs do in the adult subject, namely, producing a change in the quality of the blood which was essential for the sustentation of vitality.

Now, then, those are the peculiarities of the fœtus, as far as relates to its circulating system; and a moment's reflection will show you how easy the transition is from what we meet with in the fœtus to what we meet with in the adult. When a child is born, and the *funis* is tied, no more blood returns to the right auricle of the heart than is calculated to fill it, no more than is returned from the circulation of the fœtus. The child respires, and what blood is projected by the right ventricle into the pulmonary artery, has an opportunity of

going through the pulmonary vessels, for the lungs are enlarged by respiration, their vessels unfolded; and there is a difficulty in its projecting blood into the descending aorta, for the same ligature that has cut off the supply of blood from the placenta has also cut off the return of blood from the *abdominal aorta*, as I may say, *ergo*, the abdominal aorta is full of blood, and therefore there is a difficulty presented to the right ventricle's projecting the blood into the descending aorta, and there is an easy passage into the lungs, as each auricle receives just that quantity of blood which is adequate to fill it; and so, by the membranous folds which form the membranous partition in the ventricles being kept in constant contact, they will grow together. Yet these changes do not immediately take place; you find the foramen ovale open for some years, and not finally closed; and even the *ductus arteriosus* is found permeous for some time.

Now I have done with this part of the subject; and how am I next to proceed? How am I to tell you the peculiarities of the fœtus? Why, I may as well begin with the head. Here I may observe that the brain is immensely large and very soft. But I may say, there are three circumstances characteristic of the fœtal life, or of early life. The *alimentary organs* are excessively large in proportion to the body; that is, that nourishment may be afforded for growth. The *vascular* system is also much larger than it is in the adult subject, in proportion to the bulk of the body. The *heart* and *arteries* are so too; but there is an infinite number of small arteries. Every part in the body is a tissue of arteries; and you make the child's body as red as possible by subtile injection. That is, the vessels which distribute nutriment, and produce the growth of the body, are inordinately large and numerous. The diminution in the number of vessels, is a thing taking place from birth to death; I mean in the number of arteries. I have to state to you now an old piece of physiology, which seems to be little thought of, or quoted at the present day; as we grow up, the number of arteries is diminishing. For a little time the skin gives a sort of consistency and solidity to the structure of the body. It becomes firm in the state in which we find it at the mature period of life; but it is a thing continually going on, and then what happens? O, then imperfect nutrition, and a change is produced. I have heard some modern-day physiologists speculate in this way, "Why should we ever die?" And I say there

is that in the system, in the sanguiferous system, which, in the early period of life, tends to produce growth, which, when growth has attained its utmost degree, gradually diminishes, does eventually tend to prevent the nutrition of the body, and produce a variance in the structure of that body. As the arteries close, the veins hold more of the circulating fluid, and they enlarge as we advance in life. You know it is the character of old age to have "shrivelled hands, with veins embossed"—a dry and shrivelled skin, and not like the skin of the young. The veins in old people are enlarged, as I said, but the young are more especially liable to *arterial plethora*, and the old to venous *pletho´-ra*, or *pleth´ora*, I do not know what you may choose to call it. Well, I say there are great organs for the converting of the food into nourishment, and numerous arteries for conveying the fluid; more than that, there is a large brain, and a very soft brain: there is that which is to give animation to the whole body. And you know how sensible the feelings of children are; how liable they are to be thrown into convulsions by simple causes, and, upon looking to the head, you find the membrana pupillaris existing for a considerable time. The cessation of this from the pupil is an act of the absorbents; but the choroid, as I might express it, is perfect to a certain period of time, till the seventh or eighth month. Dr. Jacob, who has examined it with great attention, says, he can see the membrana pupillaris even to the ninth month; but this is a subject that has been spoken of before. Now I need not tell you more about the head; for what I have not now told you, has been told on former occasions. I need not tell you the child is not born with teeth, and so on.

Chest.—Then we come to the chest: of such, I have to say, that the heart is large, and the lungs are collapsed. But you will see in the preparation I now hold before you, a substance as big as the heart itself, and it is called the *thymus gland*; but we know just about as much of it as we do of another part, which we call the *thyroid gland*. It is a soft substance. O, we may say it is like the *pancreas*; at any rate we eat it, as well as we do the pancreas. It is one of the sweetbreads that butchers send. The thyroid, and thymus, and pancreas, are all sweetbreads, as well as the salivary glands. It is supplied by numerous arteries from the *subclavian artery*. It has a vein returning the blood to the subclavian vein: nothing like an *excretory duct* has ever been found in it. They say, if you cut

it open, you find some cells in the interior, containing a kind of fluid, which they have described as a kind of milky fluid; but further than that, I never heard any one give an account of it. Of course we are doomed to suppose, that it must serve some purpose connected with the economy of fœtal life, as it shrinks by degrees into insignificance in the adult subject.

Now with regard to the lungs, which are collapsed, they are heavier than water; animal matter is heavier than water; they are a very light kind of animal matter, however, but they of course will sink in water. They are very light, and when once they have been inflated, the air can never be got out of them so thoroughly, but that they will swim; and this is a fact popularly known. I do not mean to say, that where the lungs have deposited matter in them, and have become indurated, that they will not sink; but I mean without disease; and I say this fact is popularly known. It is dwelt upon in criminal courts. A mother is suspected of destroying her child, and the jury ask whether the lungs sink or swim. A medical man, of course, should be prepared for the interrogatories that may be put; and I can tell you one thing, that if ever I went into a court of justice, they should never get an opinion out of me, do what they would. I should just state the facts, and leave them to form their own opinions. It is really a very awful situation in which a medical man is put, to give an opinion to prejudice the minds of the jury. The jury are to determine according to the facts, and you have no business to influence them with your opinion. I shall tell you my feelings on the subject: I remember once being subpœnaed upon a trial, and I told the people positively who subpœnaed me, that I would give no opinion, but still they would subpœna me. They said it was a case of importance; that the public would be satisfied with my opinion, and that perhaps they would not be satisfied with any body's else; so they said; but, however, they persisted in it, and I was obliged to attend. The case was that of a Lascar, who had been struck with a marlin spike, aboard ship, and he died; and then the question was, whether the blow he had received with the marlin spike had killed him or not. When I went into the court, and after the evidence had been given with relation to the transaction, the lawyer said to the judge, "My lord, Mr. Abernethy is in court, and we will thank you to take his deposition now, as we know his time is valuable; will you have the goodness to take it now?" "Cer-

tainly." "Can you, sir, from what you have heard, form an opinion as to whether the blow the Lascar received, was, or was not, the cause of his death?" To which I answered, "My lord, there is no evidence before this court, upon which medical opinion can or ought to be founded." "Why, sir, do you not mean to give an opinion at all?" "No." "Why sir, you were sent for for the purpose of giving an opinion." I told him I should not do it. Then the party accused had a counsel, and he got up; "O, Mr. ABERNETHY," says he, "it might have been the fever, you know, that destroyed this Lascar? You are acquainted with those East India fevers, I believe?" "I am not." Then the counsel on the other side set at me, and I just said, "After what I have told his lordship, I should be unwilling to offer one word that could in any degree influence the minds of the jury." Then the judge says, "But, indeed, this is very strange, sir; we are to look to you for an opinion." "My lord," said I, "I am ready to tell you, if you please, the grounds for what I have said to your lordship." "I should be very happy to hear them, sir." So he dipped his pen in the ink, and I spoke deliberately that he might take it down: "In case of death succeeding to injury, the medical evidence goes to prove, that the subsequent death was, or was not, the effect of the preceding injury, by showing that that injury had materially affected parts essential to life. My lord, there is no evidence of this kind before the court." "Certainly not," he said, and I was dismissed. Now I did not tell them that there was no evidence before the court on which the jury could found an opinion, but that was the construction they put on it; and they said, "Why, if Mr. ABERNETHY cannot form an opinion, we cannot;" and as justice always leans on the side of mercy, the accused individual was discharged. But it is a very awful situation in which you are placed; and if you were to say, a blow from a marlin spike would not kill a Lascar, you would say a very outrageous thing. Many of those Lascars have been killed with a box of the fist which a boxer here would laugh at. They are what is called a *nash* sort of people, very susceptible of injury; and if you were to say the blow had killed him, the other man's life would be in jeopardy, though perhaps he had done nothing but what he was perfectly justifiable in doing. Then, I say, I would not give an opinion in a court of justice: but if a juryman asks you a question, you must answer; you should try—of course it

belongs to you to try, not to let his mind be too much prejudiced by the evidence with relation to the fatal point.

As to the *lungs*, the lungs may swim, and they may sink, if a slight degree of putrefaction has taken place. Nay, further, *Nature* has a powerful claim on this point, urging the improbability of the crime; but we are not to argue that. Certain it is, that a mother is strongly attached to her child. This is an instinct common to the human subject, as it is to animals, but yet we do know there are some animals that destroy their young; and some mothers may do it too. I think it is better they should do that than that they should neglect them. Well, I say we are not to believe the thing impossible, because it is possible; but, in general, it must be said, that it is highly improbable. It is one of the strongest instincts, that of attachment to the offspring. And then Dr. HUNTER says, that a woman may be delivered under very suspicious circumstances: a maid servant may be with child; she knows not when to expect her delivery; she has nobody to consult with; people are always thinking the evil day further off than it may really be; she has tried to conceal her shape, and she thinks she has succeeded in the attempt. Now women are very clever in that respect, as I can testify, having known matrons of my own acquaintance who have hired servants, who, a fortnight afterwards, were brought to bed in their own house, and to the great discomfiture of the said matrons, as that seemed to imply a want of *knowingness* with regard to a subject, a correct knowledge of which they particularly prided themselves upon having. But Mr. HUNTER says, many persons under these circumstances have mistaken the pains of labour for a disposition to evacuate their bowels; that they have, therefore, gone into a necessary, where the pains of labour have come on, and they have been brought to bed in that situation—*brought to bed*—I mean *delivered* of a child in that situation. Now suppose this should really happen; egad, it is popularly known that if a child does not breathe, the lungs will sink; but a person might try to inflate its lungs, and the female might even do that—might blow a little air into the lungs—might do that which would be the cause of her own condemnation afterwards. Mr. HUNTER relates the circumstance of a child having been born where the funis was twisted round its neck, so that any body would have said, I will be hanged if they have not strangled this child. But suppose the labour was achieved, and they had concealed

their shape till this time, would they not go on to endeavour to conceal what had happened? Suppose a young woman had done this, and had put the dead child into a box, and with a degree of fortitude that would have done honour to an old Roman, had gone about the house doing the work of it, until she was able to continue doing so no longer, and was then obliged to give up and take to her bed; then a doctor is sent for, and though she had concealed her shape, yet there was a sort of suspicion about her case. The lady says, why, it is very strange, but there is something about our Betty that I do not understand. Well, the doctor goes prying about, he sees some stains of blood, and he says, surely you have been brought to bed? I have. Where is the child? It is in my box. The doctor examines it, and the lungs float. Well, but putrefaction may have began. There is a question, too, that is asked, has she provided raiment for her child? No, how could she? she did not anticipate being ill so soon—how could she? She had no *confidante*—how could she? Well, but never neglect to examine the lungs. It is your duty, I think, to try to weaken the effect of your testimony upon this point; but do not neglect to examine the lungs, for if the lungs should be found to float, it will be manifest that the child was born alive, if the lungs are *sound*; and that it could only have been killed by violence, and if no violence can be shown to have been committed, that is sufficient to acquit the person.

Now I have done with that, and we next go into the *abdomen*; and here the organs, as I have told you, are excessively large. And it curiously happens, the *valvulæ coniventes* are not formed in the intestines. The extent of surface made for *secretion* and *absorption* is not to be found till the process is to be performed. There is stuff called *meconium* in the child's bowels, and some have questioned what it is. Some have thought it was from the liver. It is probable that it is the secretions of the bowels themselves; I could tell you a case, in support of that, where meconium was voided, and where there afterwards came secretions from the bowels, which could not be distinguished from the discharges from another child who had sucked.

Now, with regard to the abdomen, we find nothing remarkable in it except at the back part—very remarkable, and there we find the kidneys lobulated, and we find the renal capsules as big as themselves—quite as big as themselves. But I have told you all I know about the structure of the renal capsules;

we do not know their use; but here again we are obliged to satisfy ourselves that they perform some functions connected with the economy of life, which we do not understand.

It is a question, *when* the secretion of *urine* begins? Now, I own I thought that it did not begin till birth, because I have met with cases where children could not void their urine—where there was an impervious prepuce or urethra, and where the child did not appear to labour under any difficulty till about the common period when a child wants to make water after it is born, and then it cried and strove ineffectually: but there is a very curious case in Mr. HEAVISIDE'S museum, in which there was an impervious urethra, and here the child's bladder and kidneys were distended and diseased just as you see them in the adult subject, where there has been stricture of the urethra impeding the flow of the urine. Now, I say, I know not what to think about this. To be sure, if a child did *piddle* in *utero*, it must have voided its urine in the *liquor amnii*. I am really disposed to think it does not secrete urine till after birth; yet I tell you, there is an instance which is considered to show the contrary.

What is considered the most marvellous thing is, that the *testicles* are found nearly as high up as the kidney, upon the *quadratus lumborum* muscle, and just by the side of the aorta, so that the testicles did receive their blood from the nearest arterious source. Not only the testicle in the male subject, but the *ovaries* of the female subject are found there, and which receive their blood from the same source, either from the aorta or renal arteries. Then we see the testes gradually lower and lower, till they approach the abdominal opening; we find them gradually getting down, and it is between the seventh and eighth month that they are passing to their destination at the bottom of the *scrotum*. Now this was a subject which HALLER and HUNTER investigated at the same time; they made the discovery of all the series of facts relating to the subject; and Mr. HUNTER published his account as an account of the *descent* of the testes. To that word I have a grand objection; because, as children are generally head downwards and breech upwards, he should rather call it the *ascent* of the testes, I think; in short, I have an objection to any one persuading himself or others, that he understands how this transition of place is produced. There is something going up to the testes from the under surface of the abdominal muscles; it is broad where it is

attached to the testes, and narrow at the other end; Mr. HUNTER thinks it is the *cremaster muscle*, and he has called that *gubernaculum*, as if contributing to govern the testes. Now, if it did, I wonder it did not keep the testes there. Why do they go down? For the change takes place with a sort of regularity and progression of transposition from their first situation, to its ultimate situation in the bottom of the scrotum. If people say, O, I know how it is done; why then I ask—do you know how the ovaries get also into the pelvis, for there is a similar mutation of the parts, but I never heard of any gubernaculums being attached to the ovum.

Well, Mr. HUNTER describes that when the testicle has got down to the ring of the *obliquus externus*, the peritoneum with which it is covered adheres to it; and he says there is a *pouch* of peritoneum provided in front of it projecting through the ring like the inverted finger of a glove, and having got down into the pouch, it of course gets down into the bottom of the scrotum. Now none of this do I believe; that is, I believe the pouch protrudes, and that the pouch enlarges, and that the pouch descends, or gets to the end of the scrotum, and that I have seen nothing more than the testes move down to the bottom of the scrotum behind the peritoneum. I do not think it is done *mechanically*; for the very circumstance of the pouch being forced down before the testes get into it, absolutely refutes the idea of its being mechanical or done by pressure; however, down it goes to the very bottom of the pouch, and you find it, at the bottom of the scrotum. Well, then, in the fœtus you find it as you see it in the preparation I now hold up of the adult subject; for the peritoneum passes out of the abdominal opening, makes a sort of pouch, and the testicle, which has descended behind the peritoneum in the abdomen, has descended into the pouch and lies at the very bottom of the scrotum, consequently it is covered in its front surface by peritoneum. The bag in the bottom is called the *tunica vaginalis*, and having a layer going over the front that is called the *tunica vaginalis reflecta*. The process by which the testicle communicates with the abdominal cavity degenerates into cellular substance, but perhaps not so completely formed into small cells as cellular substance would usually be, and therefore you see young persons particularly liable to *hydroceles* of the *spermatic chord*, to collections of large fluids in the cells. Well, the parts remain pervious, and they are liable to what is called *congenital*

hydrocele. Those cases generally get well, and so do the cases where there are *cysts*—hydrocele *in front* of the spermatic chord—those cases generally get well.

Now I have done with regard to that subject, the descent of the testes, the facts of which are known, but the cause I believe is not well understood. Then what further have I to say? Why, that the pelvis and lower extremities of the child really are insignificant in bulk. The bladder rises up as high as the umbilicus; it is connected, you know, to the funis umbilicus. It is a long while before the pelvis gets into that state which enables it to receive the bladder, as we find it in the adult subject. Some people please themselves by accounting for the growth of the pelvis and lower extremities of the child by saying, that the aorta is formed of a certain *calibre,* and that as the return of the blood through the internal iliac arteries is cut off by tying the funis, so more blood is forced into the arteries of the pelvis and lower extremities, wherefore those parts do grow faster after birth than they did before. I say some persons please themselves by assigning vital operations to mechanical causes; now I have no pleasure in doing that; I would rather assign them to certain laws of nature relative to life, which I know are inexplicable. Life is a sort of principle which builds up structures and endows them with powers, and produces changes in them; it is an active principle moving on, and acting according to laws which we do not understand. We can only note the effects, but we are unacquainted with the laws that govern the actions of this subtile agent—*life*: they are always effects productive of good, productive of some necessary changes; but to suppose that it depends upon mechanical circumstances, is to look to an agency incompetent to effect the purposes accomplished; that I feel very well convinced of.

Well, then I have now done with the peculiarities of the fœtus; and I say that this last lecture is a lecture containing an acknowledgment of ignorance, for we really know not any thing of these peculiarities. We may, if we please, satisfy our *vanity* in some degree, by supposing that if these things were to be found in the adult subject, we should be able more perfectly to understand them.

This concludes the anatomical part of the Lectures, and to-morrow we shall begin the Surgical *Operations.*

LECTURE XLII.

ON OPERATIVE SURGERY.

In the time of LE DRAN, surgery was divided into four parts :

1. *Synthesis,* or the *union* of parts ;
2. *Diæresis,* or the *division* of parts ;
3. *Exæresis,* or the *removal* of parts ; and
4. *Prosthesis,* or the *substitution* of parts ; and a most villanous division it was, for it led to the notion of surgery being *merely* an *operative* science. The necessary performance of an operation is, or ought to be, an humiliating reflection, since it contains a confession that our art is inadequate to the cure of disease. Well, a great deal more might be said upon this point, if you choose to go to what JOHN HUNTER has said to the same purpose; but it is a good division to me nevertheless, for I am going to speak of the operative part of surgery, and first I shall speak of the

Division of Parts—which is effected by a *knife.* And I am sure people will use a knife better, if they reflect upon the nature of the instrument before they begin to use it. It is a compound instrument, inasmuch as it is both a *wedge* and a *saw ;* it is a wedge, because it is broader at the back than at the part you use, and you may cut with it, but for every fibre you cut you will bruise almost a thousand. Now it is used as a saw ; and what is a finely cutting knife ? A knife with such an edge as to intercept the least possible fibre in the interstices, so that it cuts cleanly ; and if you draw such a knife over the surface, using no further force than is necessary, you will make a fine clean cut. People often complain of their knife, when the fault is not to much of the instrument as of the hand that uses it. The fact is, that with a dull knife people often cut themselves unawares, when, if they had tried, they could not have done it, and then they say, *la,* who would have thought it!

Well, the *use* of the instrument ;—it is to be used like a pen or a pencil. There are certain muscles over which we have acquired a perfect command, such as the muscles of the fingers, and the instrument is used with those muscles as a pen or a

pencil, thrown from you by the *extensor*, and drawn to you by the *flexor* muscles of the fingers. There is a phrase, *manu suspensa*; it means, with a light hand, but not an unsupported hand, as in drawing or writing you make a sort of support for your fingers to rest upon. Mr. Pott used to say, that dissection taught a man the free use of the knife; meaning to convey, that anatomists did not hesitate to make a free use of the instrument. It is, however, literally true that, by dissection, you acquire a free use of it; and as a person who teaches a pupil to write would first make him take long strokes, going on gradually, till he wrote small hand, so the way of acquiring knowledge of dissection is to dissect cleanly as you go along, and to dissect in the line of the fibres; never mind cutting away a bundle of the fibres if you are in the line of the fibres, for cutting away the fibres in the line of the fibres, makes no disfigurement in the muscle, and that used to be a cant phrase in the early part of my life. Well, you use the knife in that way, and you ought as much to expect a draftsman to draw a line in a direction he did not intend, or a penman to misform a letter, as to expect that a surgeon would cut a fibre if he did not intend it.

There are two modes of using a knife; one is, by drawing it towards you, and the other is, cutting with the edge turned from you; and this is a very good way of dividing parts in many cases; as for instance, where you have a breast to wound. you hold the breast up, and make your incision at the under part of it, in the first instance. Well, so much for the instrument. And a saw is a knife: a carpenter, in using his saw, will not bear upon it; he draws it lightly over the surface he means to divide, and so ought you. Well, these are things for your own consideration; and I say it is a grievous thing to see a surgeon operating upon a fellow being, and not using his instrument as he should do—that is all I shall say on that point.

Now as to this synthesis, or the union of parts; would any body, any reflecting person of the modern day, believe it possible that the old surgeons used to sew up wounds as they would stitch up a rent in an inanimate part? And you see even in modern books of surgery, that *sutures* should be made *firmly*, and so on. Now the whole of this absurdity has been exposed by M. Louis; and notwithstanding that MM. Louis and Petit both published on the subject, I see books in which the use of sutures is inculcated. But what are you to do? Are

you to cut a wound, to make a further division for the purpose of getting a union of the parts? Would you aggravate the wound by putting in needles, and leaving foreign bodies in the parts, which would be sure to produce an inflammation extending beyond the first stage, and which would be certain to produce suppuration? O, this should never be done. Is there any necessity for dragging parts together? No, not if you attend to position; it is by position that you close wounds. If there was a cut through the skin across my arm, when I bent my arm, the sides of the wound would come in contact, but if I stretched out the arm, then there would be a gap. In muscular wounds it is particularly necessary that you should attend to position; you may bring the skin together by drawing it with sutures, but you never can bring the separated parts of the muscles together; or where by force you drag them together, there is always a bag left at the bottom by the receding muscles. Now where wounds can be closed by adhesion, and sticking-plaster only, none but fools would use sutures. They seem to have made strong sutures formerly, by which they dragged parts together, while they stood with a pair of scissors in their hands ready to snip them if *lock-jaw* came on. You must not draw parts together with sutures; they are useless if they are not required, and you cannot have the use of them without their being pernicious. Still I do not carry my antipathy to sutures to the extent to which M. Louis did, for I know there is a good in them; in the cut *scrotum*, for instance, a thread or two may keep the parts together; and there is another good in sutures, which is, that they keep the patient quiet: pin a child's *hare-lip* together; O, egad, the child will never cry nor laugh, nor do any thing to separate the parts. M. Louis carried his abhorrence of sutures to the extent that he would not use them in the hare-lip; but I say I would not carry my abhorrence of them to the extent that M. Louis did. Now the operations of surgery by sutures will be shown to you, and therefore I only tell you what relates to the subject as far as *principle* is concerned.

Well, I believe I had better not say any thing about instruments, and so on. A needle is employed to pass the sutures, and I would have you take care that the edge of your needle should be *lancet-shaped*, that it may cut easily, and that the thread may be in the wound without distention of the parts. If you do not give directions as to this point, you may depend

upon it the instrument-makers will force upon you a wedge-shaped needle; there is a little more consumption of the metal in making a lancet-shaped one, and therefore if you let the instrument-makers alone, they will always give you wedge-shaped needles, which I would not have.

Now, in speaking of the operations of surgery, there is no better mode of proceeding, that I know of, than that which was first adopted by PAULUS of Ægina, beginning with those of the head, and regularly descending.

Trephining.—Then about the upper part, the first operation I shall speak of is that of trephining; that is, boring a hole in a man's skull; that is what is meant by it. The first instrument was called a *trepan*, then it was called a *trephine*; there are advocates for the one, and advocates for the other, now-a-days; but I have always been impudent enough to say, that on many occasions I would puzzle the very best surgeon in London, by asking a single question—even Mr. POTT himself, whom I considered one of that character—and the question would be, "Why, sir, do you ever trephine a man?" Egad, I do not know; because—because, perhaps, he would say—because his skull was broken. Well, but, sir, I would say, I do not see you trephine a man who has broke his leg; trephining the leg would break it worse. No; O, no, but because there are symptoms. Symptoms! What symptoms? Now the answer to the question is as simple as the question itself: you never trephine, but to take pressure from off the brain in cases of injury. I have, in the course of the lectures, told you, there were cases requiring you to trephine from disease. You may take away the external plate of the skull, to let out matter between it and the *diploe*. You may uncover the whole of a part of the skull, to let out matter between it and the dura mater. But, in cases of accidents, you trephine to take away pressure; and what are the cases of pressure? Why, the bone may be beaten in upon the brain; blood may be shed beneath the bone, or matter may be collected under it; but does every depression of the skull warrant you in trephining? No, it does not appear that it does. And what are the symptoms of pressure, from the direct injury, which are potent enough to warrant the operation? Why, the symptoms of pressure are, *torpor* of the brain, more or less, according to the degree of injury. Now, in the course of the lectures, I have quoted cases in proof of the different propositions I maintain. I have told you of the

bleeding from the meningeal artery ; and I related the case of a man who was knocked down by a crane. He got up, shook his ears, and was not further stunned, but that artery was bleeding ; he thought he would go home ; but before he got to his lodgings, he felt very heavy and sleepy ; he went and lay down on the bed, and gradually became perfectly apoplectic ; respiration went on, as it were, automatically, but he was as insensible as if he had never been sensible. You might have cut him, or struck him, and he knew nothing about it. I say, therefore, there are symptoms of torpor ; and, in some people, the brain is more readily affected, and made torpid by a slighter degree of pressure, than in other persons ; but I never heard of a case where the brain was pressed upon, in which the symptoms were otherwise than those of torpor—torpor of the functions of the brain. There was a very curious case, where a sailor got a piece of his skull beaten in, in the Mediterranean ; he was brought to this country before the pressure was removed ; he was in a perfectly torpid state ; but when he was trephined he got his senses at once.

Well, I believe it is torpor that is the characteristic of pressure. Now I particularly wish you to distinguish between torpor which is occasioned by a diminution of excitability of the brain, and that state which is called concussion. Certain it is, if we had never seen a man asleep, we should have said, this man is insensible. He does not see me, nor does he hear me ; that is, he is insensible; his mind is either idle, or engaged about his own fancy; he is dreaming, or he is in a state that you would call abstraction—thinking of *nothing* at all, as the saying is. But he is not insensible; if you awake him, he will jump up, and this is the state of people who have mere concussion of the brain. The assemblage of symptoms which denote pressure are these : a greater or less degree of insensibility, manifested by a dilated pupil. When they are deeply apopletic, you may put a candle to the pupil, and it will not stir or be affected ; you may sound a trumpet in their ears, and they will never hear it ; cut them and they will not stir. They never vomit ; O, that would show a sensibility of stomach. Circulation and respiration go on, as I have expressed it, automatically ; the pulse goes on but feebly, and the respiration is proportionably slow and stertorous. But, in the case of concussion, what have you ? Have you any of those symptoms ? O, not at all ; and yet I grant you, in the first state, that the jarring of

the brain disqualifies it from its functions: a man is knocked down, he is perfectly pale, and, for a time, you may bleed him, but you cannot rouse him; still this lasts but for a short time —just while the stunning lasts. You take him up, put him to bed, and as soon as the circulation is diffused, as soon as warmth is extended over the body, he is no more insensible than a man in a deep sleep; you may rouse him if you call loud enough, but it requires some strength of voice to do that; and not only is loudness of voice necessary, but you must call out something that really will excite him, something that interests him, and then you may raise him. Now look to the other symptoms: Is there any thing in the other symptoms that indicates insensibility? No; and I am persuaded quite the contrary. The pupil of the eye is by no means dilated; nay sometimes it is more contracted than usual; the brow has frowns upon it; there is an expression of uneasiness in the patient; the pulse is frequent, it may be intermitting. As to the *intermission* of the *pulse*, I have told you I do not put any great faith in that, for it seems to be occasioned by a *capriciousness* in the action of the heart; but, as far as I know, the intermission is much more common in the case of concussion than that of compression. Raise the man, and he vomits, which shows a sensibility of stomach.

Well, now, what suceeeds to this stunning? O, it is inflammation. I say, to all mechanical injuries, inflammation succeeds. You may have a bone driven in; you may have matter formed between the bone and the brain, and you may have inflammation produced in proportion to the violence of the concussion; then when the inflammation comes on, O, then indeed, the patient does not sleep: sleep he may, occasionally, but when he is awake, he will be trying his utmost to get out of bed, or something of that sort. He will have symptoms, common symptoms, of *phrenitis* come on. Now how can I prove this to you? Prove it? Prove it by the recital of cases. I was educated under Mr. Pott; he used to give lectures; they were lectures corresponding with his book; and I will venture to say, that for twenty years of my life, there never was a man admitted into this hospital with an injured head, that I did not attend; and there never was one who died afterwards, that I did not examine; and therefore as I picked up a quantity of cases, and got information from them in addition to what I was taught, I afterwards put together and published those cases, with what I had to say upon them. Well, how am I to prove

it? By the recital of cases; and I will tell you one of the worst, and one of the slightest of those cases. There was a man who fell from the top of Meux's brewhouse, 80 feet in height, nay, 90 feet, I believe; he fell upon his hand, and broke his *carpus* all to pieces; knocked off the ends of the *radius* and *ulna*; injured the cheek, for that also came against something, and the head was jarred: he was brought to this hospital like a corpse. They put him into a warm bed, passed the warming pan over his body, tried to give him a little brandy, which, of course, he did not swallow; it, however, washed his mouth: they put hartshorn to his nose, and by degrees he came round; warmth returned all over the surface of the body, his pulse became exceedingly rapid, and this was the state in which I found him next morning. He was sweating, secretion was going on, and, as I say, the pulse was exceedingly rapid; the pulse was what one of the pupils called a *fluttering* pulse, so frequent that it could not be counted. He had taken no medicine, but having got some, and calling out the man's name, I said to him in a loud voice, "You must drink this stuff; if you drink it, you may live, and do well; if you don't, you must die: drink it I say." Well, he drank it. Now I know the ways of those people pretty well, for I have a good deal to do with them; however, he died, and his head was examined. The *pia mater* every where was excessively inflamed: absolute effusion of blood had taken place, and every manifestation of the most violent arterious action of short duration was apparent. He could not have lived long from such effects. Well, I say that shows you the worst case that I can remember. As to concussion, I have heard it was a term purely meant to conceal ignorance; and so it was formerly, but, as we use it, it is a very definable term; it is, that the brain having received a jar, it is incompetent to perform its functions in the ordinary manner. All this may happen without any lesion of the brain. In this case of the man which I have just stated, there was not a part of the brain which appeared to be torn; but I know you have cases in which some parts of it are torn. Mr. Pott says he never knew a man recover without the abrogation of some part of the organ. Well, I grant there may be some injury done to the brain, but that has nothing to do with the treatment. They may recover. I can only tell you that I have had very, very bad cases indeed, and that they have all got well, and with the perfect performance of the functions. I have not met with any

of those cases Mr. Pott speaks of. I remember of late years there was a man who had a very horrible degree of concussion, and he squinted afterwards; but I will be hanged if the squinting did not come on subsequently to the concussion, and I believe it was from an effect produced upon the brain, from the reaction of a disordered state of the bowels. He went out of the hospital quite well; but I asked the pupils to watch him, and the complaint took place as a secondary occurrence; that is, the squinting took place as a secondary occurrence. However, whether there be a rupture of any of the fibres of the brain, tending to produce some local injury or not, it makes no difference to the surgical treatment. Inflammation you are to expect, and if you permit that inflammation to go on, your patient will be destroyed, therefore you must bleed. To bleed a man who has had a concussion, in the first instance, when he is from appearance killed, would be an absurdity; but as soon as warmth has gone over him, you may take away a little blood. I call it a *preparatory* bleeding. You must do that which would lessen the force and action of the heart and arteries. I do not call this a curative measure, it is preventive; it is done with a consciousness that inflammation must succeed, but with a view to moderate that inflammation. Of course you would clear the patient's bowels; you would keep his bowels kindly relaxed. You would let him lie with his head raised, and supported up with pillows. You would bathe his head if it were heated; but when you found a sort of muttering by the patient, and inflammation come on, O, you would bleed again, apply blisters, and do all those things which the principles of the profession enjoin, and which I need not further impress on your minds.

Will this treatment be successful? Yea, verily; I tell you a case illustrative of it. There was a stout man, a Welshman, of the name of Davis, who was brought to this hospital with a violent concussion of his brain. He was attended by a pupil who was here at that time, of the name of Shepherd, and as clear a headed man he was as I ever saw. Davis was treated as I tell you, but violent inflammation came on; and a very violent strong Welshman he was. Shepherd took 124 ounces of blood from him in twenty-four hours, and he could scarcely control the Welshman even then, but he dared not bleed any more. Blisters were put upon his neck and different parts of his body; his bowels were attended to, but all this did not seem to subdue

inflammatory action ; perhaps we might say, it had opposed it. However, about the fifth day he became *hemiplegic*, and Mr. SHEPHERD came running to me, and says, it's all over with poor DAVIS ; he has lost the use of the right side of his body ; there is some effusion on the left hemisphere of his brain : egad, that's just what I thought, I said ; but I am not sure of its being all over with him. He said, there can be nothing done further to him. I said, no, unless it be the use of *calomel*. Well, he got some people to rub mercurial ointment on DAVIS's limbs twice a day, and in about three days it affected his bowels, and his guts at the same time, for he had a violent fit of purging come on. Now, whether the mercury did good to his head by the disturbance it occasioned in the bowels, I have some doubt myself at present, but the *hemiplegia* was removed. I remember going to him and asking him to let me see his hand, the hand that had been paralyzed, and he put it out as firmly as ever he had done in his life. Well, I can tell you that nothing but the most delicate nursing brought this man round. Then, I say, you are obliged to subdue inflammation, but a great deal is to be done after that ; however, DAVIS did come round, his senses were pefectly restored, and he was discharged in as good health as before he met with the accident, but it was by a great deal of attention having been paid to him.

Then, I say, this is what we are to do ; but, would you believe it, there are divers books on surgery that seem to recommend a contrary plan of treatment ; they seem to draw a parallel between the state of the brain, which renders it incapable of its functions, and a palsy from want of energy, and they recommend stimulants. For a long time I did puzzle myself to know what the plague could be meant by those cases ; and at last, I found out that they were cases where people give themselves a bump on the head, and that, being nervous subjects, a train of nervous affections come on, and they called the case a concussion ! For instance, a young lady in her closet was stooping to pick up something ; she raised her head, and struck it against a shelf ; she did not stun herself, but she felt a little hurt, and a nervous attack seized her. O, they will tell you such strange things, tell you that their brains are turning round, tell you that their skull is being turned inside out, and all these sort of things, but they are nothing more than nervous symptoms. Well, all this will be managed by a little stomachic, and taking care of the bowels.

I say, I puzzled myself a long time, to know in what sort of cases a contrary treatment could possibly be beneficial. And further, I acknowledge that I have always taught what has lately been asserted by a Mr. FIELD, of Newcastle, I believe; he seems to me to be a very sensible intelligent surgeon, and he has published a book, discommending the free abstraction of blood in those cases of concussion.

Now there are people who have concussion, who are nervous, and if you bleed them much you may do them injury. I remember being called to two females at one time, who were thrown out of a gig. I remember I was sent for down to Gravesend to see one, and as far in another direction to see the other. I told the surgeon at Gravesend what I thought of it; I said, if inflammatory action or pain in the head came on, bleeding might be used; but I really believed he had used the lancet too freely; at any rate, the woman was so nervous and fidgetty for two or three days afterwards, that she was almost fit for a place calculated for the reception of insane persons; all kinds of fancies, and so on, floated in her mind. With regard to the other case, I had myself been attending the woman formerly for some disease about the ankle; she went into the country, and riding out in a gig (for she could not walk well) she was canted out of it and stunned. There was some degree of delirium, and pain in the head; she was bled, and got better. Still, however, she complained of pain in the head, which I believe was a nervous attack altogether, and she always expressed herself greatly relieved by the application of leeches. She was naturally weak, and of course rendered more so by this treatment, when all of a sudden she declared she could see nothing, but was totally blind. This was what created anxiety, and I was sent for. The gentleman whom I met was an intelligent man, and I always think those who have seen the whole of a case know more of it than any person can know upon just coming in on a sudden. I dared not tell him that he ought not to take blood, but I said this blindness is not the immediate effect of the effusion, otherwise it would have been contemporaneous with the injury; and besides, your depletion must have done away with the effect of it. "Then what is it?" he said. To which I answered, it seemed to me to be a state of nervous blindness, and that I would treat it as such, by great attention to her system, keeping the bowels open, but not in such a degree as to irritate them,

giving her alterative doses of quicksilver, giving her ginger to drink, and so on; he did this, and she got well. Thus I caution you, that you are not to treat all people as SHEPHERD treated DAVIS; such treatment was appropriate to him, but it would not do for a delicate female.

Now I go on last of all to say, that the ultimate well-doing of a person who has concussion of the brain, depends upon your attention to their digestive organs; and, egad, I have seen hundreds and thousands of people who, having had a blow on the head and stunned, were perhaps bled, and after getting a dose of physic were very well, and no more attention was paid to them till they have become complete *hypochondriacs*, from the state of the alimentary organs. There is no hurt of the head that does not affect the bowels; but I say the great thing we have to do is, to prevent that action and reaction which disturbed nerves have upon the abdominal viscera and again from the abdominal viscera upon the nerves of the cerebrum. Well, now, if the bowels be not attended to, the people will go into a complete state of hypochondriasis, and therefore I am sure the recovery of the functions of the brain mainly depends upon the digestive organs. It is nonsense to suppose that a man who has had any violent concussion of the brain will have all the functions of that organ get right of a sudden. I remember attending a gentleman who was a great calculator, and after he recovered he said to me, if I read a paragraph in the newspapers, I understand the first sentence, but beyond that I have no comprehension of it; and he said, it was a twelvemonth before I could do those sums with the utmost difficulty, that I could do with the utmost facility before the accident happened. Therefore there is a great deal to be done if you wish to establish your patient in perfect health, for where there is that disturbance in the brain there will be a disposition to disorder in the digestive organs, and that disorder will react on the brain.

Trephining.—Well, now I have said what I have to say on the distinction between cases of compression and concussion, and what may be done in either case, so that I may now proceed to state the kinds of concussion you may relieve by trephining; and of course you may trephine in cases of compression of the brain, but not in every one of these. Surgeons always do, and it is natural for people to go into extremes, but the worst thing of all is party spirit. We are all partisans, and we do side

with one party or the other. I, however, have always said, I shall never sell my soul to any party, as long as I live; I shall never give up my opinion. There is one set of surgeons who trephine in all cases, and there is another set who trephine in none. DESAULT, a surgeon of the greatest experience, never would trephine whatever befel a skull; for, said he, I have seen some with very badly fractured skulls recover without having been trephined, but I never saw one recover who had the operation performed upon him. DESAULT practised in the Hôtel Dieu, a most horribly crowded place; I think there were four tiers of beds along the wall when I was in it, which was at the time DESAULT was there, and two patients in each bed. Now it is not to be wondered at, and it deserves your attention, that a man in the country may be trephined, and trephined again, and do well; they can do things in the country that cannot be done in London hospitals. But I want you to think of it, and to understand it, and I am aware you will never understand it by lectures; practical surgery is only to be acquired by experience, but I say the object is to send students forth into the world with a knowledge of what they are to observe, and how they are to make observations, possessing a knowledge of the principles on which the profession ought to be practised.

Now then, as I saw people going into extremes, I thought it a good thing to relate half a dozen cases of depression, and of great depression, which did well without the operation; there they are, and you may read them if you like; but there came in a case shortly afterwards, which I will mention. A young man had a fracture, extending across the squamous part of the temporal bone, half of it seemed to be just on the organ of murder, and it was the upper part of his skull that was depressed. Egad, there was no trephining it, you could not put on the trephine; you could not raise up the depressed bone. We did not know the extent of the injury; there was an external wound, but it was manifest that the parietal bone was depressed on the upper part of his head, fully the eighth part of an inch below the other part of the bone, and this could be felt, as far as we could trace the injury. But the chap had no bad symptoms at all, and he came to the hospital. Now, said I to the students, here is a case where you cannot trephine, therefore you must leave the fracture as it is, and this will show whether the opinions I have published be right or

wrong. Here is a fracture that produces no bad effect on the instant of the injury, nor have any symptoms as yet occurred; now the question is whether there ever will. Well, we gave him some physic; but begad, it is necessary to watch these cases very attentively. I saw him generally three times a-day; on the day after he came into the hospital at noon, he was as well as you could have wished him to be; however, I came down in the evening, and I found that he had got fidgetty and restless, and was trying to get out of bed, so I took a large quantity of blood from his arm, gave him a dose of physic, and told him to keep his head supported high with pillows, ordering his head to be bathed, and the next day he was very well. Now, if that bleeding had not taken place, if those measures had not been adopted at the time at which they were used, so as to have subdued the inflammation, that inflammation would have gone on and killed him; then what would the world have said? Why, a partisan would have said, all this is owing to your not having elevated the depressed bone. Now, suppose the depression had been elevated, inflammation would still have come on, and if it had not been subdued it would have killed the patient; and the partisan on the other side would have said, all this is the effect of your operation! It is right, therefore, that you should have an unprejudiced mind, so as to be able to judge fairly in these cases. However, I tell you that that man was bled occasionally afterwards, and that he did perfectly well. Being desirous of knowing whether the pressure might have any particular effect on his brain, I did what Hippocrates would have done, I inquired about his dreams, but he did not dream at all; he slept soundly, but did not dream. I made him stand on his head to see whether that would bother him at all, but it did not. I am sure the functions of that man's brain were in no way injured by depression. John Bell has published a book upon this, and he heads one of the lectures in this way, "Abernethy dislikes the trephine," making a partisan of me! I am no partisan at all. Abernethy dislike the trephine indeed! I never said any such thing.

Now be aware of this, that all fractures of the skull are to be divided like the fractures of other bones, into simple and compound fractures. And where there is a compound fracture, O, there may be exfoliation, and there may be a great deal of irritation about the bone, and the action about the bony scalp

will produce irritation in the more internal parts. To show you, in the briefest manner I can, what I mean, I will relate to you this case. A woman was brought into the hospital one night, who was struck with the leaden head of a cane upon the parietal bone, which pressed in the scalp and depressed the bone. There was a circle made, a little bigger than a shilling mayhap, but it was depressed about the quarter of an inch, and starred in many pieces. I was sent for in the middle of the night to see this accident, and I really did not know what to make of the woman. I could not tell whether she laboured under symptoms of compression, concussion, or intoxication. I did not know which was the most predominant. However, finding that the bone was broken in this way, I took it all away; I put on a small trephine, and took away all the starred parts; then cleaning the *dura mater*, I laid the trephine down, put on a dressing of a little plaster, with a little bandage to make pressure against the part that had been cut; she was bled, a little physic was given to her, and on coming to the hospital the next day, I said, "How is the woman, I hope she has not taken off the dressing." "We know nothing about her," was the reply, "for she will let nobody near her;" and when I entered the ward, then she said, "Ah, now he is come into the ward, you may look if you please;" showing that the woman was more drunk than any thing else. However, that case did well. And why is the operation of the trephine so deleterious? Why, because by performing it you are obliged additionally to injure the parts. You cut away the parts; you additionally injure the bone; you cut, you do additional injury, for you leave a gap where the dura mater is unsupported; where the dura mater does not receive the support from the bone it ought to do, and the consequence of all this is, that when inflammation comes on in the brain, as it must of necessity do after a violent injury, there would be such an augmentation of the vessels from turgescence, that the brain will be thrust up and protruded through the aperture. O, that is a very serious thing, and certainly never, never, was more judicious advice given to surgeons, than that which was given by Mr. Minors, of Birmingham. He trephined a boy, took away a piece of bone, closed the scalp, bled and purged the boy, and the whole united by adhesion.

Now I tell you the cases which bear out the different lectures; you are to put them together, and govern yourselves

accordingly. There was a man who was taken ill in this way from an abscess in the brain, and who had neither concussion nor compression, but simply the removal of the integuments from the os frontis. Well, now I tell you just one other case. There was a man in the neighbourhood where I lived, who was cleaning an old fashioned carriage belonging to an old lady, an old fashioned lady; and this fellow had got on a ladder to clean the glass; the ladder slipped, and the chap's head went bolt through the large square of plate glass of the window. The edge of the glass cut him across the scalp, and turned down the flap towards his ear; he bled profusely; he bound his napkin round his head, and ran into the house where I lived. I tied a branch of an artery that was puffing away very copiously, replaced the scalp, and advised him to come to the hospital, for I knew that if he was taken to the old woman's house, she would be in a most horrible state of alarm, so his going there would do mischief, and that probably he could not get such good advice and attention at her house as at the hospital, therefore I told him to come here; and here he was, I believe, very kindly and judiciously treated. Now here there was no stunning, no touching of the bone, it was merely a wound of the scalp. He was shaved; it seemed to do well; the wound was in a great measure healed, but that which came from it was *fœtid*. Then he got symptoms of cerebral irritation; he afterwards got better; the cerebral symptoms came on again, and it was upon that second attack that he died. He died of inflammation of the brain; the pia mater was inflamed and thickened to an enormous degree, and the thickening went into the ventricles of the brain. I say, then, that that shows the great sympathy existing between the containing and contained parts, and it is that which renders the operation of the trephine so far injurious, besides leaving the gap in the bone, in the case of trephine, when inflammation is likely to come on, and effusion.

In doubtful cases, said an Irish surgeon of the name of O'Halloran—and doubtful cases there must be,—it seems wisest to leave the depression till the inflammation, which will necessarily come on, is subdued; and then if depression of the bone is found to produce torpor, or, in any way to affect the functions of the brain, when the inflammation, the consequence of the injury, the consequence of the concussion, is gone off, why then, said he, you may trephine the bone, and

you will only have to encounter that inflammation which is the result of the operation; whereas, if you trephine immediately, you will have to encounter the inflammation which is the result of the injury, in addition to that which is occasioned by the operation. Now, as to that practice of O'Halloran, he has shown the practicability of it, and I have seen it, in many cases, showing it to be a good practice in doubtful cases; but, I say, there are cases in which you are called upon to trephine instantly; and, I say, that in those cases, much, of course, will depend on the constitution of the patient; if he be a countryman, I should not mind trephining him—if he lived in the country; but, I say, that will not do in people of unhealthy constitutions, or in the crowded hospitals of London.

Well, then, to elevate a depressed bone, you trephine, and Hey's saw may be used.

Thirdly, and lastly, you trephine, to let out matter collected beneath the bone. Now this cannot require the sudden operation after the accident, for you must wait till the matter is formed, and that may be at an uncertain time. Mr. Pott has described the cases, but, to tell you the honest truth, I have seen but very few of them, and, therefore, you had better read Mr. Pott's book. He says of those cases, that after a time the person's head aches all over, that the matter forms beneath the bone, that the bone perishes in consequence, that if there is a wound you see the bone ash-coloured, and like a table, that if there is no wound there is a puffiness, and that then you are to trephine. You may make out the description of the cases, all that relates to the cause of this operation for the removal of the pressure, (and that is the only thing you perform the operation for,) by reading Mr. Pott's book, so that I need not detain you much longer, and yet I should like to finish the subject while we are about it.

Say that you do trephine a person—say that there is no blood under the skull, are you warranted in opening the dura mater? If it was thrust up into the aperture, if it was purple, egad, you must do it—you must go on; but I say, O, that is a horrible thing, because it is like a protruding wound into the cavity of the abdomen; you do a thing which must necessarily bring on inflammation in the parts which imbed the brain—you must bring on inflammation. Say you must do it some time after the accident, well, you let out the blood; nay, it may be all over the surface, and you may let out the serum, but you

cannot let out the coagulated blood; for my own part, I should be very averse ever to divide the dura mater; but I will tell you a contrary case which was sent up to me here by a pupil—it was a lucky case: there was a woman knocked down, and her scalp was divided somewhere about the upper part of the parietal bone; this happened in a small town; two or three doctors were sent for, who, I suppose, were not at home when the messengers arrived, for they came in afterwards one after the other. The two that had arrived before him had declared that no operation should be performed, because the bone was not broken. The woman was apoplectic, had a labouring pulse, stertorous breathing, and so on, and this chap said, these are decided symptoms of pressure, and I will trephine; I will take the responsibility upon myself, and will trephine. Egad, he did trephine the woman, and there was no blood under the bone; he divided the dura mater with a lancet, and let out about six ounces of fluid blood; and as soon as that was done the woman bolted upright in bed, and cried out—What the devil are you all about? Why, what are you all doing? I think he said she raised herself up like a lion, so suddenly does the brain resume its functions. But this was a lucky hit; it is not a warrant for the division of the dura mater in general practice.

Well, I have only one word more to say, which is, that there are some cases of injury to the head which would puzzle the devil himself, I believe, and those are the cases of fractures of the base of the skull. If a man pitch on his head, it is true he gets a hard blow on the top of the skull, but the arch-like form saves that part of the skull, in a great measure, from injury, and he has a harder blow on the base of the skull, from the weight of his body falling upon his head, and which occasions a crack at the bottom of the skull. Well, I say, those cases are attended with a train of symptoms that would puzzle any body. They are a mixture; sometimes like compression, sometimes like concussion; in short, they are a sort of mixed case, and they generally terminate fatally, though I am convinced that I have seen a case of fractured base of the skull do well. I could tell you the case; but I can assure you that such cases are not likely to do well in general. The general opinion of surgeons is, that they are not likely to do well, and I am sure I am of a similar sentiment; but I have to tell you that I have known a case of fracture of the base of the skull where the

person has recovered to that degree, that he has gone out of the hospital, that he has been taken ill again, that he has returned to the hospital and died, and that upon an examination of his head taking place, it was found he had a fractured base of the skull. Well, I put them down as perplexing cases, but I have told you how the accident occurs; I have told you what enabled us to discriminate that the case I have related was a fracture of the base of the skull; and of course in such cases you must pursue the most cautious and judicious treatment for a considerable length of time, till you are assured that the injury is repaired, and that therefore there is no likelihood of any fresh occurrence of irritation from the accident.

LECTURE XLIII.

THE EYE.

I now go to speak of opertions done about the eye; and first, of

Cataract.—This term is applied to opacity of the *crystalline lens*, or of its capsule; which prevents the rays of light passing to the retina, and consequently prevents sight.

A capsular cataract I have nothing to say about; that seems to be a disease that may get well.

Of the opacity of the crystalline body I have to say, that that may become opaque without losing its natural texture—that is, a firm cataract, or it may become opaque and lose also its natural texture, being more or less broken down, and that is called a soft cataract. Now the observation of this fact led the old surgeons to suppose that there was a ripe and an unripe state of the cataract. They thought the firm was the ripe cataract, and the soft the unripe; but it depends upon the different nature of the disease with which the eye is affected, and that which is softened will never become firm by residence. There are, too, those who pretend to tell you whether the cataract is firm or soft by the look; I do not put any faith in that. I should place my faith, judging of the nature of the cataract, by the history of the case; and if I were to select a

person on whom I should choose to operate for the cataract, it should be a person who had a free mobility of the *iris;* that would give me an assurance that it was the lens only, and not the capsule that was affected; for where the disease is followed with any inflammatory action, and the capsule becomes affected, the iris is apt to adhere to it; the free motion of the iris, therefore, would seem to imply to me that the disease was in the crystalline lens only. I should choose a person who could see the transit of a candle through the room, or tell me where the windows were, because there can be no opacity in the crystalline lens so opaque as not to give you the idea of the situation of luminous bodies, and that would prove to me that the retina possessed its due powers.

Now if this body be opaque, the operations which surgeons have devised for putting it out of that situation, where it does impede the transmission of the rays of light to the retina, were, first, to depress it, to lodge it at the bottom of the *vitreous humour;* that was called the operation of couching the crystalline lens, or of depression. It began at the time of Celsus, and the term I should like to have expunged from the Surgical Dictionary. Formerly they knew not what they did in this operation. If you put an instrument into the vitreous humour, divide the capsule of the lens, break the vessels which supply the lens, divide it with the needle, suffer the water of the vitreous humour to become commixed with it, O, you render it, as I may say, meet for absorption; you kill it; it is no longer supplied with the necessary vessels; it is a dead part; its texture may be softened by the water that gets access to it, but the absorbents take it away, even though you do not depress it to the bottom of the vitreous humour. If you did depress it to the bottom of the vitreous humour, you would not only kill it, but leave it in the place where it must become absorbed, and in a place where it would not impede the transit of the light, so that you await its gradual removal; but if the cataract was not firm you could not depress it, or if you did you would effectually destroy it, and the parts would be gradually absorbed. Of late, Mr. Hey, of Leeds, has advised this operation: He has said, that by employing a very fine needle he can do it without destroying the eye; that people are not adverse to submit to repeated operations in this way, and that he has never failed to render the eye transparent. Now the old couching needles were large ones, but Mr. Hey

employed a small one. If you, however, meditate operations of this kind, you must read books upon the subject; you must read, and of course you will read, Mr. HEY's works. I now show you the sort of needle that he employs, and the operation is really a very innocent one. You plunge the needle through the sclerotic coat, through the choroid coat, and at a certain distance from the cornea, as far as you can from the cornea, because the degree of inflammation is always in proportion to what you do to the front of the choroid, the *processus cilliaris*, or iris. Bring the front forwards so as to divide the back part of the capsule of the crystalline. Well, you divide the back part of the capsule; you put your needle into it, it is firm, you stick your needle into it; then you had better just draw it back and depress it, and lodge it in the bottom of the vitreous humour, if not, you proceed as I have said. Now, Mr. SAUNDERS brought the same operation into practice, performed from the front of the eye; but he was particularly induced to do it on children who were born with cataract, with a malformation of the lens. There is such unsteadiness in the eye of persons when they have never been accustomed to guard objects, and to fix the eye, that really the best operators would be puzzled in performing the extraction; and therefore, those were patients that Mr. SAUNDERS operated upon in the manner I shall next explain to you. If you let the children grow up with the cataracts, who have been born blind, they are cut off from one of the great inroads of knowledge to a mind, by being deprived of sight. Then Mr. SAUNDERS applied belladonna to the eye-brow, enlarged the pupil, put in a curved small instrument through the cornea, and divided the front of the capsule of the lens; I mean, that which it derives from the membrane of the vitreous humour, and even its proper capsule, and a creamy sort of fluid oozed out and commixed with the cataract, and he drew it out; but he did that which destroyed the vitality of the capsule, destroyed the vitality of the part, broke its vessels, suffered the water of the vitreous humour to commix with the cataract so as to have softened it, and it was absorbed. The success attending that operation induced him to practise it upon persons more advanced in life, where he believed the cataract to be fluid, and the two operations are now distinguished by the posterior and anterior operation for the cataract. It is the same operation; it consists in making a division of the back of the capsule of the lens, or of the front

of the capsule of the lens, allowing the water to mix with the cataract, as it were to soften it; whether it does so or not I cannot tell, but it assists in destroying it. In other instances you cut up the lens. Now, if Mr. SAUNDERS had lived, the world would have had a full, true, and particular account of the merits of this operation, because Mr. SAUNDERS was a candid man, and he would have told nothing but the truth; as it is, he unluckily did not live long enough to give any decided opinion upon the operation. When he cut up a lens that was partly solid, and partly absorbed, he sometimes left portions to come into the anterior chamber of the eye, and he found that if any firm portion of lens got into the anterior chamber of the eye, it produced inflammation; and therefore he seemed to think, that the better way was to cut the lens fairly, to let the aqueous humour out to commix with the cataract. He seemed to be of opinion, that where the cataract was a solid cataract, the extraction was far better than depression; and I have no doubt about it in my own mind, from the little I have seen. I grant what Mr. HEY, of Leeds, has said, to the fullest extent that can be demanded, that you may so conduct the posterior operation, as to render the part behind the pupil transparent; but I ask, do they see as well as when the extraction of the lens has been fortunately brought about? And to that I should answer, certainly not.

Now, then, the extraction of the lens is a very difficult operation, very difficult. I need not say so, perhaps, but it certainly is an operation that no one can perform, but in consequence of possessing that sort of firmness of mind which is the offspring of success. If a man sets about it hesitatingly he makes some blunder or another; and, therefore, it is an operation which always should fall to the lot of surgeons who have been in the habit of practising it.

Suppose the cornea is a circle, though it is not quite so; then the object is to cut the cornea to the extent of about half its surface, and as close to the sclerotica as you can. Mr. WARDROP has said, you cut it to too great an extent; but it is of much consequence that it should be cut largely to give easy access to the lens. It should be more than a half, suppose we say four-fifths; they may differ about that, but I know that as the flaps are considerable, so will the future steps be conducted without injury.

Now I have seen many people operate for the cataract, many

of great repute, but I never saw many operate like Mr. PHIPPS. Egad, when I saw him first operate, I thought he would have scooped the eye right out. His knife was a large knife; it is not here, but I show you Mr. WENZEL's knife; it is very pointed. A pointed knife will penetrate the cornea, and go across the cornea sooner than any other; but if it be too long, the point is apt to interfere with the internal canthus of the eyelid before the broad part gets to it; therefore Mr. WARE used another sort of knife, and his account of it should be read. He was a man of great experience. It is the same sort of knife, but not made so pointed. I say Mr. PHIPPS used an extremely broad knife: he stuck it through the cornea, and carried it across it with an address that was surprising. When it has been carried right across and appears at the opposite side, the operation may be said to be achieved, for the eye is then fixed upon the instrument, and it will never move any more. If you go hesitatingly to work, and the eye starts about, and the aqueous humour escapes, then every thing is bad; but to do the operation as it ought to be done, you should fairly introduce the instrument, carry it right across the eye, fill up the parts, and then you may say all is done.

Now I wish to show you that my opinion respecting the form of the knife is; that the form does not so much signify: suppose the point of the knife to cut to a certain extent; in going through, take a knife of any shape you please; if you can get it through, as it goes through it cuts so much of the cornea, and then if you carry it on, it cuts the rest in a semicircular shape; it crosses it regularly, and cuts the rest of the cornea, and divides the lens. But Mr. PHIPPS never completely divided the cornea with his knife: when he had cut it all to a sort of filament, leaving only a small portion, he withdrew the knife from the eye, and put down the eyelid; that I take to be a very capital thing, a capital mode of proceeding, for think of the situation of the patient: he has his eye fixed, all the muscles are in strong action, and if you complete the section of the cornea in a slap-dash way, egad, the muscles will force the capsule, and burst the vitreous humour all out in a gush; but Mr. PHIPPS did not do that. Well, then he had another little knife which he put into the eye, and divided the filament. He soothed the operation by saying to the patient, "Now, sir, the operation is achieved, and I am certain I will extract the cataract, only do not you make any further effort

with the eye." Then he had the eyelid lifted gently up by the assistant, he pressed the under eyelid, he put in the small pointed knife, and divided that filament, or part, which he had left undivided, and then pressed the eyelid down again. I had that instrument here formerly, but I am sure I do not know who has been so very obliging as to get hold of it with some others. This having been done, and the eyelid once more lifted up, the least sort of pressure will cause the solid lens to spring out, which you may catch in a bottle. Then the flap of the cornea is put down accurately; the eyelid is closed over it, a compress laid upon it, a wet compress and a bandage, and it is necessary to confine both eyes, because, as the motions of the eyes are corresponding, if a man is left to see with one eye, the other eye will move. After this is done, in sixty hours the union of the flap is complete. Upon opening the eye, you see nothing more than a sort of white opaque scar, and you find the cornea distended. A sort of common ophthalmia will come on after that, which is to be treated in the usual manner.

Well, such is the operation, and I need not have shown you the instruments without showing you the operation; but it is impossible to demonstrate it upon the dead subject. Now so much for that, and next for the

Fistula Lachrymalis.—I really think the case of fistula lachrymalis comes on in a manner that strongly impresses upon my mind that there is a stricture in the *ductus nasalis*, that is to say, a person has an uneasiness in the corner of the eye; a little uneasiness from a division of the lachrymal bag, and he naturally puts his finger up to press a little upon it, and to force the stuff down into the nose, by which means he relieves the uneasiness. Now if this affords relief, it is, of course, an easy mode of relieving a person; and finding relief in this way, people suffer the case to go to a considerable extent, and yet do not require an operation. They have found a mode of relieving themselves, and they adopt it; they do not consult any body. But after a time, they resort to the usual expedient, and pressing upon the sac, the stuff does not go down into the nose, as it did before, but it regurges by the *puncta lachrymalia*, comes out in their hand, and they think something has burst; then is it that they first apply to a surgeon. I read that there are some cases of fistula lachrymalis reduced by inflammation of the lachrymal bag, and by secretion of stuff that does not discharge itself into the nose. It may be so, for anything

that I know to the contrary; but I say it comes on in a way, in the generality of cases, that induces the supposition, that there is a cataract in the lachrymal bag.

Now, to relieve this, it has been proposed to syringe the lachrymal bag, by putting instruments down the ductus nasalis. In such a case, the instrument I now show you ought to be used. But can you puncta, it may be inquired, from the punctum lachrymalia into the lachrymal bag? O, yes, it is easily done: you may pass the instrument from the superior or inferior punctum into the lachrymal bag; and having got it there, by raising one end of the instrument, the other will go down into the nose. But I do not think this is likely to do any efficient good; it is right, however, that you should do this, and practise this, if it were but to know whether the ductus, leading from the puncta into the lachrymal bag, be *potent* or not; for there are some cases of water of the eye, where the tears would never go into the lachrymal bag. O'Neill proposed to syringe this case: in doing so, he put the pipe into the inferior punctum, closed the other, and if you throw in water, and with a considerable force, you can wash out the bag, and, therefore, it is an instrument every surgeon ought to be possessed of.

Well, now, under these circumstances you see the beginning of cases of fistula lachrymalis, where the tears sometimes pass into the nose, and where they sometimes do not. I have represented the circumstances of the cases at the beginning, you remember them, and I need not repeat them. In one case, Sir William Blizard proposed to fill the lachrymal bag with quicksilver: that is an operation I have often practised, and upon my honour, I believe it to be a very good one. You press out the tears from the lachrymal bag by force with your finger—empty it of its contents; then you inject the lachrymal bag with quicksilver, just in the manner in which you would inject absorbents in the old way of injecting those vessels: get a fine pipe, such as will readily go into the puncta lachrymalia, with a stop cock; fill the tube with quicksilver, put the point of it into the inferior punctum, let an assistant turn the stop cock, and you inject the lachrymal bag with quicksilver. When it is quite full, it runs out of the superior punctum, and then you withdraw the instrument. Then you say to the patient, now do not put your finger to the eye—do not make any pressure on it. The quicksilver is a very in-

nocent fluid, it produces no inflammation in the lachrymal bag; it is a very subtile fluid, and if there is the least opening in the lachrymal bag, it will find its way out; it is very weighty, and consequently, always has a gravity down through the lachrymal duct. You may call in three days hence, and without the eye having been touched, you will find there is no quicksilver in the bag, it has all gone down through the nose. You fill it again, and you call in three days afterwards, and the patient knows very well that the quicksilver went down in a very short time. At last, as you fill the bag, you will see the quicksilver running out at the nose, and then you can do no more—that is the end that is accomplished, by treating the case in its incipient stage. But will good come out? O, that I am sure there will, and more especially if you attend to the patient's constitution as you should do; for I say, all irritation about the nose is very much connected with stomachic irritation. Then will good come of it? Yes, as I can testify in a number of cases; and to prove to you that I do not speak to you without authority on this subject, I tell you the following case: I was sent for to see a patient in St. George's Fields, or somewhere thereabouts, and I saw a woman with a very erysipelatous inflammation in the eye. I said to her, O dear, Madam, let me look at your tongue. She put it out. A pretty state of health you must be in. Well, all you can do will be to soothe the disease; it will get well, and you must be particularly attentive to the management of your bowels and so forth. She said, you do not seem to recollect me. O, if I had done as you advised me to do this never would have happened. Do not you remember, that about fifteen or sixteen years ago, you used to put quicksilver into my eye? I called one morning and you told me I had not done it enough, and therefore ought to continue it a little longer; but I was so well that I thought nothing at all about it, and discontinued it; if I had not discontinued it, this would not have happened to me I am sure. A medical man was present, and he said, I will tell you another very curious thing, which is, that when the bag burst —for the lachrymal bag had ulcerated and burst—there were some globules of quicksilver mixed with the pus. Well, I tell you the facts of the case: she was so much relieved by the introduction of quicksilver, that she did not choose to have it done any more, and the conviction of her mind was, that if it had been done effectually, she would have been saved this subsequent trouble.

But suppose these measures are unavailing? O, at last the lachrymal bag will become inflamed and will break like an abscess. You know this is the state in which people generally apply to you; and this is the state in which people require the operation for fistula lachrymalis, which I should always perform in the same manner exactly, whether the bag was burst or not burst; and what is the manner? O, it has been told you in the course of the lectures. If you put a knife below the tendon of the orbicularis palpebrarum and within the edge of the orbit, and carry it downwards, you cut the whole front of the lachrymal bag as far as the lachrymal groove will let you; you cannot carry on your knife beyond a certain extent; you make a semicircular incision in the form of a segment, and leave a pretty wide gap in the front of the sac. Now you may say, I can pass a probe down and clear away the obstruction without dividing the bag; but the cause of the stoppage is a thick sort of mucus, and if there is not a free opening, that sort of stuff distends the bag and keeps up irritation. I should always like to have the front of the lachrymal bag fairly divided, whether there was an opening in it or not prior to my proceeding in the operation, which would be effected by putting a probe down the *ductus nasalis*. Well, upon doing this, a drop or so of blood may issue from the patient's nose. Then I should put in a bougie, so as to fill up the lachrymal duct, to bring it to its full development; turn it down and put a little bit of plaster on it. I say I would put in a bougie to bring it to its full dimensions; then, after that, I would put in a WARE's nail-headed style, as he calls it; that is, I would put in a style that would remain in for an unlimited time without annoyance to the patient. The wound would close round the style, and there would be an aperture left like a pin's hole. It might be taken out every morning and put in again; children can do it for themselves, and the aperture can be maintained in this way till it is, as it were, perfectly fistulous, having no disposition to close. Mr. WARE says, he has taken this style out after it has remained in for a considerable time—three or four months—that there has a contraction taken place again, a necessity for a new operation, which he has performed, and some time afterward he has proposed taking away the style again, but the patients have said, "No, no, thank you, I am very well, and this occasions me but very little inconvenience." You know you blacken the style by heating and rubbing it on

a little sealing wax, so that there is no other eye-sore from it than would be occasioned from having a little bit of black sticking-plaster on the side of your nose. You had better read Mr. POTT on the subject, if you please ; he tells you to keep in the style as long as you can, for if you take it out too soon the duct may close again. Well, with this instrument, you can keep the duct open for a considerable time ; and I have seen many of those cases in which the inclination to contract has been so great as not to allow the tears to trickle down with the style in ; in such cases I should not put in a bougie, but I say that constitutional treatment should go hand in hand with the other treatment.

Well, there are those who say you had better break down the *os unguis*, and there is a ground for this assertion. God knows, it is easily done, and by doing so the tears will run at once from the back of the bag into the nose ; but I am persuaded that the natural channel is the best ; that it is placed in the best possible direction, being perpendicular, and I cannot foresee a case in which a person may not restore the natural channel, therefore I should not be inclined to break down the os unguis. Mr. HUNTER himself proposed to trephine the os unguis with a small trephine, because, says he, that the holes in a person's cranium grow up again. But, for my own part, I cannot believe but that you may restore the natural channel, and I know that such a course is adopted throughout the kingdom with success, for I see it in different places where I go.

There have been cases that have baffled surgeons, where they have been disposed to close up the lachrymal bag altogether; there have been such cases. I hope such cases will not occur again, for I hope the practice of surgery will improve and the disposition to such cases not occur ; but there have been such cases, and the natural channel has been abolished. Now what a hardy operation does that appear to be ! But I mention it just to lead me to say, that what a person might think would be the natural result of such an operation really does not occur. They think the natural result of closing the natural passage into the nose would be, that the tears would be continually running down the cheeks ; but that is not the case. At first a man may have every now and then to wipe his cheek with his handkerchief, but after a time he has very seldom occasion to do that.

Polypus. Now I go on to speak of operations about the

nose; and here I say I have nothing to say about that blackguard operation of extracting a polypus from the nose. I call it a blackguard operation, for they pull it away. There are some surgeons who say they have more refined notions, and they think they could cut them away; but I am sure I do not know how this is to be done. Those polypi we have met with are more or less of a tumour. Sometimes there is one, sometimes there are many, and at last they grow so as to fill the tube of the nostril, the bottom of it, which is a sort of tubercular cavity, the inferior meatus, that is, the passage through which we breathe; and whenever that is plugged up, it is very inconvenient to breathe at all. People lie with their mouths open, the fauces get dried, they are greatly inconvenienced, and then we try to cure the disease, and to restore the passage in the nostril by pulling those polypi out. They grow to correspond with the floor of the nostrils. You pass the instrument as far down as the pharynx, and then one of those things falling down into forceps, you catch it and pull it away. If there are many, the first comes first, which you pull away, and you go on pulling them out until the patient can breathe through his nose, that is all you can do. I say it is as blackguard and unscientific an operation as any I know; but yet it is the only one I know of that will answer the purpose; and I have only to say, have your forceps made with what they call nut-cracker teeth. Polypi are such slippery things they will not be held without, but I have never had them slip out of forceps with nut-cracker teeth. The clearing of the floor of the nostrils, which is the object of the operation, so as to give a tubular passage to breathe through, is an operation which a surgeon alone can perform.

Now the consequence of these polypi? I say every disease connected with the pituitary membrane is connected with the state of the constitution. (Laughter.) Aye, people may laugh as they please, but it is an unquestionable fact. I want to make you feel as I do, and I know not how I can do that, but by telling you cases:—There was a man who was an artist, and a very clever fellow he was; he drew likenesses: he was a man of peculiar talent, and JOHN HUNTER used to clear his nose of polypi. Any poor man of talent, JOHN HUNTER was sure to pay every possible attention to, and more than he would pay to a rich man. When Mr. HUNTER died, the artist used to come to me, and I did what Mr. HUNTER would have done, I used to

clear his nose out. Mr. HUNTER had tried a number of things with a view to suppress the growth of these polypi in this man's nose, but nothing would do any good. Snuffing up things, and the application of ointments, really seemed to me to produce more fidgettiness, and more harm than good. But after this course of conduct had gone on for some time, the man began to talk to me, saying, he felt himself very unwell, and that really it was a great annoyance to him in his business. He was much dejected in spirits, and he said to me, "If a man comes to have his portrait taken, and I set down to sketch it, egad, the blue devils will come before me, hover about me, whisper in my ear, and, in short, render it impossible for me to draw the likeness. I am induced to say, I hope you will do me the favour to call on another day." "O, that is the case, is it? Let me look at your tongue. O, your stomach is all wrong." Well, I gave him a lecture about his bowels, and from that time he never had a polypus extracted from his nose for many years. Many years afterwards he fell down, as I believe, on the neck of his thigh bone, and he was laid up for a long while; after that he once called upon me to have the polypi extracted again, and that is all I know of the case; but I could tell you a great number of similar ones.

Hæmorrhage.—Well, then comes the business of what is to be done in hæmorrhage from the nose, but hæmorrhage from the nose is the result of the same cause. Hæmorrhage to a certain amount from the nose is good, but it sometimes goes on to an enormous extent. HALLER has given a chapter on the immense quantity of blood that has been lost in hæmorrhage from the nose, wherein he endeavours to estimate the quantity of blood that probably may be circulating in our vessels. He says it must be considerable, since such an immense quantity has been lost by hæmorrhage from the nose. Now, there are no vessels in the nose that would bleed in the manner I allude to, unless there was an irritation kept up, unless there was something keeping up hæmorrhagic action. I grant the vessels to be large and numerous; I grant the pituitary membrane to be excessively vascular; you know when it is injected it looks as red as the injection itself; but still I say, there must be inflammatory action to produce such hæmorrhage, and when people are quite faint from the loss of blood, a surgeon is called in to stop it. How is he to do that? Why, they have talked of doing it in this way,—any one can stop it from the front you

know, but they say you are to stop it by plugging up the pharynx. Put a tube through the nose as far as the pharynx, then put down a piece of elastic steel, catch it and bring it into the mouth, and tie a piece of sponge to it adequate to fill up the posterior opening of the nostril, then draw it in and plug up the opening. Now, that this can be done sometimes I know, because I have seen it done; but upon my honour, they must be cleverer people than I am that will do it—cleverer or more patient, I do not know which it is. Whenever I have tried to do it, I have never been able to accomplish it. Of course I can do it very well on the dead subject. I could do it too on the living subject, if these *levatores palati* would be quiet, but they will not, and I have not been able to do it. Well then, I have never seen a man with a bleeding from the nose—and I have seen plenty of them—that could not have that bleeding stopped by plugging up the nose, introducing lint from the front. You make a cylinder of lint adequate to fill the tubular part of the nostril; you wrap it round a probe, making it long enough to go from the front aperture of the nose to the back of the aperture; you wet it, twist it round, make it something like a large bougie, make it hard, and then you introduce it from the front aperture, carrying it along the floor of the nose till you are sure it has reached the back aperture. Now, I never saw hæmorrhage take place after that was done. It does not come into the throat, and yet it seems to plug up the back apertures. When you put the plug in you withdraw the probe.

Now, to show you how careful I am not to tell lies in these lectures, I will tell you what happened to me; for about half a dozen years, I said, I never knew but one instance in which this treatment failed. I had said this for about five or six seasons, when a man called upon me one morning, to ask my opinion about some trumpery disease he had. I told him, as well as I could, what he should do for it; but he said, O, I called upon you, sir, for I owe my life you; I should have died of that hæmorrhage, if it had not been for the plug you put into my nose—that is all he said. I immediately exclaimed, what is all that about, for I know nothing about it. Now, he was the individual in whose case I thought the treatment had failed, for somebody had told me it did, but he himself then told me it did not; and I am confident that that treatment will stop the hæmorrhage. But it is a very unpleasant sort of proceeding; it requires some resolution to make the plug large

enough, and likewise to press it till you get it into the back aperture.

I remember a neighbour of my own, who had a country house about thirty miles off, and he had hæmorrhage from the nose to a very great degree. He sent for me, and I went down to see him. I found him as pale as a corpse, sitting in a cold room in the winter time, and waiting the return of the hæmorrhage every moment. I said, well now, this is not a very comfortable room, and so you had better come into the other room, where there is a fire. I should like it better; and I should think that, in your state, you might take a cup of tea—of warm tea, or something of that kind. *Ice* seems to me to be very cold. Yes, but, said he, this hæmorrhage will come on again. O, that is just what I want; I just want it to come on again, and when it does, depend upon it I will stop it. He had not been near the fire, and taken the tea long, before gush came the blood again. I plugged it in the manner I have described; he did not think it possible that it could be suppressed, but I stayed all that night in his house, and not a drop more of blood issued. Now the plug remains in for three or four days, and a nasty stinking piece of business it is. Let it stay in for three or four days, and, during that time, you ought to be employed in putting the alimentary organs to rights; then withdraw the plug.

Well, with regard to the nose, we may as well say that there may be tumours in it, which you may tie. At any rate, talking of tumours in a narrow cavity, leads me to speak of the means, or of some of the means—for there are ten thousand of them, as you will see by reading books of surgery—by which a tumour may be tied in a narrow cavity. Where tumours have extended bases, and you tie the bases, it is done by puttting threads through the different parts, and connecting them together, so as to make separate ligatures at the tying of each part of the base of the tumour. Well, now, knowing the expedient I allude to, I may proceed without saying any more about that, it is not worth detaining you upon it, for such a thing is not done now-a-days; it was done at the time when surgeons were horribly afraid of hæmorrhage. For the tying of tumours from the uterus Leveret invented a double canula, with a silver wire, which he twisted round the tumour. Hildanus invented a ring, with a thread to go round the tumour. Leveret's double canula seems to have all the advantages of Hildanus's ring, in maintaining and cutting the tumour in a narrow cavity; and it

may be used with thread, as well as with wire, if you put your finger into the cavity ; and it seems to me to be an instrument that will stand. But LEVERET himself did not seem very fully to understand what should be done with it.

LECTURE XLIV.

ON THE EAR.

Now, then, I may say, with respect to operations about the ear, that syringing the ear is necessary, if there is wax in it. If there be wax in it, it may be got out by syringing, but depend upon it you will never syringe it successfully with an elastic bottle—it must be done with a brass syringe. Quacks and aurists get reputation for this when surgeons lose it, not because the quack has more knowledge of his profession, but because he takes more pains than the surgeon. There was an inquiry set on foot to know what was the best solvent of wax, and water was found to be the best. Where there is wax in the ear, you should endeavour to put in a little water with a camel hair pencil to soften it ; but you should also have a brass syringe, and holding the pinna of the ear, you must throw it down with very great force indeed, and you must do this repeatedly before you can remove the wax. I have seen wax so empacted on the table of the ear, that if you had taken a little piston, and tried to ram it down into the ear as full as you could, you could never have put a particle more into it, and after having been syringing for a long time, I have got away a large flake of wax that lay upon the tympanum. That flake prevented the vibration of sound upon the tympanum.

I only want to impress upon your minds, that the syringing ought to be done attentively, and by the best contrivance. Well, then, there is a *speculum auris*. I am not one of those who are fond of many instruments, but the speculum auris will expand the ear. I do not know that it is an instrument of any great importance, but it is one of the instruments that is used in this proceeding.

Then you have instruments for getting out bodies, foreign

bodies that may have got into the ear, and been pushed into its base: beans, for instance, and things of that kind. Now they contrived to make a pair of forceps with separate legs, and they say, if you can get one leg behind the body, you will keep it from receding, and if you put the other over it, you keep it between a sort of forceps, and by sliding down a ring upon them, you make them gripe with more force. There are other contrivances for the purpose. This was the invention of somebody in the hospital, but I forget of whom, to get the leg of the forceps to go past the body, because if you take a common pair of forceps, you will push the body further in.

Now I remember, not a very great many years ago, that there were paragraphs put into the newspapers, about a child having got something into its ear, which could not be got out. The poor child was in great agony, and it was said that this foreign body produced irritation in the child's brain, and I do not know what all. As the British public are very much alive to humanity, there were, morning after morning, in the papers, paragraphs exhorting the friends of this family not to be afraid, for the body would come out. To these paragraphs, I confess to you, I added one myself, and it was to the following effect:—A child got a bead from its mother's necklace; it was a large bead, and in playing with it, it put it into its ear. In trying to get it out, she only poked it further down. In others attempting the same thing, they only made bad worse in the same manner. A surgeon was sent for, but this large bead had got so far down into the tube of the ear, that the surgeon said at first he did not believe it was there; being assured that it was, he put down a probe, and felt that there was something in it. The child was a good-natured child, and the surgeon said to it, Now I wish you would just lie down upon that ear; put your head upon this cushion, it was a large soft cushion belonging to a fashionable sofa. The child did so. He took up another cushion, and put it upon the opposite ear. Now, said he, I will strike the opposite one, and tell me if it hurts you. The child said, No, it does not hurt me at all. He therefore kept thumping the cushion that was lying on the child's head for eight or ten times, and then he said, Now let me look at your ear. He looked at it, and then said, Now I see it plainly enough. On repeating the operation, the bead came out. And now I really believe that that is the best way of getting those things out.

In speaking of the diseases of the ear, I have cautioned the students not to be extracting excrescences from the tube of the ear, and I trust they will remember what was said on that subject. I have known cerebral affections arise from the irritation of such operations in the tube of the ear; nay, indeed, the disease affects the petrous part of the temporal bone, and leads to cerebral irritation in many cases, and you may aggravate it by operations of this sort. However, I need not repeat what has been formerly said on the subject.

ON THE MOUTH.

I proceed to speak about operations performed in the mouth; and you know we have to take off fungus that may spring from the alveolary processes, and many jobs of this sort, which it is difficult to do with a knife. I think, in many cases, you will find the farrier's drawing knife an exceedingly good instrument to take off fungus in the mouth, and so I keep one of them. If it is osseous, and would haggle a knife, you may have a knife which is a saw, and take it off with that. I have taken off excrescences with an instrument of this kind from the palate and other parts of the mouth. I keep a trephine for perforating the antrum. All that is necessary has been said about the necessity of doing this. There is no need of a central pin, or any thing to steady the circle; you may almost take off the jaw; the teeth are all loose and shaking; you may draw them all out with your fingers; perhaps they may be out already. There may be fungus; you take off the fungus, put this little trephine on the alveolary process, and a few turns will cause it to go into the antrum; then put up your finger and scoop out the fungus. There is a thing called a lancet, or bistourie caché; it was first used, I believe, as they say, for puncturing abscesses in the tonsils. It serves very well for puncturing abscesses in the throat, or in any other cavity, but more especially in the throat, for the method in which it is concealed tends to keep down the tongue whilst you are introducing the instrument, and then a lancet springs out to make the incision. There is a screw which regulates the distance to which you may plunge the lancet, and you may make a cut with it of any size. This seems to be a good instrument.

But now with respect to this operation of puncturing abscesses in the throat in cases of quinsey:—O, I do not know

what sort of cases they had in former days, but there are no such things now as are described. I have some reason to speak with regard to this disease, because I was the subject of it myself. If ever I caught a cold when I was young, which did not often occur, I got a violent sore throat, and it suppurated, as inflammation will do. It prevented me from speaking, either to be understood, or swallowing any thing without the greatest difficulty. I am sure it was bad enough. Matter formed, the abscess broke, and then I got well. As to its being suppuration in the tonsils, that it is not; and where the suppuration forms, I do not know, but I suppose it may be in the duplicature of the soft palate; and where it breaks I cannot tell you; but a case of this kind may be very annoying to the patient. And to give you a degree of knowledge upon this, I may say that I have been a very considerable time in drinking a very small quantity of warm milk, which I thought was the only nourishment I could get down; I thought it was necessary to get down something, therefore I tried the milk. Now I held the basin to my mouth, and by the action of my tongue I forced the milk over the dorsum of my tongue, and it returned through my nose; so that it made a circuit from the basin over the base of my tongue, through my nose, and into the basin again; and having performed this circle for some time, it gradually diminished. Lord bless me! if I were to find inflammation in my throat again, I should think I had renewed my youth. However, it was violent inflammation, it suppurated and broke, and there was an end to it. It was so violent, that sometimes I was confined for six or seven days together; and vexed at this, I several times shoved in a lancet, but I never let out any matter. Many a time I have been called upon by patients, who have said to me, "O do puncture it;" and I have said to them, "If you will tell me where to put the lancet in, I will do it, but I really cannot tell you." The whole soft palate seems diseased, but it is phlegmonous inflammation, and matter being formed, it is brought to the surface, where it bursts, and there is an end to it. So in my case, at last, some night or another the thing broke, and the next morning I was as well as ever. I remember having had one of those sore throats, just at the time I was to give some anatomical lectures at Surgeons' Hall in the Old Bailey, and if it had continued, I could not have given the lectures; however, I did not say anything about it; I was waiting, thinking it

would break; it did break the night before, and I went and gave them without any body having known that I had been for a week laid up in that manner. Well, there are people who excise and tie enlarged tonsils; however, I think large tonsils ought to be treated as any other enlargement of the body, and I believe by such treatment they do well, and do not require excision.

Hare Lip.—Now about hare lip; this is malformation. A child is born with the lip formed in two pieces, and very often there is a corresponding vacuity in the bony palate—a fissure all the way along. The operation for the hare lip consists in paring off the thin edges of the hare lip on each side of the fissure, making clean wounds on the edges, in bringing them together, and in uniting them. It consists in making two cuts in the form of the letter V inverted, then approximating them together, and then the lip becomes united by a middle line. Now the best way of cutting the lip is by what they call the hare lip forceps. You take the lip with a pair of forceps, and you pare off the edge. One plan is to place a bit of wood upon it, to cut along that plane, and thus may you make a perfectly straight line; it is a very good way to cut off the parts. I have seen pendant growths from the scrotum, nay bulky growths cut off in this way. I have put a wire on each side, fixed them together so as to secure all behind, the testicle and so on, and slipped it off with one cut; that is the way it is done, and it is a good way to make a straight line. Then the edges are to be pinned together.

Now the operations are to be shown to you, and I want no more at this time than to tell you what I think with regard to operations. This is what I think with respect to the hare lip, that the operation is not an act of necessity; that it is done to remove a deformity; that it should be performed when it can be accomplished most effectually; and when is that? O, there is no doubt that when persons have arrived at the years of discretion and reason, they will submit to all the control that is necessary. But we do it upon young children, just when they are born, and wherefore? Not on account of the child, but on account of the mother—the mother is horrified. Well then, as they are sleeping so much of their time, and so on, as they are scarcely said to be alive yet, O, you may pare off the edges and join them together; but if you do not make a very good job of it, it cannot be helped; there is no blame

to be attached to you. But I would advise you never to do it when a child has got to a certain age as to have a will of its own, for it will then scream, and cry, and tear open the wound as fast as you close it; I have resolutely set my face against performing it at that period of life. You see it is malformation; the alveolary process may take a wrong direction, and if it does, you have to patch and pull at the lip, or the lip would sunder again. I say I have resolutely set my face against doing it when they have been between the second and third years, or upwards, until they have a motive for submission; and that is, when the child gets fidgetty or uneasy, or when the young person, I may say, gets into that state because it is deformed. When vanity, that commanding principle, will induce them to submit to pain and keep themselves quiet, in order that they may be made to look pretty; then is the time. Well, now, under these circumstances I have done it in very bad cases of hare lip, I can assure you, and made the lip to look as if there never had been a division in it.

Cancer.—In taking away cancer in the under lip, you are obliged to take away a great portion of the lip, but you can bring the sides together very neatly, and there will scarcely be any appearance of your having performed the operation. You pin them together, and I know no better mode of proceeding than uniting them with the hare lip pins and the twisted suture; but I place my great reliance in DENTE'S uniting bandage. You can bring forward the parts in such a way as to cut off the two edges, and then close the wound.

Double Hare Lip.—There are cases of double hare lip: and in cases of a double hare lip; I need scarcely tell you, that one side must be cut and closed first and the other afterwards; you cannot do both at once.

Now I want just to tell you that the loss of the under lip is a most grievous calamity; say that you could bring the sides into contact, what sort of a mouth would you make, there being no under lip? The whole mouth would be made a terrific sort of aperture by the corrugators of the upper lip. The loss of the under lip is a horrible calamity. I saw a gentleman who had his under lip shot away, and he was obliged always to hold his handkerchief to his mouth, which was continually wet with slaver. There is no articulation with the loss of the under lip, except in an exceedingly imperfect manner indeed. Now, under these circumstances, I hold the performance

of the Taliacotian experiment of making the under lip, to be a very great acquirement, with respect to our ability to mitigate human suffering. This was one of TALIACOTIUS's experiments; but people hardly ever thought of TALIACOTIUS, and scarcely believed what he did. Then of late, seeing the powers of nature in uniting parts, hearing what had been said of transposition, and hearing that of late, people had been making artificial noses and artificial lips—artificial I cannot call them, but substitutes—making flaps, and bringing the stump of the nose in contact with the wound; but as to this I despise it altogether. TALIACOTIUS may say what he pleases about his noses being better, and smelling better than the natural ones; I never saw any artificial nose that I should like to wear on my face. A piece of pasteboard is far better than a piece of skin dangling to the stump; it is such a contemptible, despicable thing, I would have none of it. I have seen an artificial nose that was made of India rubber, and it seemed a good sort of nose, not to the eye, but to the feeling; but this I would not condescend to make. As to the under lip, Mr. LAYTON, for a man in the Westminster Hospital, after he had pared off the lip, made a fresh bleeding part, and cut a large triangular flap of skin from under the jaw, turned it round and united it. I saw the man afterwards, and it was a very decent and good under lip; it kept his saliva in his mouth, and enabled him to articulate. Now, I say, that is a great step in exhibiting the powers of surgery—powers which it had obtained long ago, but then those things were discredited formerly.

Mortification.—The under lip may mortify in consequence of scurvy of the gum, if I may call it so. Mr. POTT used to tell us of those cases very particularly, though they were little known formerly; but many publications have now been produced, with relation to the disease I am going to describe to you—the mortification of the gums, or lips, or cheeks. The fact is, that a man, woman, or child, may be attacked with this disease; but it generally occurs in children, I think, and usually the child is out of health. It looks pale and yellow. The gum is swollen; in this state mortification will suddenly seize the gum, and the gum will come away, or the under lip may mortify and come away in the same manner. Now, if you are only forewarned of what termination there may be to such a disease, it will excite you as soon as possible to correct

that state of health, and that spongy state of the gum that may terminate in so dreadful a manner. And it is easily done; it is only to attend to the diet, the alimentary organs, and to give them nitric acid, or some acid. I have seen many children where the disease must have gone on and destroyed the parts, but where it was prevented by attention. Well, now, so much for disease occurring about the mouth.

Salivary Fistula.—Salivary fistula is spoken of, where the parts belonging to the ducts and glands are spoken of.

THE THROAT

Then diseases about the throat.—What are you to do in diseases about the throat? O, you know as to tying the carotid artery, that that is an operation that may be shown to you, and I have spoken of the division of the *sterno mastoid* muscles. But here I have to speak of passing probangs or instruments down the œsophagus, and whenever we set about this, thinking of the course of the œsophagus, we never succeed; because, by introducing the instrument according to the course of the œsophagus, the instrument goes down over the dorsum of the tongue and brings on such violent coughing that we never can succeed. The true way is never to think of the course at all but to push your instrument right straight forward, and when it gets to the end it turns downwards, and describes a large arch over the dorsum of the tongue, never touching the dorsum of the tongue at all—and that is all I have to say about it.

When people cut their throats, what are you to do? Why, they may cut open the pharynx, and when they swallow, the liquor may run out through the wound, and keep it from healing. Now I should, in every case of cut throat, be inclined to put a tube down the œsophagus, by which I could inject food into the stomach. This I take to be a very great improvement in modern surgery. It was first done by DESAULT, in the case of a man who was shot with a pistol, by which the roof of his mouth was broken in, and he was left in a state unable to swallow. DESAULT put a varnished catheter through his right nostril, and it went down his gullet. With such an instrument as this, you may squirt food into the stomach, or medicine either. Mr. HUNTER put an eel skin down the throat of a man, but I think a varnished catheter is the best, for it lies as snug as possible. You see, if people swallow

at all, they never can swallow without tugging up and down the larynx. So that, I say, a cut throat is likely to do best where you put in a tube of the sort I have described. Now, to impress this on your minds, I tell the following case: There was a man brought to this hospital who had cut his throat, and so determinedly had he done it, that he had separated the œsophagus from the larynx and *thyroid cartilage*. He had lost as much blood as was adequate to kill him. Of course he had divided all the front branches of the carotid artery; however, the vessels had been secured. Well, I put a varnished catheter through his nostril, which I saw in his throat. I put it down behind the broad part of the cricoid cartilage. There were two portions of the cricoid cartilage hanging loose, which I put two threads into, and attached the ends to their proper places. I closed the wound, brought up his head, and made all fast with a bandage below his arms; for people, in that situation, are mad, and if you do not take care of them, they may fulfil that which they had only imperfectly accomplished. Having done this, I said to him, you are on no account to stir this, or to move it in any way. I am going to give you some warm milk to support you, for you are exhausted, and if it makes you sick, raise your hand. I threw in half a pint of milk, he making no prohibitory sign. Well, I said, if you wish for more, raise your hand; and he raised it, and so I threw in another half pint. The man's pulse was scarcely to be felt; and all I have further to tell you is, that, for the time he lived, he was continually making signs for nourishment, which was given to him in moderate quantities.

Tracheotomy.—Now, tracheotomy is an operation done here; and this is done for two reasons, either for the purpose of inflating the lungs when respiration has been suspended by a person falling into water, by being suffocated with carbonic gas from standing over a vat, from a person having strung himself up with a halter or any thing of that kind, or you may perform it in a case of diseased state of the larynx; in the case of chronic angina trachealis, which produces a sort of croup, where the respiration is dreadful to behold, threatening to suffocate the patient every moment. To prevent such suffocation, you make an aperture low down in the trachea through which to breathe. Some say, O, cut open the thyroid gland at the division of its lobes. Now, in the case of diseased

larynx, I am convinced that that is not the best place, for there you are making an incision and introducing instruments near to the seat of disease. O, I am sure it is best, at least I think it best, to make the incision below the thyroid gland. And if you want to blow up a man's lungs, that is as good a place for the purpose as you can find. You can introduce a tube there, or the nosel of a pair of bellows, which will just answer the purpose. I tell you a case, for that is the way I can bring all that is in my mind soonest home to yours, and I am sure that good is done by it. There was a person in this hospital, who had diseased glands on the sides of the neck, and by pressing on the pharynx he died suffocated. They ran to the surgeons who were going round, to say that the man was suffocated, and we ran to the ward where he was, but he was fairly dead. It seemed that he had died suffocated; his veins were turgid, and he looked like a person who had been suffocated. I said to one of the other surgeons, suppose we inflate the lungs, and he said if you like. Well, I made a cut in the front of his neck; and you know, when a man is suffocated you need not be very nice in dissecting. You want but a pen-knife and a pair of bellows. After you make the cut, separate the parts with your fingers till you see the windpipe, and I am persuaded that this is the best way. Make the cut through the hoops of the windpipe; it is a longitudinal pipe; make the cut big enough to admit of the introduction of the nosel of a pair of bellows; introduce the bellows, and then blow up the man's lungs. Now, I will answer for it, that all that was done in the case I allude to was in about six minutes from the time of the man's death. His lungs were inflated, we pressed the air out, we pushed the viscera of his abdomen against his diaphragm, pressed down his ribs, blew up his lungs again, but it did not seem to do him any good. Now, I had been just using the electrifying machine, and I said to a gentleman, run up and bring it down with all possible speed; he did so, and we sent some shocks through his heart, but still did no good. We covered him up warm—that is a main thing; you must never let the body cool. Put the warming pan on it; somebody must be close to it to keep it warm; you must not suffer the body to cool, for warmth is necessary to animal action. In vain you try to excite the living action if the parts are benumbed by cold. When I found that nothing more was to be done, I gave him what I may call a rattler, and it was curious

to see his arms spring up, but, as they say, the vital spark had fled.

Well now, you will say, that is a good one, to tell us a case where no good was done; that you will hear by and by. The good was done in dissection. But the Humane Society may say what they please, it is not likely that people who have been long under water will be recovered, for black blood has been circulated throughout the body, and the functions of the brain are paralysed. But I do say, that this man died under very different circumstances from the cases of those whose vital functions have been violently excited: he died from disease; the vital powers were tired and glad to be at rest. But the body was opened, and there was no blood whatever in the heart, yet there were the large veins leading to it, and the whole aortic system, gorged with black and apparently bruised blood; so that the blood had passed through the heart; the lungs had been inflated, and the blood had got into the aorta, but there was no action in the arterial system. Then I say, I believe that all this does good.

Well now, if I perform tracheotomy in the case of chronic angina trachealis, I think below the thyroid gland is the best part, and it is necessary to introduce a tube, otherwise the aperture would close. Now, this is no new doctrine. Dr. Monro was the inventor of this sort of tube to be put into the trachea, which allows a person to breathe through; but Dr. Monro found that it became clogged with mucus, so that he invented a sort of double canula, which allowed him to take out the one to clean it while the other remained in; then, when the one was cleaned, he put it in again. The instrument has shoulders made to it—shoulders to prevent its being swallowed, or pressing on the sides so as to hurt. Now, of late this sort of thing has been done in this hospital, and in one case Mr. Stanley performed it; but he thought it was a far better mode of cleansing the tube, to have it made wide and funnel-shaped. By being made large at one end and funnel-shaped, he could take a little lint and clean it out.

ON THE CHEST.

Then I go on to speak of operations performed about the chest; and here there is the amputation of diseased breasts, and the operation of paracentesis thoracis.

Now, as to the propriety of the operation for a diseased breast, it depends upon the nature of the disease. You have a tumour which you do not believe to be cancerous, then you treat it as you would treat a tumour any where else. You have a disease which you believe to be cancerous, which you are obliged to remove; and, in doing so, you take away a sweeping portion of the breast; cutting wide of the part diseased, taking away all the skin that is affected, and taking away not only that which is cancerous, but all that which is within an inch of it. The disposition of the disease extends further than the seat of the disease. Then, whatever skin you determine to leave, you reflect back to a certain distance, hollow out the subjacent parts, take away a considerable portion round the diseased part, and sweep away the whole from the pectoral muscle. You must never look at the bleeding wound to know whether you have taken away enough or not, but you make a division of the part removed, to see that there are nothing like fibrous bands shooting from that part into the remaining cellular substance. Well, you may thus take away all the disease, and all that has a disposition to disease, but the whole prosperity of your operation then depends upon preventing that state of constitution which has produced the first disease, and which is liable to produce it again. Now I have seen cases where people have had carcinomatous tumours fairly removed, who have lived for five or six years afterwards; and I have seen cases of cancer where, without performing the operation, by mitigating the circumstances that tended to produce it, the persons have lived for a considerable number of years. I have told some of these cases, and I need not say more on this subject.

Then, with regard to this making of an opening in the chest for the discharge of pus, or for the letting out of water, you would, of course, wish to make the opening at as dependent a part as you could; but then the question is, which is the dependent part? Why, you cannot cut through the large mass of muscles on the back; and if a person is raised up with pillows, you cannot cut in any place where you might chance to hurt the diaphragm. O, between the sixth and seventh, or seventh and eighth ribs, keeping at the lower edge of the upper rib, is the best part. The first thing you do is to draw up the skin considerably, make a cut through it, so as to expose the muscles; keep them in view, detach them, so as to bring to

your sight the intercostal muscles, and then divide them. The precaution required in this operation is to keep the air from rushing into the chest, by which the patient's lungs might be compressed, so that you have to make a valvular opening, and by which you let out the water or pus. I am sure it is an operation not hazardous, and may be productive of considerable good in many cases. You may even syringe out the chest, in cases of putrid blood being lodged in it; but I have told you of these cases in the course of the lectures, selecting those which I thought would make the strongest impressions on your minds, and lead you to remember the anatomy of the parts.

ON THE BELLY.

Now about the belly you have the operation of gastroraphy —the suture of the bowels. Then what I have to say on this is, that if a man is run through the bowels with a small sword, why you are not to lay open the belly to see what is wrong in it. But suppose it was cut open with a broad-sword, and the bowels came out; or suppose a person had anything run into his belly which occasioned the bowels to come out, what are you to do? You should be prepared for these accidents. If the bowels come out, and be not wounded, you should put them back again, and the whole case is to be treated on the principle of reducing a hernia. There was a boy running in the street who had a pointed stick in his hand; he tripped, and the stick went into his belly through the linea semilunaris. The boy became horribly sick, and with the effort of vomiting he forced out, I suppose, at least one half of the small intestines through the wound by the time they got him to the hospital; it was only a small wound, and what is to be done in such a case? The opening must be enlarged. Enlarged! to what degree? To the degree that will enable you to get in your finger. Then you look for that portion of the bowels which has come down first or last, and you replace it, which you must do with all possible gentleness. You never can put up the middle portion first; you must put up the first or the last first, and close the wound; then you must bleed to prevent inflammation. But in sabre cuts received in battle, the bowels have come out, but they have been cut, and the *mesentery* has been wounded. I need not tell you that nothing of

this kind should be returned till you have secured the vessels. You must then give the patient cordials, endeavour to raise the circulation, and until you had secured the vessels you could not return those vessels which would be liable to bleed. If the intestine was cut in two what would you do? Or, if a piece was mortified, what would you do? They say you may put it into the belly and let it adhere to the parietes or sides of the abdomen. Now, I think, putting sutures into the intestines is really a very effectual thing; it is described in old books of surgery, and it seems to me the moderns have perhaps lost a little sight of this. You never can introduce one part into another with propriety. You may make a hollow cylinder with soluble substance, with isinglass if you please, it is easily done; get a round ruler, and dip it in a strong solution of isinglass; the isinglass adheres to it; dip it in again, and when you have thus made a hollow tube of isinglass, and when it is cold, cut it in two places and withdraw the ruler. You then take a portion of the cylinder and work it into the upper part of the intestine. It might be difficult to know which was the upper part, but you must do as well as you can in endeavouring to discover it. Endeavour to introduce the under part thus into the upper part, and not the upper into the under. Well, then, having put them in so, you can with thread attach the two portions of intestines to each other. Then the question is, and how many threads should you use? The old surgeons were for using many, and the young surgeons few, because they said this is wounding the intestine further, and it must increase the hazard of inflammation. Of late, experiments have been made upon the subject by Mr. Travers, and he finds that it is better to sew the intestine with what one may call numerous threads, because, he says, if you put in too few, the contents of the intestines may work their way out into the abdomen, and peritoneal inflammation may be produced. Indeed this did happen where few stitches were put in, and in a case where more were put in it did not. Well, I am only telling you what the cases are, and I am telling you of cases where little hope can be entertained; but still there is a sort of duty to be performed, even though it is a hopeless one, and we must discharge it. As to making an artificial anus, and so on, I can only say that I should be very sorry to live upon such terms myself; but I know that this has been done, and done with success, in London. Where there has been a piece of mortified

intestine in the case of a hernia, the ends have been united in the manner I have described, and the patient has done well.

Well, I have done with that subject, and the next is with regard to

HERNIA.

Now you know what is called hernia is a protrusion of some parts of the abdomen out of the cavity. There is *umbilical hernia;* there is strangulated hernia, which does not often occur; there is a protrusion of the bowels through the *sacro ischiatic foramen;* you have the bowels descending by the side of the bladder and navel in the female subject. But it is not necessary for me to go into the particulars of these very rare cases; it is the common practice of surgery that I want more particularly to impress upon your minds. When hernia happens, it frequently goes up if a person lies in bed, and comes down if he goes about, more especially if he is engaged in straining at all. If hernia is returnable in this way, it does not become the object of surgical attention: you return it, put a truss on it, and keep it on. But if it is not returnable, or if it is difficult to return, and so strangulated is the intestine as not to perform its functions, then it is of great importance to return it by some means or other. I would keep a man in bed, starve him, and return it in that way, so that he might wear a truss. I have returned many that were exceedingly annoying in that way. Give him physic. Exciting the peristaltic action of the intestines has a great effect in getting the hernia drawn up. Now I am sure of that, for there are some herniæ where the neck of the hernial sac is so small, that it is difficult to return them; yet they are returned in consequence of people lying in bed and taking opening medicine. But if they are positively strangulated, what is to be done? Strangulated so that the alimentary part cannot pass back into the abdomen, what are you to do then? Why this is a very bad case indeed; and it is in this state that you are first called upon to attend a patient. Now I should not be very fond of handling the tumour; much pummelling and pressing of the intestines must do harm. There is something that experience teaches you, and nothing else, I believe, in the feel which enables a surgeon to say, this is a hernia returnable, or which I can return, or which I can not return.

If you make pressure upon an intestinal hernia, considerable pressure, such as is adequate to force the intestines upwards, and continue it for a time, not to hurt the parts, but such as is sufficient to occasion the return of the parts, and you do not hear a sort of bubble of air or water, you had better not touch that part further, but immediately set to work in order to do those things which are found to be the most effectual in returning the parts to their proper place. There is a gentleman in Dublin of the name of GAHON, who has laboured to show this, that there is a great deal of mischief done, and bad made worse, by the handling of hernia. He has said, that cold is a good thing; that the application of ice to the parts, to occasion a corrugation of the scrotum, or to gain a truce with the inflammation, seems to lessen the hernia. You put the patient into the warm bath, and would you bleed or not? Why that depends upon circumstances; if the patient was plethoric, you would bleed; but I beg you to consider the various results of strangulation, according to its degree. Sometimes there may be a stricture not vehemently compressing the parts, but teasingly compressing them, and an inflammatory constitution, where considerable inflammation will come on, and the parts will be extremely tender; bleed—bleed in that case, of course. But sometimes stricture may be to that degree, as not to induce much inflammation, but merely to prevent the return of venous blood from the parts, and then you will have a considerable effusion of fluid into the hernial sac; and this I have often known to be a deceptive circumstance; the sac being distended when you have felt the hernia, you have not thought it right to compress it, and people have said, this cannot be the cause of all the injury. A loop of the omentum will deceive you also. But the stricture may be so great as, begad, to produce a mortification in the part, and particularly in old people. I have known hernia, where the patient did not suffer much, where it was not long down, and where the operation being performed, the gut has been mortified. There can be no treatment laid down as to the management of hernia, that is appropriate to all the different kinds of cases. As to bleeding, if there was inflammation, why you would bleed if there was arterious excitement; if there was general arterious excitement, you would be more particularly called upon to bleed, but you cannot bleed in every case. Really there can be no one settled plan, nor any rule for the practice of surgery laid down, so as to do away with

the necessity of every individual practitioner exercising his own judgment; and the more intelligent the man is, the more likely will he be to do right.

Well, then, supposing the case to be a hernia, I say, speaking generally, you think it will be difficult to manage, and what are you to do? As to the bleeding, you must act according to circumstances; but I believe that cold is the best thing; and the cold that produces corrugation of the scrotum, makes a depression, and diminishes the bulk of the parts, is a very considerable object: in short I divide the indications, as to what is to be done in hernia, in this manner: the object is to diminish as much as possible the bulk of the protruded parts, and to gain a truce with the inflammation which would augment the bulk, to leave the aperture, out of which the parts came, in as patent a state as possible—that is, putting the parts into such a situation, as to leave the aponeurosis loose; and the third is to apply some power capable of shoving the protruding parts in through the patent aperture—the diminished parts back through the patent aperture. That may be done either with your own hand, or by exciting the peristaltic action of the intestines; but if you excite the peristaltic action of the intestines from above, some say you must not give purgatives. Well, I own to you, I do not see any reason why a small dose of opening medicine should not be given, but to give irritating purgatives would be absurd.

They say, if you give purgatives at all, you produce a motion and feeling throughout all the bowels; well, I do not see what harm that would do; but certainly the general opinion that purgatives should not be given must be true, as they lead to a stimulating action. You may, however, excite peristaltic action from below by clysters, and that this is done to a very considerable degree, is true. The fumes may be puffed up with great effect. I have been in the habit of seeing that done, and with very considerable benefit. People have had compresses of powdered ice mixed with salt, laid neatly on the scrotum; they have had tobacco fumes puffed up through their guts to a certain extent; the surgeon has withdrawn, and those who were present; they have gone back in a short time again, and found the patient fast asleep; they have turned up the side of the clothes, and seen that the hernia has gone up. That this sort of proceeding does produce considerable peristaltic action you may be sure, by the rumbling it makes in a

man's guts which you hear. The tobacco, too, is to produce a relaxation—a thorough relaxation of the parts. The first case that Mr. Pott tells of this kind is strongly in proof of what a man is to do; there was a man with a large scrotal hernia, and the surgeons could not reduce it; they had agreed to perform the operation, but, egad, some tool was wanting when they were ready, which they sent for; the man was on the table; and they said, suppose we try, in the mean time, a tobacco clyster; they threw it up, there was a rumbling heard, the parts diminished, and the tumour entirely receded out of sight! But even the tobacco clysters are gone out of fashion now, and what is to be done? I grant you it may be used, and may be used with great effect, and to a greater extent in some cases than in others. You may use it in the case of an old shoemaker, differently to the manner in which you would use it in the case of a delicate lady; but it does produce very long and peristaltic action, and I should be very sorry to see it used in all cases of hernia.

The warm bath, of course, you must resort to. O, that very often enables you to return a hernia. But all these measures failing, what is to be done then? I should then say to my patient, for I would not delay; and here I speak what is the opinion of all the best surgeons in London, that you are not to delay. If you have tried the most efficacious means to-day steadily and perseveringly, and they have failed, what right have you to expect success by milder measures to-morrow? Why do people die after the operation is performed? Not from the operation itself, for that is a mere nothing, but because the intestines have been suffered to be imprisoned for so long a time that disease has been occasioned which is irremoveable. Then, after you have tried these measures steadily and perseveringly without effect, you must proceed to the operation. But to prepare my patient's mind for it, I should say, "Why, sir, I have tried all these things to get it to return, but I have not succeeded. I have not tried my hand yet, I have not tried what they technically call the taxis; do you know that I have a great inclination to try that, and a strong conviction that I shall succeed in it. I have tried it many times where persons have been condemned to the operation, and I will try it now, if you please; but I will try it only on one condition, which is, that if I do not succeed you will give me leave just to divide the skin." Well, that prepares a man's mind; you

get him on the table; you get assistants who hold up his pelvis, and then you set to work. In femoral hernia, and indeed in all these cases, you do your utmost to return the hernia without the operation, but satisfied you cannot return it, why you proceed to the operation, and that operation will be shown to you.

Now it still remains to me to speak of the after treatment of hernia, and that I shall do.

TREATMENT OF HERNIA.

There is one thing with regard to hernia which I did not say. We are often really puzzled to know whether the disability to procure *stools* depends upon the strangulation of hernia or not, and I think it would be very well if every one would consider, whether there was not a state of symptoms appropriate to *impermeability* of the bowels: I am convinced that there is; it is an *increasing fulness* of the bowels above the impermeable part—a gradually increasing fulness. There is not a tenderness in this case that belongs to *peritonitis* or *enteritis*; there are actions, and rumblings and workings about in the alimentary canal which you do not find in the *ileus*.

Now here again I shall take the short way of telling you what is in my mind, by the recital of a case:—I was asked to see a woman who had lived for five or six days ill in the country, and without having any passage from her bowels. She had two herniæ, one on each groin—that was curious enough, but they were very indolent; still by pains-taking I got one of those herniæ reduced, so that that narrowed the case, but I could not reduce the other. There were all those characteristics present which I am speaking of; and I said to the surgeon who had been attending her, she will die, but we ought to see whether anything can be done for her; there may be a knuckle of intestine entangled in this hernia, which we ought to ascertain. Accordingly I divided the skin, and when I came to examine what it was, I found it was nothing more than *omentum*. I did the woman no harm; I put it back, closed the wound, and went away, but I went away exceedingly mortified with myself. She died, and it was afterwards found that she had a *contraction* of the *sigmoid cavity* of the *colon*; there was an obstruction there, it was not in the groin.—Well, there will be perplexing cases, and I go on now to speak of the

After Treatment of Hernia.—I say that *operation* is not the main part of our profession; it is to know *when* an operation is necessary, it is in knowing that, that surgical ability consists, and in knowing *how* to treat a patient afterwards so as to insure the patient's well doing in future, so as to render the operation effective of what was meant.

While a person has a hernia there is a something that seems to put a stop to inflammatory action; there is a faintness, and languor, and sickness present, so that we have not inflammation so predominant in the alimentary organs generally as you have afterwards. To be sure there will be more or less inflammation in the parts, dependent on circumstances. But after the operation, when you return the parts, peritonitis will come on, and enteritis will come on; and more especially if the parts have been long protruded, and the coats of the parts have got thickened, you will have enteritis, and the inflammatory action will be proportionately vehement, greater than would be during the existence of hernia; and the after treatment of a case of hernia is the treatment of enteritis.

Now I want to show the grounds of my opinion upon this to you, and for that purpose I select one case in proof of it. The case was that of a man who submitted to the operation for hernia in this hospital; there was a clammy coldness all over his skin; he seemed to be like a dying man. I remember there was a very large quantity of bowels down; I should think fourteen or fifteen inches of bowels: they seemed tumid and thickened, and did not look remarkably bad; it was a case in which the patient might be said to be recoverable. The bowels were put back into the abdomen, but still he remained in the same state, just like a dying man. Clysters were thrown up; such medicines as would remain in his stomach were given; small doses of opium, and so on, were given by the mouth, but no change took place. It was two days after the operation before his bowels were brought to secrete, then he had free discharges from his bowels; but during the whole of the time he was ill, until this period, there was that sort of languor and depression of the nervous power which seemed to describe him as a dying man. As soon as the bowels came to discharge, the most violent inflammatory action took place; his bowels became hot on the surface, and the abdomen was very tender indeed; his pulse became frequent, but not bold and strong. Now I do solemnly believe, that that man's life was saved by the in-

telligence of the house surgeon at the time. They had been obliged to give him cordials to support him, and medicines to solicit secretions from the bowels; clysters were thrown up, but he was sinking from languor till his bowels began to secrete, and then the very reverse occurred. The house surgeon bled him largely, fomented his belly, applied leeches, put on blisters, and so on. Now the discharge of the bowels no one would stop, for nothing eases the secreting bowels so much as discharge; secretion and discharges disburthen the vessels.

Well, I say, the after treatment of hernia is the treatment of enteritis. You must give mild and gentle medicines; you are to endeavour to get the bowels to secrete, and clysters may be used as co-operatives. When discharges come on they are to be suffered to continue and not to be suppressed, though they were capacious, for those discharges from the bowels ever indicate that the organs are in a state of irritation, and the discharges relieve that irritation. If you suddenly stop purging in any case you will have inflammation in the bowels.

Well, the common treatment of such cases is nothing more. You will see cases where you will very often be afraid to bleed, for the pulse has by no means the fulness and boldness that it has in inflammation of parts of less consequence to life. It is a small and frequent pulse; but you bleed, and the pulse rises from the bleeding. The blood being buffy and concave on the surface, this is your warrant for going on with the bleeding; for here it is as it is in every part of importance to life, where there is inflammation you must bleed to reduce the inflammation, or the inflammation will produce fatality.

Tapping.—Now, about tapping the abdomen: I have little to say on this subject. You are to plunge a *trocar* in, as has been mentioned, as low down as you conveniently can, for the decanting of the liquor in the *linea alba*; but it is generally, we may say, about half way between the navel and the os pubis where you tap. You are obliged, in *ovarian dropsy*, sometimes to puncture at the *sides*.

Well, as to the instruments: some like *flat* trocars, for they make a wound unquestionably more like an incision than the others, and some have *rounded* trocars. O, the incisions made by rounded trocars discharge thick matter, or any thing of that kind, best. I am one of those who like the old-fashioned tools. If I see them always answer, I should never try new

ones; and I always puncture as you see practised in this hospital.

As to the mode of introducing the instrument, there are some who make a wound first, and then put a cold piece of iron down into that wound. In this way they may perform the operation with less force; but, I say, those who have submitted to have the operation performed both ways, know which is the best, and I know they have given the preference to the vulgar mode of tapping, What do you tap for? You would not do it unless there was a sufficient quantity of water to be discharged. You make a plunge with the instrument, in order to pass it through the integuments, and the linea alba, but you never drive it up to the hilt. Plunging it in this way, you occasion but a moment's pain. Pain does not depend upon the degree of injury done, it depends more upon the duration of the time in which you are inflicting that injury; and if you do it properly, it is done in a moment. You regulate the depth you mean to thrust the instrument through, with your thumb. Well, then, supposing you have got it through, I should withdraw the *stilet* from within the *canula*, and then the water gushes out. Now this mode of performing the operation is particularly requisite in *hydrocele*, where you mean to follow it up by injection, for many operations have been performed, in cases of hydrocele, where the surgeon has done it, as if he meant only to perform the palliative cure. He has withdrawn the canula, injected the fluid into the cellular substance, and produced sloughing. Now, when you puncture for hydrocele, you must find out where you can do it without hurting the testicle; having done that and punctured, then withdraw the stilet, within the canula, urge on the canula into the *tunica vaginalis*, and then you may inject without fear of the canula slipping out.

Lithotomy.—Well, now I want, in the next place, to speak of lithotomy—what I have to say respecting it *preparatory* to the operation, and *afterwards*.

You know all these operations will be shown to you, and Mr. STANLEY will make his own remarks upon them. I am only solicitous that I should have he opportunity of telling you what I think of them; I was prevented from doing so in the first course, and I do it now. I do not wish to detain you longer in these lectures than is absolutely necessary, and therefore I have been talking with Mr. STANLEY on the sub-

ject, telling him what I meant to do, and I find it would be useless for me to go through the whole of the observations necessary to be made in all the operations, which, if I were exhibiting, I should deem it my duty to do. What would be the good of my telling you about the preparing of the operation of *castration*, because all this has been said in the proper part of the lectures? What should be done in performing operations, and what should induce you to perform them are the material points. If a person was to give a course of lectures upon operations, and say every thing that was necessary to be said, why it would just be a repetition of the whole course you have gone through.

Well, I say, I am now anxious to speak about lithotomy—the taking of the stone out of the bladder. Now I am convinced that stone, and that all the diseases of the *urinary organs*, are produced by a primary disorder of the digestive organs; and that where people are liable to stone, they have strong *acidity* of stomach, from not digesting their food. You know that in former times, stones were thought to be of an earthy substance, but it was SCHEELE who showed they were principally composed of *animal matter* rendered *concrete* by acids. That *calculi*, in general, are composed of animal matter held in union by acids any one may satisfy himself, by pounding a bit of a calculus, and dissolving it in water; you will then find that it is strongly acid. The animal substance will go into a state of putrefaction. There is very little lime in stones in general. If you throw a piece of calculus upon a hot iron, it burns and smells offensively, and altogether evaporates; there may be a little particle of lime left, something that is not dissoluble by heat. There may be, but I say, in general, calculi are produced in this manner. At the same time there are other causes which produce them; and if medical men give *alkalies* to a considerable amount, they will precipitate the calcareous phosphate from the urine. Many people have had a stone which was of the nature I described—a stone containing very little lime, but made concrete by acids, and they have been put under a course of alkalies; afterwards they have submitted to the operation, and what sort of stone did the surgeon take out? A stone with a very thick coat of phosphorus (ammoniaco-magnesian phosphate). Now there is a paper of Dr. WOLLASTON on the subject in the Philosophical Transactions, and a Dr.——(MARCET)—I am very forgetful of names, has

published on the same subject; a gentleman who died about a year or two ago; a very deserving man in the medical profession:—it is very strange that I should be so forgetful of names, but, however, there is something defective in the *organization* of my head, that I do forget names.

Well, all I have to say is, that attention to the digestive organs is of great value; and that I believe, when you ascertain there is a stone, or from the quality of the urine that there is acidity, it would be well to give small doses of alkali, but not to great excess. But the question is, how do you know there is a stone in the bladder? You say by the symptoms. But I say that symptoms are excessively delusive. The symptoms of stone are the symptoms of irritation, produced by any other cause. The symptoms which seem most characteristic of stone are an increase of pain after the discharge of urine; making water without much pain, but the pain augmenting as the water is discharged from the bladder. That is, when the coats of the bladder press upon the stone, the pain is augmented; and there is generally a reference of the pain to the front of the penis. It is usual for children in this condition to pull their prepuce; so that a longer one than usual has been considered as a sign of a stone in a child. I know not any stronger symptoms than these, but you must determine for yourselves.

Now this is an operation which I have to tell you ought to be very carefully conducted or you may miss the stone. I have to tell you that there are many surgeons, of considerable ability, who have searched for the stone and have not been able to find it, though a stone has actually been in the bladder, as after-examination has proved. I remember one man, who had been in the habit of drawing off his own urine for many years. He had disordered urinary organs, but he never had any of those pains which were characteristic of stone; and for a very good reason, the bladder had lost its power. He had stone, accompanied with a paralysed, or very enfeebled action of the coats of the bladder; therefore he had none of the signs of stone that would attract the notice of medical men. He had been in the habit of drawing off his urine for himself. He had consulted all the people in London, and myself among the rest. I took it to be a case of irritable bladder, and told him he was to soothe it by tepid bathing, attending to his bowels, and so on. He went into the country and died there. He left it as a request in his will, that I should examine his body, and

I did so, and found in that man's bladder no less than seven stones ! !

Now that man was in the habit of introducing the instrument for himself for two years and a half, and yet he never felt one stone ! Well, this proves it must be done attentively; the probability is, if the stone is lying at the bottom of the bladder, and you press the instrument down, you may pass the instrument over it; sometimes the stones are higher up. Well, as I said before, it must be done carefully and attentively.

After Treatment.—It is the after treatment of the case that seems to me to be the most important part of it, and that varies very considerably as to what you are required to do. The after symptoms of lithotomy are excessively curious. The patients, prior to the operation, have been labouring under a disordered state of the digestive organs. They are in a state of health, from that cause, that even any common operation would be likely to render them very ill. I should never like to perform operations myself where there was a considerable disorder of the digestive organs, even of a common kind. Then after the operation of lithotomy, sometimes you have the stomach so irritable that it will not retain any food. You will observe the patients turning their tongues about in their mouths, having intolerable thirst, and sickness in the stomach. Well, then, the after treatment goes on to try to put the digestive organs to rights, to induce regular discharges, and so on. Sometimes there is a sinking of the powers after the operation, the result of the want of stimulus. People sink after the operation of lithotomy in a very strange manner indeed: they are worn out and exhausted with the disease; they are cut for the stone, and all the stimulus within them exhausted; they sink in a very curious way indeed. Now here I want to tell you a case which I have seen; and these are cases people do not meet with without having had a great deal of experience. Ask a surgeon of experience how patients do, and he will tell you that he has lost patients in a way he did not understand at all. If you think the treatment is in *bleeding*, &c. in all cases, you will be very much mistaken; the treatment is different in different cases. I remember a man, (and this is the case I alluded to) who asked me about something that was wrong in his *water works*, and I am sure I did not know what was the matter. I passed the *bougie*, but still he said he was very fidgetty. I said, you may have a stone in

your bladder. O, says he, I have been examined by so and so, (naming surgeons of the highest reputation) who say I have no stone. Well, if you have no stone, said I, upon my life I cannot suggest any thing better for you than what you have been doing, nor say what is the matter. He then went away. Afterwards he sent for me, and I had scarcely entered his chamber, when I observed a chamber pot with mucus in it, and *little specks of blood*. Lord, sir, said I, you have a stone in your bladder. And this, let me tell you, that I have often been assured of patients having a stone, from observing small specks of blood mixed up with the mucus they have passed, and that has been where the stone has injured the vessels. Well, I introduced an instrument into this man's bladder, and I found a large stone there. But he was most horribly disordered in his bowels, and I said to him, we must have this out, but you must have your bowels put into better train first. Some week or ten days were employed in doing that, and they were better. Then he became rambling in his mind, and was delirious. In the interval, however, I had requested him to consult Mr. CLINE ; he did so, and Mr. CLINE said, the operation must be performed ; the disease was killing the man, and the operation was the only chance he had. The patient sent for me the morning before the day when it was agreed this operation should be performed ; and when I went to him he could not tell what it was he had to say to me, nor was it necessary, for I knew full well that it was that he did not think he was in a state able to submit to the operation. I said to him, "Be assured, sir, no operation shall be done, if, when Mr. CLINE and I come here together, we would not have the operation done upon ourselves, if we were in the same situation." Well, the stone was taken out of the bladder ; it was easily done ; we were not three minutes about it ; and to give you an idea of the state of this man's mind, I may tell you that he was hallooing all the time, "What are you at, my boys? Pull away, pull away, my hearties; go on, pull away." Well, he was put to bed, but it was very difficult to keep life in him. The whole after treatment of that operation consisted in supplying the man with *brandy* and *soups*, and so on. It was the *third* morning after the operation when I first found his pulse beat steadily ; and on first speaking to him, he answered me rationally. I said to him, "I am very glad to find you better ; very glad to find you better. Well, but have you

felt any pain or any inconvenience ?" He smiled like an *idiot*, and said, "I lost that." I said, "About the parts where the operation was performed." "What operation ?" "Did you not know that there was a wound made between your thighs ?" He laughed. Well, they do sink in a curious way, and I say, if you are not aware of the different modes of treatment, you may really lose patients.

Where there is affection of the stomach and bowels, there will be no urine secreted. Certain it is, that many patients who undergo the operation have diseased kidneys. A long diseased bladder will lead to disease in the kidneys; there are preparations here to show that. There is a preparation here of the urinary organs of a boy, who died at the age of eight or nine years. I examined the body, and the kidneys were thoroughly diseased, with a stone in the bladder. The nature of his malady was not known. Well, I say, I can tell you that I have performed operations on people in the country, where I have been sent for to do them, professing that the patient would die; professing that there would be no secretion of urine after the operation; predicting the symptoms that would occur—a very pretty thing to do. I have said, "You have sent for me to do this operation, telling me it was a fair case for the operation; why, good God, this urine is such as denotes a thoroughly unhealthy state of the kidneys. But I am here; I know the patients cannot live without the operation being performed; it is the only chance they have, and very doubtful indeed it is. I know I must do it, but I wish you had not sent for me, for it is only hurrying the individuals out of their misery." All the old surgeons, the old hands that I remember, used to ask, "Does he *wet*, nurse? Does he wet after the operation ?" "Yes." Well, that was considered a very good thing, because it showed the state of the kidneys, and the state of the digestive organs to be such as to occasion the secretion of urine.

Never put on dressing after cutting for the stone. Lay a little *spermaceti* salve on a piece of lint, or something, and lay it upon the part. In former times they closed the wound, and the first thing the surgeon heard of after he had left the patient, was a messenger who had come, calling him for God's sake to go to the patient, who was in great distress. He went, took off the dressing, found a clot of blood in the bladder, sponged the parts, and it came away; but I say to you, do

not do this, and that the wetting by a patient after the operation is to be considered a very good thing.

Now suppose you go on so far; suppose there was urinary secretion, and that all appeared to be going on well, what is to be dreaded? Why, sometimes you have inflammation of the bladder, what I may call *cystitis*, requiring blood-letting and the soothing of the bladder; it may call for it soon after the operation. I have seen the formation of inflammation all round the bladder. But say there was none of this local inflammation round the bladder, and yet you are not sure, for the bladder may be inflamed to a certain degree, and the peritoneum may not, where it covers the bladder. This is a thing that does not come on for eight-and-forty hours; it is generally the second day after the operation, I think, that the peritonitis comes on, and then the patient complains of pain in the hypogastric region, and that is enough to frighten any body. Well, a surgeon ought to be prepared to meet it; and I say, if the patient gets on well till that period, and then dies from peritonitis, he is no surgeon at all who is attending him. I am sure he might subdue it if he knew his profession. If I were to see a person in this state, I should say, get the warm bath ready as fast as possible. I would take a good quantity of blood from the patient's arm; I would give him a large quantity of castor oil to cleanse his bowels, and when his bowels had been fairly released, I would foment his belly. When his bowels had been fairly released, I would put him into a warm bath, then have the bed warmed, dry him well, put him into bed quickly, and then I should expect that he would break out in a state of perspiration. I would give him small doses of antimony to keep up the perspiration—very small doses, not to nauseate the stomach, for I am sure that in all inflammation of internal parts, that of keeping the skin in a state of perspiration is most desirable; it is the keeping up of the circulation in the body; and I would throw up opiate clysters to keep all quiet about the bladder. Well, having done this, you may be under the necessity of bleeding again and putting blisters to the peritoneum.

But I say, I should think there was some mismanagement in losing a patient by peritonitis coming on after a time when all seemed to do well.

It is really a horrible operation, for you know not what is to happen. I have myself done some operations that were bad

enough, and the patients have really done better than I expected. I remember once taking out an adherent stone; I could not move it with ease; I was obliged to use a degree of force which I shall be very averse to do again. After using the force something gave way and the stone came out. There were two pieces of flesh embedded in either end of the stone. Well, this man was immediately affected with what I may call cystitis, and the inflammation of the bladder; it was subdued by bleeding, and we were obliged to set to work at once. Well, then, I say, it is an operation which one cannot undertake with *nonchalance*, because you do not know what is to happen.

I will tell you a case: I was sent for into the country to see a man who was more than seventy years of age, and who was worn down by sufferings. He had been searched by a surgeon of a provincial hospital, and a clever man he was; it was said he had no stone. He was attended by physicians; he was sinking and getting worse and worse. Then they said, you had better send for a London surgeon; he will examine you, and if there is a stone in your bladder let him do the operation. When I saw him, I said, if this gentleman has not a stone in his bladder I'll never ask for a symptom again. I introduced the instrument and struck the stone. There was some debate took place in the family as to whether the operation should be done or not, as he was so much reduced. It was then agreed it should be done. Well, I did it, and I think in two minutes the stone was extracted. I put my finger into the bladder, and I thought it was all clear. I told another surgeon to do so; he did so, and he said, it is. Well, said I, let the family know it is all over. I could not feel very far into the bladder, and therefore I thought I would put a scoop in, which I did, and struck another stone far up in the bladder. I put the scoop over it, but I could not bring it down; I then put the forceps in, touched it with them, opened them, took hold of it, and drew it out; it cracked, and there was a little of the shell broke off it; we threw a little warm water in to wash it. Begad, there was a third stone in that man's bladder, which we also took out! When we had done that, the man being still on the table, his eyes opened and his jaws dropped. He is dead, said one; this is no common faint, said another; and I said, what is to be done? I put his legs over my shoulders, and said to some of the others, take hold of his head and shoulders and let us put him into bed. We heaped blankets on him, got a warming pan and put over him

and got hartshorn and put to his nostrils. After some minutes there was a gurgling in his throat, and he breathed. Now he was in the strangest state from his stomach; certainly and indisputably from his stomach. If the medical attendant had gone down stairs a moment, he was immediately sent for into the room again. When he returned, the patient did not know what to say. On one occasion he said he was thirsty, would they allow him a little warm tea to drink? They said yes. No sooner had he put the tea into his stomach than he broke out into a perspiration, to such a degree that you might have wrung the clothes that were about him. Now, I say, it was *stomach* all this; it augmented his fidgettiness; he was more restless, and became extremely anxious and uneasy. We were all had up again to see him; and on applying to me, I said, I knew nothing about what he was to have. I said it was a pity they had given him tea; they might, if they liked, give him a glass of brandy. O, his stomach would not allow him to take that. Well, said I, let him alone altogether, and he will come round. In the after part of the day he had some irritation about his bladder; he tried to make water, but not a drop of urine came. Towards night the secretion of urine commenced—he *wetted.* I insisted he should take nothing but small sips of water, not much; but if he felt thirsty, or his mouth dry, that he should take small sips of boiled water; and thus was the night passed. The next morning he was evidently better, but still restless. Should he not take something? was the question. No—take nothing but the small sips of boiled water till he has a desire for food; and when he has a desire for food, then I have done with him. At three o'clock he expressed a desire to eat, and he had a teacupful of strong broth, with a biscuit. In two hours afterwards I said, "Good bye to you, I am sure you will do well if you only attend to what is said." He did do very well, and lived for many years afterwards. I felt anxious about him, and I wrote to the doctor who attended him throughout, to know how he was going on, and the answer I got was this:—"You need not give yourself any trouble about him; he went to bed last night at such a time, and rose this morning, eight hours afterwards, and voided almost a quart of urine, and in as strong a stream as ever he did in his life."

Now, having said thus much about stone, and the operation for it, it may be expected that I should go on in the same strain

about that and other operations, which I could do for hours, but that will be unnecessary, as the operations will be more particularly illustrated by Mr. STANLEY.

Now, you know, Gentlemen, that the bones and muscles have been gone through, that the physiology has been discussed, morbid anatomy, and so on; you know those things have been effected. If you should say aye, but in a very imperfect manner—O, I am very well contented you should say that. I say, O, nonsense, there is nothing perfect in human life, and you need not be looking for anything perfect in a course of anatomical lectures. I say that this is such a great course of lectures, that a person may be excused if he does not complete it in all respects. A little course of lectures, where you could at all times command your preparations and subjects you might give more complete than such a course as this. Little works admit of a high polish, which large ones do not. I say that that is an excuse for anatomical lectures in general, and that is all I wish. I am conscious that I have done my best, and sorry I am that that best has not been better. I have, however, felt so, and will continue to feel so, as long as I continue to give the lectures, which will be just as long as I feel competent to do it in an efficient manner; and I do not think that ever I shall want any Gil Blas to inform me when I am unable to give anatomical lectures in an efficient manner. There will be a number of young men in the room who will tell me that.

I say I mean to go on with the lectures as long as I am able to do it in an efficient manner; and you may depend upon it I will not continue longer, at least, on my own part, voluntarily. I am not one of those who seek to keep in office, and to do things for what they can get—that is very well known; nor do I care whether there is a numerous class or a small one. I should be as well contented, or better, to lecture to half a dozen attentive pupils, than to a thousand inattentive. I always feel great gratitude to the class for the attention with which they hear me; and it has been a great gratification to me throughout life, and I consider it to be a very great honour, that I have possessed the good opinion of students. I say an honour, because it shows they were conscious that I have endeavoured to do my duty in the situation of life in which I was placed. But students now-a-days may not be what students were formerly; formerly, they came up with a zealous

and honest desire to learn their profession. Egad, I hear some of them say now, I have attended enough to pass the College; the College does not require more than this! I say, and what is the object you have in view? Is it merely to pass the College, or to make yourselves intelligent members of your profession? I cannot help the changes of circumstances or of times, I only mean to say that I have possessed the good opinion of students, and I hope I always shall, because I know the bulk of the class must be good men; I may say, because they are young men, and if they know that a person honestly endeavours to discharge his duty they will think well of him.

I say I am very much obliged to you for the attention with which you have honoured my lectures; and that has been demonstrative of your desire to gain a knowledge of your profession—of a profession which I hold to be of the highest importance to the public—of a profession which I hope it will be your lot always to practise with reputation to yourselves and benefit to others.

And now it has been the custom for teachers of anatomy to profess their willingness to serve their pupils at any future period of their lives; and I make you that profession in conformity to custom, and as others do it; but I do not wish you to regard it as mere profession, for I say you may all be philosophers on this subject if you please, and put it to the test of experiment. I am not conscious of there having been any slackness on my part of having endeavoured to serve pupils when it lay in my power.

Well, I make you this profession.

And, now, Gentlemen, I have no more to say, unless I were to repeat, that for your kind attention I feel very much obliged, and to assure you that I wish you all, all manner of happiness and prosperity.

THE END.

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