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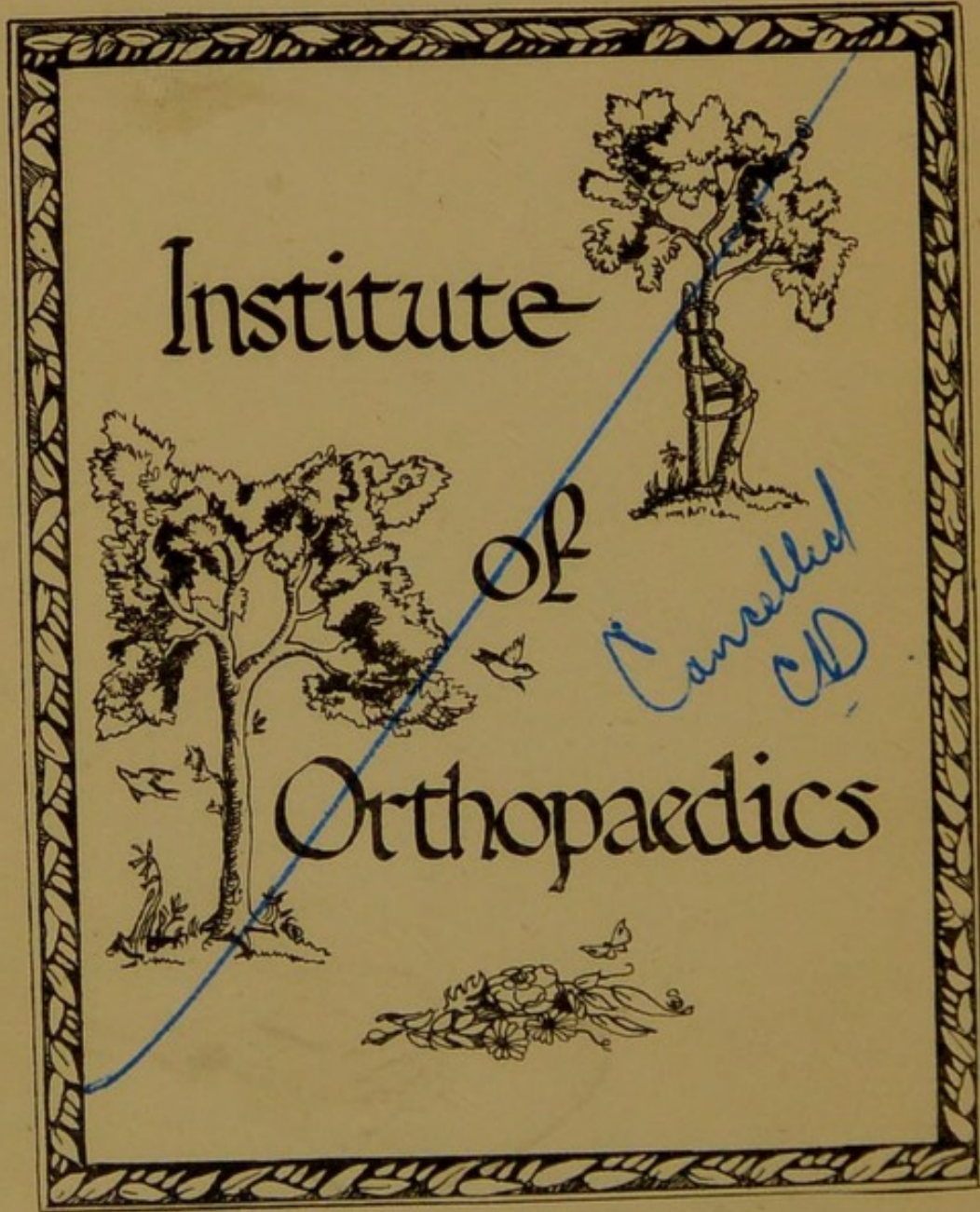
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CLINICAL SURGERY

C. B. LOCKWOOD

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CLINICAL SURGERY

BY

C. B. LOCKWOOD

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PREFACE TO THE SECOND EDITION

IN bringing out a second edition the opportunity has been taken to add a section dealing with the operative treatment of fractures of the patella; and also one dealing with the great amputations. At the same time the title has been somewhat shortened.

19, UPPER BERKELEY STREET.

PREFACE TO THE FIRST EDITION

THESE Clinical Lectures and Addresses have already appeared in the columns of the *British Medical Journal* and of the *Clinical Journal*; and I desire to thank the Editors for the leave which they have so kindly given me to print them again. In spite of the rather colloquial style of some of them, I venture to do so, because so many of my friends and pupils have expressed a wish to have them in a form more convenient for reference.

19, UPPER BERKELEY STREET

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I

AN INTRODUCTION TO THE STUDY OF CLINICAL SURGERY

It is part of my duty to lecture to you on clinical surgery, and also to teach it in the wards. I propose to try and impart to you some of the general rules by which I wish you to be guided.

In his delightful memoirs Marbot says that he has noticed that each profession has its point of honour. He instances the Alpine guide whose point of honour is never to desert the traveller entrusted to his charge. The sailor's point of honour is, first, the safety of the passengers, then that of the ship, and last of all his own. The point of honour of the medical man is the safety and welfare of the patient. Now the safety and welfare of the patient cannot be safeguarded without a correct diagnosis of the disease or injury. Clearly it behoves all of us to use our best endeavours to learn that indispensable art. To make a correct diagnosis the patient has to be examined. Now the examination of the patient is guided by a few

simple rules. First, and above all, by those of Sir George Humphry, 'Eyes first and much, hands next and little, tongue not at all.' No one could tell in a few words the meaning of these, but after a while, as their import becomes known, you will learn to use them without thinking and as a matter of habit.

Let us try to understand the meaning of these rules, and begin with 'Eyes first and much'. In by far the larger proportion of surgical cases the effects of the disease can be seen with the naked eye. This is most fortunate, for we can be sure of what is seen, but by no means sure of what is felt, heard, smelt, or tasted; seeing is believing. But to see anything correctly, even the simplest thing, much thought and training are needed. You come here to be taught to see and to think. At first you may be disheartened by the hardness of the task, but after a while it grows easier, and you will have learnt not only to see what others can see, but perchance to show others what they have never seen. You will have become, in fact, an original observer. Seeing is far from being a simple act. The mind itself has to think over and pass judgement upon that which is seen. A clear mind which can judge correctly is a very rare gift, but I believe that every one can be trained to think clearly if he will only begin by acknowledging his deficiencies and seek to supply them. To see anything rightly is one of the hardest of tasks, but perhaps the

hardest task of all is to give a clear and truthful account of that which has been seen. But after all, as the Stoic says, 'No great thing cometh suddenly into being, for not even a bunch of grapes can, or a fig.'

Logic, the science of reasoning, is greatly neglected, and I strongly advise you to devote a little time each day to reading an elementary book on logic, such as that of Jevons. The absence of a logical training is, in my opinion, one of the greatest defects in preliminary medical education, and in English education in general.

Every day one has to wonder at the credulity in which our minds are steeped. We enter the temple of science through the portals of doubt. The everlastingly repeated story of an injury suffices to put every one on a false scent. Here is a man who says that he fell and put out a joint of his finger. A dislocation is diagnosed. Why? Because the joint is deformed, and the man says that he fell and put it out three months ago. The electrical department works at the joint with the screen, and reports that a dislocation can be seen. Doubtless the observer saw it. He had heard the history. The mind will make the eye see many things when under the influence of an obsession.

What ardently I wished, I long believed,
And, disappointed still, was still deceived.

You all accepted the report, and were prepared

to try and reduce the dislocation. But, as I was unable to see the dislocation with the screen, a photograph was taken, and showed a very slight displacement of the articular ends of the phalanges. Such an amount of displacement is not unusual in chronic arthritis. Further inquiry threw much doubt upon the history. He might perhaps have fallen down, but clearly the arthritis began some months before the supposed injury.

Do not accept the statements of others ; see for yourself with your own eyes. You may remember another occasion on which a girl said she had swallowed her false teeth, and that she felt them in her throat. Quite a number of credulous people used the screen and X-rays, and saw the false teeth in the upper part of the œsophagus. I passed a full-sized bougie into the stomach, and next day the teeth were discovered amongst the bedclothes.

Touch the patient with infinite care and tenderness lest bodily pain be inflicted. The human being fears death above all things ; after death he fears pain, and after pain, parting with his possessions. It has been written that ' a man will give all he hath that he may live '. The fear of death and of pain is deeply implanted in the human breast. The infliction of pain is nearly always the result of ignorance, carelessness, or clumsiness. No one who causes needless pain can ever hope to win the confidence or gratitude of his victims. Learn to use your eyes, and if the eyes do not suffice, learn

to use your hands gently, skilfully, and with a definite intent. There are right and proper ways to feel for fluctuation the kidney, the spleen, the edge of liver, enlarged lymphatic glands, and many things besides. The art is learnt by watching the methods of experienced surgeons and by constant practice. A cultivated sense of touch is a valuable possession; what Luther Holden called 'surgical fingers' is a rare gift. Hence Sir George Humphry's rules, 'Eyes first and much, hands next and little.' The eyes may tell everything, the hands may hurt very much and tell nothing. A little rough handling at the beginning of an abdominal examination throws the muscles into spasm sufficient to mask a tumour of large size; and the terrified patient usually declines to be hurt again.

What is meant by feeling with intent? Beginners feel without any clear idea of that which they intend to do; perhaps they do not know what they ought to do. An inflamed knee, for example, is handled anyhow on the chance of something being felt. Had the feeling been done thoughtfully and in proper order, the hand would have been gently laid upon the joint to feel for heat, the tip of the finger would have touched the patella to learn whether it had been hidden by the swelling; the patella would have been gently pressed to see whether it lay in contact with the femur, and the swelling would be tested for fluctuation; finally, each constituent of the joint would be felt in turn to try and learn the condition

of the capsule, synovial membrane, synovial fringes, ligaments, cartilages, fibro-cartilages, and bones.

The order in which anything is done should not be left to chance. As a rule it is best to proceed in anatomical order. In a genito-urinary case begin with the urethra, then proceed with the membranous urethra, the prostatic, the bladder, ureters, and kidneys; from the prostatic urethra proceed to investigate the prostate gland, the vas deferens, vesiculæ seminales, epididymis, and body of testis. Similarly follow the alimentary tract from mouth to anus. One of the objects of an anatomical training is to enable you to work in this systematic way. If we proceed in anatomical order the superficial structures are examined before the deep. Thus the skin and subcutaneous tissue of the abdomen are taken before the muscles, the muscles before the peritoneum, the peritoneum before the viscera.

The order of the examination may be determined by the question of pain. Thus, an inflamed joint is long looked at before being touched; the least painful part of the touching is done first, and movement is attempted by the patient before it is attempted by the surgeon. A man's thoughtfulness and gentleness are clearly shown by the way in which he handles an inflamed joint. When a joint is inflamed, its movements depart and return in definite order. In tuberculous arthritis of the hip-joint, for instance, rotation is lost before abduc-

tion and adduction ; and abduction and adduction are lost before flexion and extension. When the joint recovers, the movements return in due order, and those which were the first to depart are the last to return. Flexion and extension come back before adduction and abduction, and adduction and abduction before rotation. This is, I believe, a law for all joints which possess these movements. When the law is ignored and rotation is attempted before flexion and extension, great pain may be caused.

The degree of danger may determine the order of the examination. The abdomen is first examined without anæsthesia ; then with the patient under an anæsthetic ; and last, by means of an exploratory incision. I am accustomed to call this a surgical crescendo. The principle of progression from the easy to the difficult, from the painless to the painful, and from the safe to the dangerous pervades the whole of surgery.

The principles of the surgical crescendo also apply to treatment. For example, in congenital equino-varus the baby's foot may be massaged and manipulated, next wrenched and put in plaster-of-Paris bandages ; afterwards the plantar fascia may be cut, the tendons and ligaments divided, and, at last, the astragalus or portions of the tarsus may be taken away. Each step of the treatment is more severe than that which went before.

Beware of medical terms. Many are the names

of mere abstractions. Such words as 'diathesis', 'cachexia,' or 'dyscrasia', or 'idiopathic' call up no clear image in the mind. No one can show you a diathesis, cachexia, or dyscrasia. It is hopeless to try to define such vague terms. Let it suffice for us to use terms which serve to mark things which can be seen, handled, and measured.

It is useless to think that cases can be remembered and used for purposes of comparison. A few very striking ones may leave a strong impression on the mind, but in time these fade away and are lost for ever. Nevertheless, although cases are forgotten, the training which their investigation ought to give in seeing and thinking is never lost, and becomes a habit which helps not only in medicine and surgery, but in all the affairs of life.

Besides, a long array of remembered cases would not be very helpful, because no two cases are exactly alike. Neither the patients nor the diseases are ever the same. Should you fail to notice differences, begin to suspect that you are wanting in proper powers of discrimination. Each social grade has its own diseases. Rickets and chronic ulcers of the legs are common amongst the poor, gout amongst the rich. So that one who is familiar with the diseases met with in hospitals will be sometimes at fault when his practice lies elsewhere. It will not then help him much to remember a number of dreadful ulcers of the legs or severe cases of genu valgum.

The cases which we see together ought to be pondered over and read about in the textbooks. One who has cultivated the critical faculties cannot read too much. 'Reading maketh a full man.' 'If he read little, he had need have much cunning to seem to know that he doth not.' And Bacon also tells us how to read: 'Read not to contradict and confute; nor to believe and take for granted; nor to find talk and discourse; but to weigh and consider.' I advise you to read big rather than little books; and monographs rather than textbooks, dictionaries, or encyclopædias. It is far easier to read about the disease than about the patient. No complete work has yet been written on human nature; perhaps memoirs and the writings of men such as Pepys tell us sometimes of what we want to know. But whatever you read, let it be read critically, and remember that—

Books are not seldom talismans and spells,
By which the magic art of shrewder wits
Holds an unthinking multitude enthralled.

The proper mental attitude for one who desires to learn clinical surgery ought to be one of doubt and incredulity. 'Without doubt there will be no inquiry, and without inquiry there will be no knowledge' (Buckle). It ought to be instinctive to say, 'What reasons have I for believing that this is true?' But before the mind can weigh the proffered evidence, it must be endowed with knowledge,

judgement, and wisdom. How many confuse knowledge and wisdom !

Knowledge and wisdom, far from being one,
Have ofttimes no connexion. Knowledge dwells
In heads replete with thoughts of other men,
Wisdom in minds attentive to their own.
Knowledge, a rude unprofitable mass,
The mere materials with which wisdom builds,
Till smoothed and squared and fitted to its place,
Does but encumber whom it seems to enrich.
Knowledge is proud that he hath learn'd so much ;
Wisdom is humble that he knows no more.

In describing that which has been seen I would counsel you most earnestly to learn to use the shortest, simplest, and easiest words. Much of my time is given to showing how ill you understand many of the words which are everlastingly on your lips. 'For words are wise men's counters, they do not reckon by them ; but they are the money of fools,' wrote Hobbes of Malmesbury more than two hundred and fifty years ago. It is surprising how little we all know of the full meaning of everyday words.

Take a word which is often used, such for instance as 'normal'. Here is a knee-joint which is called 'normal'. Now tell me why you call it 'normal'. After a while you will learn to base your judgement upon the position in which the joint is held ; upon the appearance of the skin over it ; upon the absence of swelling or tumour ; upon the absence of

muscular wasting or contraction ; on the absence of heat ; and upon the presence of a full range of painless movements. But until all of these—and more, perchance—have been thought of and verified, the statement that the joint is normal is a mere guess, and the word used carelessly and without any clear meaning. The use of pronouns leads to many mistakes. Do not be afraid to call things by their names. We are accustomed ironically to define a pronoun as ‘a word used instead of a noun to make a sentence ambiguous’. ‘Never put an *it* upon paper without thinking well what you are about. When I see many *its* in a page, I always tremble for the writer.’ How dreary, stupid, and bad, is the style of the lecturer who says, ‘Gentlemen, this structure begins here, and after passing over that, ends over yonder,’ and yet this is not a very gross exaggeration of some we have had to endure.

The true teacher may be known because he tries to educate, but not to instruct. Instruction merely teaches to know ; education teaches to think. But it is not enough to know what words mean to yourself ; try and learn what they mean to others. A little while ago we saw together a man who had cellulitis of the leg with death of a large piece of skin. This man was not in the least alarmed when I told him that some of the skin of the leg had sloughed. Asked what he would have thought if he had been told that it had become gangrenous, he

looked anxious and said that he would have thought that that was very bad indeed ; and when asked what he would have thought if he had been told that it had mortified, his face became full of trouble, and he said that he would have thought that he was about to die. All patients give a sinister meaning to the word 'tumour', and therefore it is best paraphrased. It is very wrong and heartless to scatter words at random. A lady was told that her child had had 'a touch of infantile paralysis'. She at once said to herself : 'My infant has had paralysis,' and next : 'I have a paralysed infant,'—but happily all untrue.

Cultivate the habit of attention, for those who have it can fix the mind upon the matter in hand, being at the same time alert and ready to do the right thing at the right moment. Without the habit of attention no one can follow a train of reasoning or listen to a long narrative. Both sick and well dislike one who cannot listen and whose thoughts wander. They also dislike interruptions. 'Do not interrupt' is sometimes called the eleventh commandment, although the cynical give another. The assistant at an operation must possess the habit of attention ; if he does not, his thoughts are sure to be far away at the critical moment when his help is needed. Try always to come to a definite conclusion. A thing either is or is not. It is impossible to escape from this alternative, which few seem able to comprehend. I remember

some one saying that a child had incipient disease of the hip. His bewilderment was extreme when asked whether the child had or had not got disease of the hip. Of course, if the child had got disease of the hip he had got it, and the word incipient merely implied that it was in its early stages. On the other hand, if the child had not got hip disease, then it may be supposed that the word incipient was intended to imply that he was about to get it, and that the surgeon was prophesying. Some, 'too weak to bear the insupportable fatigue of thought,' cannot make any direct statement without qualifying it with some such words as 'I think', 'it seems,' 'a somewhat,' 'apparently,' and so forth.

Do not despise experience. 'Much memory, or memory of many things, is called experience,' and 'much experience is prudence' (Hobbes). The surgeon who makes a mistake during an operation, remembers and does not repeat it. As surgery and medicine become more scientific the need of experience grows less. Fifty years ago it would have needed great experience to recognize an anthrax pustule; to-day the characteristic square-ended bacillus may be found by some one who has never seen the patient, but who can nevertheless give a positive opinion upon the nature of the disease. Experience teaches us to be prudent and painstaking, and not to fall into error. In addition, those who have long been in practice learn a great deal about

the proclivities of their patients. It is known that one cannot take arsenic, another cannot take mercury, and another quinine.

Sir George Humphry's rule, 'Tongue not at all,' need not be taken too literally. It merely intended to emphasize the extreme caution with which questions should be asked and opinions given, when the eyes and fingers have found out all that can be found out by their help. Information of great value can be sometimes obtained by judicious questioning, but always remember that the sick are critical, and that a silly question or remark may sap confidence. Inconsiderate questions are to be avoided. Mental pain is perhaps worse to bear than physical.

In asking questions try to avoid those which suggest the answer. Leading questions cannot elicit the truth. Also ask your questions in an orderly manner, and with a clear intent. For instance, the patient has obscure abdominal pain. Begin at the mouth, and proceed onwards. The mouth suggests the question, 'What can you eat?'; the stomach, 'Have you pain after food?'; the small intestines, 'Do you suffer from flatulence?'; and the large, 'Are you constipated?' 'Have you passed blood?' Of course, many other questions have to be asked, but no important one can be forgotten if you follow this plan, and think in anatomical order.

The unconscious gestures which accompany some

diseases are valuable aids to diagnosis. The gesture of the young adult who is suffering from syphilitic cephalalgia is most striking. When asked whether the head aches, the patient's face becomes woe-begone, the brows are slightly knit, the corners of the mouth depressed, the head drops towards the chest, and the tips of the fingers are placed upon each temple. Added to this the sorrowful voice and upturned eyes leave no doubts as to the reality of the suffering.

In chronic appendicitis the gesture is almost unmistakable. When the patient standing up is asked whereabouts the pain is felt, the trunk is slightly bent forward and to the right, the face being turned in the same direction, and the flat of the hand is placed upon the right iliac fossa. Sometimes the right thigh is at the same moment slightly flexed, adducted, and rotated inwards. These movements of the trunk and limb are instinctive. The question recalls the pain, and the attitude which gave most relief is instinctively resumed. The hand is held flat, to cover as much as possible of the painful area. A strictly localized pain is indicated with the tip of the finger.

Attitude is often a guide. A child with tuberculous caries of the lumbar spine stands in a stooping position with bent hips and knees, and each hand upon the front of the thighs, so as to help in supporting the weight of the upper part of the body. When the odontoid process is carious, both hands

are used to hold the head. It is strange to see the patient rise from the bed by rolling over on the abdomen, and struggling into a kneeling position, and thence on to the feet, the head being meanwhile held in the hands. No one who has ever seen a boy with that strange disease, pseudo-hypertrophic muscular paralysis, can ever forget how he rose from the ground by climbing with his hands up his own legs.

Sir William Savory used to say that mistakes in diagnosis were more often caused by want of care than by want of knowledge. But I seldom meet any one who is not ready to take every possible care, provided that he knows what kind of care is required. The observance of three cardinal rules of diagnosis prevents any serious blunder.

First : Look at the whole patient.

Secondly : Examine the whole of the diseased part, limb, or structure, and at rest.

Thirdly : Compare the two sides of the body.

Again, by looking at the whole patient, how much can be taken in at a glance, 'in a moment of time, for thought is quick'! After awhile you may learn to guess the patient's social position, character, mental peculiarities, general health, and perhaps the disease. Habits of rapid observation and of rapid inference are capable of the highest cultivation and yield astonishing results.

A glance at the patient's face has revealed an error in diagnosis. One day some one said, 'This

girl has a syphilitic rash which does not get well, although mercury has been given until she has been salivated.' The girl's eyes were bright, her cheeks red, her hair glossy. Clearly she had no trace of the anæmia which is always present during the period of general syphilitic eruption. This glance at her face awoke suspicion, and the suspicion was confirmed when the rash was seen to be scaly and upon the extensor surfaces of the limbs, which had been scratched and torn to allay the itching. As a general rule syphilitic eruptions occur most on flexor surfaces, are not scaly, and do not itch. The girl had simple psoriasis, which no amount of mercury would have cured.

Learn to watch the muscles of expression. The emotions of the mind always disturb the muscles of the face. Every one is born a physiognomist, but few cultivate the gift. Most of us are familiar with the commoner expression of anger, pain, and pleasure, and grief, but dead to the subtler ones. 'When he spoke I saw a little muscle at the corner of his eye give a twitch, and then I knew that he was telling me an untruth.' Whole books have been written upon the expression of the emotions. Those of Lavater, Bell, and Darwin, ought to be read and compared. Lavater is full of observation without science, Bell betraying the dawn of science, and Darwin full of observation guided by scientific method and reasoning. Much remains to be done in this fascinating branch of science.

But the rule which tells you to examine the whole patient implies more than this. Over and over again have mistakes been made as to the nature of venereal sores, when the removal of the garments would have revealed a general syphilitic eruption. By use of a little tact and kindness and the avoidance of unnecessary exposure, there is seldom any difficulty in following this rule.

The second rule, which tells you to examine the whole of the diseased limb or structure, is often violated. Here is a man with a small ulcer upon the dorsum of the foot. He has had this ulcer for many years, but latterly it has grown bigger and become more painful. The base of the ulcer is hard, and adheres to the tarsal bones. I remember how much time was spent in endeavouring to tell what kind of ulcer this was, but no one knew until all the garments came off, and brought in sight a mass of enlarged, hard, and adherent glands at the base of the Scarpa's triangle. Then it was clear that a neglected ulcer on the dorsum of the foot had become cancerous, with secondary invasion of the lymphatic glands.

It is also most important to examine diseased structures whilst the patient's body and muscles are at rest. Here is a patient with a mass of enlarged lymphatic glands beneath the left sterno-cleido-mastoideus. As he sits in the chair with his face turned towards the right, the glands feel immovably fixed to the spine, and some would think that they

were enlarged by malignant growth which had spread beyond their capsules into the bone. But now ask the patient to lie upon the couch, and see how the glandular mass can be moved beneath the relaxed muscles. Further examination reveals other groups of enlarged glands in the subclavian triangles.

Examine the whole of the diseased structure. In this instance the lymphatic glands of the neck are diseased. Examine, therefore, the lymphatic glands in the groin, axilla, and abdomen. This can be done in a few minutes. Next, go a step further and feel for the spleen. It contains a great deal of lymphatic tissue. Diseased lymphatic glands sometimes send forth lymph cells into the blood, so that a blood-count has to be made.

The examination of the whole of the diseased structure may be a hard task. To find out the source of a little pus or blood in the urine, the urethra may be looked at with an endoscope; the bladder with another endoscope; the kidney may be photographed with X-rays; the urine separated; and this in addition to the usual examination of the rectum, abdomen, and loins, and the microscopical and bacteriological examination of the urine.

Compare opposite sides of the body, for the sound side is a standard by which to judge the unsound. When a joint is diseased its healthy fellow tells the proper size, shape, temperature, and range of move-

ment. When the bones are at fault, the comparison of opposite sides is all-important, and may prevent serious blunders. I remember being shown the trapezium tubercle on the spine of the scapula, by a man of long experience. He had taken it for an exostosis, until the corresponding tubercle on the other scapula had been pointed out. When the bones are in question, it is wise to have an X-ray photograph taken. Both sides of the body ought to be photographed for comparison, because the shadow picture is so apt to mislead. A curious instance of misconception happened once. A man injured his hip. An X-ray photograph was taken, and as the neck of the femur on the injured side was so much shorter than the other, an impacted fracture of the neck of the femur was diagnosed. The photographer had omitted to observe that on the sound side, owing to the position of his lamp, a longer shadow had been cast by the rami of the pubes, the ilium, the neck of the femur, and by the shaft of the femur, which looked twice its proper width. This made the injured side seem too short; as a matter of fact in the photograph it was the right length, and the opposite side was too long. Opposite sides of the body should be compared in feeling the arteries, testing the conductivity of the nerves, or the strength of muscles; hereafter you will learn to compare the eyes, the ears, the nostrils, and in truth all the paired organs.

Insist upon seeing what passes from the body.

A man complained of a severe pain in the lower part of the abdomen, which was rigid and tender. The urine was passed into a glass, and contained flakes of lymph and muco-pus. He suffered from no increased frequency of micturition, and evidently did not attribute his pain and discomfort to the urinary bladder. A thin, anæmic woman had constipation and colicky pains in the abdomen. The course of the colon and sigmoid flexure was rather tender. After an aperient the motions were seen to contain mucous casts. A woman after parturition had a high temperature and rapid pulse-rate, abdominal pain, and, it is said, frequent vomiting. With very great difficulty a specimen of the vomit was seen, and found to be pneumonic expectoration. It had been assumed that she had so-called puerperal peritonitis.

Decline always to pass opinions on things which you have not seen, and you will escape many pitfalls and humiliations. You ought to be ready to answer, 'I cannot tell, for I have not seen it'; or, 'I was not present.' A pathologist, asked to report upon a piece of tumour, said that the growth was cancerous, and ought to be removed forthwith. Not having seen the patient, he was not aware that the unfortunate man had growth in nearly all the lymphatic glands of his neck. A surgeon was asked what he had found during an operation. In his reply, he advises that no further operation should be attempted, although he had not seen the patient

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for two years. He ought to have thought that in such a long time much might have happened. If you pass random opinions on things you have not seen, you are sure to be guilty of grave injustice to others, and will end by bringing yourself into contempt.

Learn to measure as accurately as you can what you see or feel. It is most unscientific and positively ludicrous to have tumours compared to foetal heads, oranges, apples, nuts, millet seed, and such like. Moreover, it is to my mind exceedingly nasty to compare the secretions and excretions of the body to cream, pea soup, coffee, and other kinds of food.

These are some of the rules which I offer for your guidance. You will learn to use them by constant attendance in the wards and out-patient departments.

II

CLINICAL REASONING

I BEGAN my course of clinical lectures with an introductory one which dealt with clinical methods. Clinical methods are only the methods of obtaining something which the mind has got to use. It is of no use having clinical methods unless you have got a mind which can turn their products over, can extract their essence, and convert that essence into a quintessence which ought to be your diagnosis. The methods which I advised you to use were, first of all, methods which applied to the observer, to the person who is trying to make the diagnosis. First, you must use your eyes. I think most people would manage with their eyes if they never used anything else; if the other sense organs were abolished the eyes would usually suffice. The hands I advised you to use with great care, and your tongue with still greater care. Please do not believe or assume that I do not think the tongue can be used with effect—I mean in diagnosis. The tongue will extract very important information from educated people who have some intention of speaking the truth. Now, with regard to the patient, I advised you to cultivate the habit of looking first

of all at the whole patient. We have had some instructive instances of the importance of looking at the whole patient. I am quite sure that the child in Coburn Ward, and which some of you may have seen, owed its life to the fact that the person who saw it was in the habit of looking at the whole patient, and was in the habit of drawing rapid conclusions from what he saw. In that ward was a wretched child which looked as if it had an enormous head, the reason being, of course, that its body was small, being wasted by illness. In the next place, it was exceedingly anæmic; it had no colour in its face at all. The poor little creature was so anæmic, and its muscles so flaccid, that it sat in its cot in a sort of hump and supported itself wearily with its arms. The muscles of the neck are strong muscles, and they ought to support the head erect and keep it straight upon the shoulders, but in this child the muscles were so exhausted that its head hung down upon its chest. I inquired whose patient it was and was informed that it was mine. I asked what was the matter with it, and was informed that it had cellulitis of the scalp. I remarked, 'There is no such disease as cellulitis.' The disease in this child's case was ascertained by the pathological laboratory, and the report came that the child had streptococci, and some form of staphylococcus in its scalp, probably in its hair-follicles, and above all things, they informed me that it had diphtheria bacilli in its scalp. As soon

as that was ascertained the child was put upon anti-diphtheritic serum, and began to mend, and after great dangers in consequence of paralysis—it had paralysis of the various muscles supplied by cranial nerves, paralysis of the bladder, and so forth—it began to mend, and ultimately got well. I am sure it would have died if the person who saw it had not been keenly alive to the importance of looking at the whole patient, and drawing proper conclusions from the appearance of the whole patient. You will remember the next rule. After looking at the whole patient you were advised to examine both sides of the human body. Here is a most striking instance of the troubles which may accrue from that rule being neglected. I was asked to see a friend of mine—I will not specify further, because it does not reflect credit upon those who saw him. He was said to have great trouble inside his chest, which had resulted in the formation of matter, and that the matter was pointing behind the angle of the right scapula. One naturally asked to be shown the abscess which was pointing near the angle of the right scapula. After examining the abscess I said, ‘I cannot open this abscess, because the abscess happens to be the rhomboideus major.’ This statement of course excited some amount of interest, and he said, ‘Why do you say that?’ He put his left arm into a similar position, and the left rhomboideus major assumed a similar position to what was thought to be an abscess on

the right. Obviously the muscle had been mistaken for an abscess ; perhaps the man was supporting himself in bed with his right arm. Now came the question, What were we to do ? I want you to observe now the methods of clinical reasoning. I may be quite wrong in regard to my methods of clinical reasoning ; but as I advise you to doubt everything and to grow up rational human beings, I assume as a matter of ordinary fact that you will doubt my clinical methods and my methods of clinical reasoning. Should any gentleman doubt my methods and see serious cause to consider that they are wrong, I myself should feel to him under an eternal debt of gratitude if he came to tell me that they were wrong. I should merely ask him to do one thing further and state his reasons for thinking they were wrong. Now, these were the steps in reasoning. First of all, it was quite clear that the disease was of an inflammatory nature. This may seem to you a very elementary proposition. But it is a very important point if you are sure of it. I could adduce cases which had been assumed to be of inflammatory nature, but turned out to be malignant, and bring plenty of examples of things which were supposed to be malignant and have turned out to be inflammatory. So if you are certain of that particular fact, which is one you will often have to decide, you will have proceeded a long step upon the way towards a correct diagnosis. Next, the temperature chart demonstrated

a continuously high temperature, and the pulse record was continuously high; it was usually 108 or 110. Further, assuming that this disease was inflammatory, it was inside the chest, it was not outside. I had been called to open an inflammatory disease outside the chest, so it was of not inconsiderable importance to get it inside. The next step was to tell whether the inflammatory affection inside the chest was solid or whether it was fluid. If it was fluid and composed of pus, one must have pursued the ordinary surgical methods and let it out. If it was solid, one would wait a little while and see what happened to it. Pus is more likely to increase than it is to depart, but a solid inflammatory swelling of the pleura or lung will as often as not take its departure. The voice sounds could be heard, and it was clear that the sound was proceeding through something which was solid. A slight fall in the temperature and in the pulse-rate told that this solid inflammatory trouble inside the chest was beginning to get better. There was a small but continuous drop in the temperature and in the pulse-rate, and over and over again the preliminary to recovery in an inflammatory affection is a slight drop, either in the pulse-rate, or in the temperature. The pulse-rate usually falls before the temperature. When the temperature has been high—102° or 103° F., a small drop in the pulse-rate, say from 96 to 90, has told me, and in future will tell you, that the patient was

getting better and not at a standstill or getting worse.

I remember another case illustrating the importance of seeing everything, and being quite sure of what you see, and relate it to you because of the gentlemen in this theatre many are not going to be occupied either in amputating at the hip-joint or in opening the abdomen several times a day, or, in fact, in doing that very rare kind of surgery which is called 'major'. You will be occupied in other pursuits. I remember a friend of mine coming to me in a state of great distress to say that he had attended a patient in her confinement, and that at the end of five or six days she had a very high temperature and very rapid pulse, and he feared that she had got puerperal fever, which is, of course, another word for sepsis. Now, this was not exactly a surgical case, but to help my friend I went to see this patient. I saw a woman with a temperature of 103° , and whose pulse was rapid, but when one examined her abdomen and pelvis nothing was found to account for the high pulse and temperature. I was told as part of the clinical history that she had been vomiting, or the expression used by the person in attendance was that she had been 'bringing something up'. Curiously enough, difficulties were placed in the way of my seeing what was brought up. But I insisted on seeing what the people said had been brought up

by this patient, and after some trouble obtained a pot in which was pneumonic sputum. On further examination dullness was found at the back of the right lung. From this one was able to say that the young woman had got pneumonia of one lung, and it was not apparently getting worse, and that therefore it was almost certain to end in recovery. You cannot tell what weight of anxiety and care that lifted from the mind of my friend, because he was engaged in practice, and had other midwifery cases to attend, and as an honourable man he could not attend other midwifery cases if he had already under his care a patient suffering from puerperal sepsis.

As regards the importance of the next step in examination of the patient, the examination of the whole of the diseased structure, we have at the present moment in Lucas Ward a patient who is a very good illustration of the importance of examining the whole diseased structure. She has in both posterior triangles of the neck a quantity of enlarged lymphatic glands. These lymphatic glands were diagnosed as being lymphadenomatous, whatever that may be. The enlarged glands in the posterior triangle of the neck having been seen, the examiner was not led to proceed further and say, 'I have found enlarged glands in the posterior triangle of the neck, and will proceed to examine the lymphatic glands in all parts of the body.' In this particular instance the lymphatic glands were

enlarged in the arm-pits and groins, and further, she had enlargement of the lymphatic tissues in the other parts of the body, namely, of the lymphatic tissue in the spleen. It is thought by some that if you name a disease everything is clear. To me it is not so at all. To me lymphadenoma is a phrase. We removed one of the glands from the posterior triangle, and we were disputing as to its nature, as to whether it was lymphosarcoma, or whether it was lymphadenoma. In lymphadenoma I would go so far as to accept this as a true statement, that large endothelial plates can be seen in the enlarged lymphatic glands. I do not know what they mean, and do not know what the disease is or how it arises, neither do I know what the end is; but know that the patients not infrequently die of it. Dr. Andrewes thought that the case was one of lymphosarcoma, and the only thing which shook his opinion was my statement that she had considerable enlargement of her spleen. If she had lymphosarcoma that growth, he thought, would not occur in the spleen, but more likely in the lungs, or possibly in the liver.

Now, I will endeavour to go a step further with our processes of reasoning. And obviously when one looks at a patient, if there be a tumour or a surgical disease, something is to be seen, and something is to be inferred from what is seen. If you go into Kenton Ward, there is an excellent case for you to practise your powers of seeing and

also your powers of inference. You may possibly come to a correct conclusion about the nature of the case. The man in Kenton Ward has a large tumour occupying the left parotid region and the upper part of the posterior triangle of the neck upon the same side. Between the parotid tumour and the tumour in the neck there is a rather deep groove. Naturally you would say this about the case, that the patient has a tumour of the parotid gland and that the swelling below the groove in the posterior triangle of the neck is a group of enlarged glands related to the growth in the parotid gland. That would be the natural thing to say. I should have said that, if I had not a little knowledge which one derives from experience, and which could not very well have been known by the members of my class ; and it is that a parotid tumour has a curious habit of descending into the neck. I have seen a tumour almost in the anterior triangle of the neck, and low down on the sternomastoid, and which was a parotid tumour which had descended down the anterior border of the sternomastoid and lodged where it was seen in the upper part of the triangle of the neck. With that knowledge I was a little reluctant to assume that the tumour in the triangle of the neck was in the lymphatic glands. I rather inferred it was a portion of the tumour which had grown down ; that is what it was because there was complete continuity between the tumour in the parotid region itself

and the tumour in the upper part of the neck. There are many other points about that case to which you should pay attention. In looking at the whole of it it is very important that you should reason anatomically; the important points would be overlooked if you did not. For instance, in the parotid region, besides the blood-vessels which pass through the parotid gland there is a very important nerve—the nerve of expression, the facial nerve. Many members of my class did not observe that the man had facial paralysis. If one of the class had proceeded to operate, and had removed the tumour, and had not observed the left facial paralysis, or the patient's friends had noticed it after the operation, what would the position have been? They would have said, 'In removing this tumour you have done it badly, because you have divided the facial nerve.' As a matter of fact it had been destroyed before, during an attempt which had been made to remove the growth. There is another important matter in connexion with that man. The tumour of the parotid gland may pass along the prolongations of the parotid gland, especially the portion which inhabits the parotid fossa. He could not open his mouth very well, so perhaps the tumour had passed into the glenoid lobe of the parotid gland. Also the tumour was exceedingly fixed, as if it was anchored by some prolongation. But if this growth was malignant it might have penetrated in various

directions and gone through the jaw, or into the pterygoid fossa, or through the temporal bone into the middle ear and so forth. So we ascertained the hearing. If any person were to undertake to remove the tumour, and afterwards the patient found that he was deaf, he might reasonably say the operator had injured his ear. The patient happened to be deaf on the side where he had a tumour, and not on the other side. Perhaps the operator who had already made an attempt to remove the tumour may have injured his hearing. A man in a humble position in life would not be very particular about that, perhaps, but more educated people would consider it a serious ground for offence, and rightly too. In drawing inferences from what you see the patient has got, you must be exceedingly cautious. I am always making mistakes in the matter of inferences. I do not think people make so many mistakes in their observations. Their eyes are educated to see, but the human mind is almost an absurdity; it is always jumping to wrong conclusions, it is always telling you untruths. The mind is an exceedingly idle possession; it hates thinking carefully and taking trouble. Whatever the *ego* is, it has to kick the human mind along to make it take enough trouble and prevent it from deceiving the *ego*. The other day I was asked to see a lady who was supposed to have had an abscess in her left kidney. The reason for thinking she had an abscess in

that kidney was that she had got a great deal of matter in her urine. In addition to that I was told, as a positive fact, that the left kidney had been enlarged and tender. I was called in to see the lady with the view to the removal of her left kidney, which was the kidney which was supposed to have in it an abscess, and which was thought to be a useless organ, and, moreover, a source of danger to the patient. I had to ask myself, 'What do my senses tell me about this patient?' The only thing which I knew for a fact was that I had seen a quantity of pus, say ten per cent., in her urine. Next, the pus must have come from her kidney or her ureter, for a very simple reason. If the pus is from the bladder it is usually mixed with mucus—in other words, it is muco-pus. The pus which comes from the ureter, or from the kidney itself, is not mixed with mucus, and looks, when it is in the urine, almost like a granular precipitate. I had a further report concerning this, namely, that the pus was associated with the presence of the colon bacillus, which is very common, and further that the histological appearances indicated that there were some renal cells in it. I had been told all that, and think it was probably true. I thought, relying upon the evidence, that that pus must have come from the left kidney. But in reasoning about the patient it became clear that we had very little to go upon—merely the presence of pus in the urine and the assumption that

it came from the left kidney; so before proceeding to perform nephrectomy, which is a serious operation, further evidence was required, and it would be necessary to examine the patient under an anæsthetic to see what both kidneys were doing. The patient was given a dose of methylene blue. I confess not to be very learned in regard to methylene blue, but if you take methylene blue the kidney epithelium selects it from the blood and passes it out into the urine. Obviously, if on one side or the other of the body you have not kidney substance which is working, that side will not pick methylene blue out of the blood. The first thing which happened under an anæsthetic in this case was that the endoscope showed that the mouth of the left ureter—that is to say, the one we are speaking about—was red. You may look upon the mouth of the ureter as a sort of optic disc of the kidney—that is to say, that the ureter bears the same relation to the kidney as the optic disc does to the brain. You are aware that when it is some question of disease of the brain—I am putting it into very popular language—you may see the end of the brain in the eye, for the end of the optic nerve is, developmentally, a prolongation of the brain. If that looks red and inflamed, it is a fair inference that the brain is red and inflamed also. If it is swollen and full of venous blood, you may assume that the brain also is full of venous blood. The mouth of the

ureter is very much in the same relation to the kidney, therefore, as the optic nerve is to the brain. With the endoscope we could see the urine descending the ureter in question. So you would have said, 'There is enough evidence to go upon. You have the history of a swollen and tender left kidney, you yourself have seen the pus which comes from the swollen and tender left kidney, and you have seen the red end of the ureter of the left kidney, the right not being red or inflamed.' If you think about it as a matter of reasoning, you will see that that is not quite enough. You are engaged now upon a difficult quest in reasoning. That is not definite, clear evidence. There is a fallacy—you have never seen the pus coming from the left kidney. We proceeded further, and put the separator into the bladder. In a few minutes this is what happened: from the right side of the bladder the urine came out full of pus and deeply stained with methylene blue. From the left side of the bladder a little perfectly clear, watery fluid came out slowly, drop by drop. It was obvious that what had happened to the patient was that she had had a violent attack of septic inflammation of her left kidney, that when I saw her that septic inflammation had ceased and had left a damaged left kidney, so that it could not excrete methylene blue and urea, and that the pus which was seen in the urine was not coming from the left kidney at all, but was coming from the right. Needless to say, no nephrectomy was

done, her left kidney is still there, and I believe the right kidney is improving under ordinary measures, and by using urotropin, and, best of all, going to live by the seaside. I have described this case rather hastily, but you will observe it affords a great deal of food for reflection.

I now propose to tell you of another case which, in my opinion, is a very good instance of correct clinical reasoning. I wonder if the gentlemen here, if given the facts, can reason correctly from them. About February or March last year I saw a stout lady who had had repeated attacks of appendicitis. I need not go into the clinical symptoms of the repeated attacks of that disease, but from a clinical point of view, there could be no doubt as regards the nature of her illness. The attacks had been typical. By that I mean she had the general symptoms of temperature and pulse. Her temperature had been up to 102° ; and she had had a rigor, which always means some serious form of septic absorption. You may always look upon a rigor as a danger-signal. In addition, she had had very violent vomiting during the attack, and also had fainted from the pain. Although she was improving when seen a year after the commencement of the disease, she had not got well, so I concurred in the opinion that the appendix should be removed. I removed the appendix, which was very long, very inflamed, and very adherent. After she had recovered from the

operation I saw her in the month of June, which was the fourth month after her appendix had been removed, and she was then of opinion that she had made a perfect recovery, and was very much better than she had been for a long time. So, clearly, we were upon the right tack. I heard nothing of her for six months, when I was asked to go and see her. I was told that a month before she had been seized with pain in the region of her liver. In addition to that she had had shiverings, and her pulse had been rapid, so that it was when I saw her 120 or 130. In addition, her temperature had ranged continuously high, and was almost 103° F. She was exceedingly ill, and was obviously going to die unless something occurred. I proceeded to reason in the following way. First of all, 'The patient's present illness, whatever it is, is obviously caused by serious septic absorption. If you doubt that fact, look at her pulse-rate and her temperature. But we ought to have a little more information, which can be easily obtained; we ought to know what her blood-count is.' Afterwards I was told that her leucocytosis amounted to at least 20,000, and perhaps a little more. So we were getting clear evidence of a serious septic absorption. The next step in the reasoning was this, Where has the sepsis been absorbed from? We could get one step further in ascertaining this because she was exceedingly tender over the region of her liver. So I went a step

further with my reasoning, and said that the sepsis was in some way associated with the liver. Next, as the tenderness was so much greater at the situation of the gall-bladder, the septic absorption was probably taking place from empyema of the gall-bladder. Dr. Hooper May, a man of great experience and acumen, agreed with that, and said that he had observed very much more tenderness in the region of the gall-bladder. Do you observe that I have not told you I felt it? I am very chary about feeling gall-bladders—indeed, about feeling anything. So there the case stood. Would you now proceed to the rational treatment? which is to give an anæsthetic and remove the sepsis from the gall-bladder. I was reluctant to do that, because of something which was brought to my notice, namely, that near the angle of the tenth rib, on the right side, there was a very tender and rather hard swelling. How shall you reason from those facts—I think they are facts—profound sepsis related to the liver, most probably related to the gall-bladder, and then a lump upon the angle of the tenth rib, which was tender and inflamed? Some might have assumed that the patient was septic, and had a septic periostitis in connexion with the tenth rib. However, I said this about the patient:—Observe that we are now coming to another branch of surgery altogether; and I wish to draw your attention to this other branch, for it is surgical strategy; it is an exceedingly difficult

branch, and one which you have to learn to apply. In this instance I said, 'We can explore the lump upon the angle of the tenth rib, and if that proves to be a mere septic periostitis, it is reasonable to go on and see what has taken place in the gall-bladder. But if that lump on the tenth rib proves to be of a different kind, we shall still know what we ought and what we ought not to do.' An anæsthetic was given and I made a cut down on the rib. It was quite dramatic. First of all, the lump to the naked eye was obviously inflamed. When touched it fluctuated, and on being opened a fluid like pus came out. By means of the finger bare bone was felt, but it was red. A piece of the wall of that apparent abscess was given to Mr. Shaw, and in three minutes I saw the most perfect specimen of columnar-celled carcinoma I ever saw in my life. How did she become infected with carcinoma? I say infected because one cannot help feeling that it was so. I have been looking with extreme care and interest at the vermiform appendix to see if I could discover anything which might be a source of infection. In the first place, we may say she had no tumour there; but a cancerous infection need not necessarily imply a tumour which is obvious to the unaided senses. We know that a little while ago a patient had a tumour removed from her breast of such a size that it might have been covered by a threepenny-piece, but the tumour in her axilla was a huge mass. I

have seen patients with minute ulcers on the tongue but masses of cancerous glands underneath the jaw. There were curious things in this appendix. Of course, when I speak of cancer I am talking about an unknown disease. Nothing is known about the origin of cancer, and it will not be known until people recognize the fact. The appendix was distended with material which may be mucus or it may be colloid material. It had no tubular glands. Next, the epithelial lining of the lumen was proliferating, and it had become converted into mucus or colloid material. At the gap in the appendix through which the little arteries and nerves pass into the meso-appendix you can see this material passing through the muscular wall, and filling up the spaces in the meso-appendix. I cannot see any epithelium in the fluid or in any of the stuff in the meso-appendix. If you saw epithelium there it would be evidence in favour of cancer, but altogether to my mind it is a curious state of things and suspicious. Of course, if a post mortem had been made we might have found a carcinoma in some part of the alimentary tract.

I would now draw your attention to the great danger we are always in of drawing wrong inferences. You may have seen a patient yesterday in the theatre; if not, go to Lucas Ward and you will see a woman seventy-three years of age, who yesterday had laparotomy performed. She was seen by Dr. Champneys, and had in her pelvis a tumour

which felt elastic and hard. The sense of touch is very fallacious. Dr. Champneys declined to express a clear opinion as to its nature, but thought that it was so likely to be malignant that it would not be advisable to operate upon an old woman of seventy-three. I examined this patient myself, and by the vagina felt the uterus fixed; which was, of course, a suspicious fact. Behind the uterus a very hard, rather elastic swelling, which I could not move. Of course it is likely you may not be able to remove a tumour if you cannot move it on examination. *Per rectum* it felt fast to the left wall of the pelvis, another suspicious circumstance. A malignant tumour growing from the ovary was infiltrating the broad ligament and had begun to infiltrate the pelvis. However, the inferences which you draw from things which you merely feel are so apt to be fallacious and incorrect that it is desirable they should be confirmed by the sense of sight, and I proceeded to look and see whether my inferences about the fixity of this tumour were correct or not. The abdomen was opened and a cyst with thick walls was found filling up the pelvis. The tumour was of a curious black colour, and inflamed throughout the whole of its extent. It was stuck to the back of the uterus, to the left broad ligament, to the left side of the pelvis, and also to the intestines which overlay it. These adhesions were easily undone, and I ended by withdrawing

from the pelvis a small ovarian tumour with thick walls and full of ancient blood-clot, due to the fact that it had had a hæmorrhage into it in consequence of its pedicle having been twisted. When a vascular pedicle gets twisted, at the beginning the thin-walled veins are blocked, so that the blood cannot return, while the thick-walled arteries keep open so that there is a discharge of blood into the capillaries, and, as in this case, into the interior of the cyst. I have said before that if the human blood becomes extravasated into any part of the body it begins by clotting and then causes its surroundings to inflame. That was the case here. It was an apoplectic ovarian cyst, very easily removed, and I could not doubt but that the patient will make a good recovery. My confidence happens to be based upon reasoning, because the old lady has a very good nervous system, an excellent heart, excellent lungs, excellent kidneys, and so forth. I saw the contents of her abdomen, and they looked exceedingly good. The thing which was bad about her was the apoplectic ovarian cyst which had been removed, so I think it is only reasonable to think that she will get better.¹

Next week I shall go on to talk of the same subject, and it is a subject which may be of great trouble to you in practice. Please do not think I can make it clear to you, but I will try to make you think about it. The subject is the course of

¹ She made a rapid convalescence.

abdominal inflammation. Because it will happen to you all. 'We know this patient has got an inflamed appendix. I wish I knew what was going to happen. Is the case going to get better, or is it going to get worse, or has it reached a point at which I ought to intervene?' These cases are very difficult to solve. Next time we meet I will try to tell you the kind of reasons which impel me to operate upon these cases of intra-abdominal inflammation.

III

THE COURSE OF INTRA-ABDOMINAL INFLAMMATION

LAST week I endeavoured to show you that the diagnosis of a clinical case included various steps. First, there is the process of making clear observations with your own senses, above all with your eyes, and to some extent with your hands. Next, that from these observations inferences were to be drawn. And then, when you had collected all the inferences that could be drawn from the case you ought to reason from those inferences, and that the ultimate end of the reasoning brought about the diagnosis. To perform all these mental acts correctly requires, of course, a long period of training. You get this training by seizing every opportunity of using your eyes and other senses, and the correct methods of inference you can acquire by watching the process as performed by others and continually correcting your own mistakes. Of course the process of drawing a general conclusion from your inferences necessitates considerable mental aptitude, and you will only acquire that by long and continuous reflection.

To-day I propose to endeavour to draw your attention to a very important matter, and that is the course of intra-abdominal inflammation. Observe I have purposely taken a general subject. It does not want much effort of the human mind to deal with a few trifling particulars, but it is a great effort of the human mind to be more general, and I am myself quite aware of my own deficiencies. But, remember, it is a process which you ought to expect from all who pretend to teach you—that is to say, to teach you to think. I tell you why I have chosen the course of intra-abdominal inflammation for my subject. It is that the course of intra-abdominal inflammation, in the vast majority of cases, can be quite correctly surmised. But if you are incorrect in your surmise as to the course which intra-abdominal inflammation is to pursue, there will be some dreadful disaster to the patient who has committed himself or herself or itself into your care, and in addition there will be some dreadful disaster to your own reputation and to your own fortunes. I cannot help often recalling seeing a youth of about twenty years of age who had had intra-abdominal inflammation for at least ten days. Incidentally I had heard that it was proceeding to a safe issue. But I was asked, one morning early, to see this youth and found him with his hands and feet cold, and a cold sweat on the surface of his body. His pulse could hardly be counted—I should think it was 150 per minute—his abdomen was

rigid, and he was obviously dying. I do not think that he knew he was dying, because those who have the abdomen full of septic material are fortunately in a state of intoxication, so that their eyes are bright, their minds are alert, and often they are happy, and will laugh and talk, and they will have no conception that they are about to die. I cannot help feeling that this condition of septic intoxication is one which often leads to mistakes, as I shall tell you directly. Why had this youth got into his parlous condition? It is true he was known to have intra-abdominal inflammation, it is true it was supposed to be running a safe course, but I do not think that the exact site or situation had been correctly diagnosed, and those who were watching him were not aware of a collection of pus in the pelvis which could be felt *per rectum*, and that its course almost inevitably was to increase and at last burst and infect the general peritoneal cavity, and in all human probability to kill the patient. Another distressing case I recall was that of a young woman who had not long been married. She was seized with a pain in the lower part of the abdomen. There were speculations as to the cause of this pain, but a very shrewd man who saw her said he thought she was beginning to have an acute attack of appendicitis. For some reason, she was placed in other hands, and was watched from day to day. She had a continually high temperature, a continuously rapid pulse, and there was consider-

able tenderness in the lower part of the abdomen. At length the day came when the collection of pus at the brim of the pelvis burst and infected the rest of the peritoneal cavity and slew the patient. When I went into the room to see her, her hair was quite glossy and like the hair of a healthy person, her eyes were bright, she smiled when one entered the room, her mind was quite alert, and at any light remark she laughed. Apparently she had not the slightest conception that she was going to die. I think many of those who were looking on did not know that. But one glance at her abdomen, distended, tender, rigid, especially if one had one's finger on the pulse for a moment (for the pulse could barely be counted), and her cold hands and feet at once told the observer that the end of that unhappy person was approaching. I am sure that those poor wretches ought not to have died in that way. I am not sure that they ought not to have died, but they ought to have died after a better attempt had been made to save their lives. I can recall some splendid successes of people who must have been saved from death by the methods which were adopted for evacuating the pus and other septic material which were present. Dr. Bridges, an old house-surgeon of mine, rang me up one night at a quarter to seven, and said he had that moment seen a child whom he believed to have an exceedingly acute attack of appendicitis, and further, that he thought it was the kind of case I would

operate upon if I saw it. I requested him to get an anæsthetist and nurses, and have a room prepared, and I would arrive by a quarter to nine. At the appointed time I entered the house and saw the child, and before 9.30 p.m. we had extracted from its abdomen a gangrenous appendix, and given exit to a quantity of exceedingly septic matter. The night before that child went to bed perfectly well. At five o'clock in the morning it was awakened by a pain in the abdomen. At breakfast time—that is to say, about eight o'clock—the mother gave the child a dose of calomel—a piece of domestic medicine. It got worse during the course of the day, and Dr. Bridges saw it at a quarter to seven, and I saw the patient at a quarter to nine. The child was lying in bed, and I believe it was reading a book. It did not look very ill, but had acute tenderness over the lower part of the abdomen, and its pulse was 120 per minute. Also I was struck by the appearance of the child. You cannot always define—from want of education in my case—the signs which convey impressions to the mind. But the child was very pale, it had a curious livid appearance, and I remember my remark to my friend Ernest Bridges, namely, 'I am absolutely certain that that child has got something horrible proceeding in its inside, and I shall not be happy until I have seen what that is.' I looked, and found a gangrenous appendix. The next day the child was better, and the day after that we told the friends we thought

it was on the point of convalescing, and that they might now cease to be anxious about it. It was a rapid course of events altogether. Abdominal inflammation is over and over again overlooked because a rectal examination is omitted. I remember a child whom I saw in the country who had been seen by a friend of mine in London. The latter had considered that she was merely suffering from colic, or some ailment of that kind, so she was allowed to go into the country, where the pain returned. But at that period the child had become worse, with a heightened temperature and pulse. It was a deceptive case, and the reason was that its inflammation was proceeding in the pelvis and Douglas's pouch. Now, had a rectal examination been made on that child when it complained of so-called stomach-ache, it would have been ascertained that it had extreme tenderness in Douglas's pouch, and that there would have been a hard, tender swelling there. And the person who performed that examination would have been certain in his own mind that something very serious was proceeding in the pelvis. As a matter of fact that child had a perforated appendix removed from its pelvis. It also had an abscess in its pelvis, and it ultimately got an exceedingly acute form of sepsis, and died in a few hours. It had got some form of septicæmia. The history of this case seems clear. The child had an appendix hanging into its pelvis and containing a concretion. The concretion

grew and was accompanied by chronic ulcerative appendicitis. This had gone on to perforation or gangrene and a pelvic abscess. The next stage was removal of the appendix and general sepsis. To my mind that is a perfectly clear sequence of events. I was struck with the next thing that happened in that family. A cousin was at school, and looked very ill and pale. He was doing the ordinary work at a public school, but continually complained of pain in his abdomen. That pain might very well have put anybody off the scent. In the first stage of bad abdominal pain I should think it a safe rule to say it is always felt in the middle of the abdomen, hence the silly expression 'the dry belly-ache'. But if the pain in the middle of the abdomen is accompanied by vomiting, especially vomiting of bile, then it may be called a 'bilious attack'. This boy had not, when I saw him, a pain in the centre of the abdomen, but one which was entirely on the left side, underneath the ribs. His mother suspected—I do not know why—he might be like the cousin who died of sepsis due to pelvic abscess. How would you proceed with a patient of this description? I have got, now, a routine method of treating these doubtful abdominal cases, and I consider every person with pain in the abdomen doubtful until I know what is causing the pain. This boy was treated in the ordinary manner, the routine way. First of all he was put into bed. I shall mention some very

elementary and rudimentary facts to you which do not occur to those who are immersed only in hospital work. So much of the work is done for you in hospital by sisters and nurses, and you come across so many interesting and good cases, that you are little aware of the kind of case which you may have to deal with later on in your careers. I am going to tell you of an absolutely typical one. Why is the boy put to bed? For a great variety of reasons. The general and local symptoms of inflammation in that boy's case were absent. I only knew that he had pain underneath the left costal margin and some tenderness there. He had no rigid or tender spot in his abdomen, and I think most people, who had examined that boy's abdomen with an unprejudiced mind, would have said there was nothing the matter with it. First of all he was put quietly into his bed, so that it would be possible to get the correct reading of his pulse and temperature. You can never read a person's pulse right if he is walking about or going up and down stairs. The correct pulse-reading, in my idea, in a case of this sort, is taken when the patient is asleep in bed and by an intelligent nurse. A visit by the doctor will send up an ordinary person's pulse ten per minute. So it is obviously important that these patients should be kept quiet when the readings are taken. You will never get a proper reading by the thermometer unless the patient is quiet. If the person gets up and walks along the corridor,

sits in a draughty water-closet, and then walks back to bed, you cannot get the proper reading. It is clear that such a patient should use the bed-pan when the bowels act, which entails a skilful nurse. You may be tempted to say that these are trivialities, but they constitute correct medical practice. For several days the boy's pulse and temperature were normal, and then suddenly the pulse gave a jump, and simultaneously the temperature went up. Being in bed and properly nursed gave us another opportunity of investigating this boy; we knew what went into him, and we saw what came out. There came out a quantity of mucus streaked with blood. Therefore it was clear that he had something odd going on in his colon. He had colitis, whatever else he had. We watched him for nearly three weeks. A friend of mine (Dr. John Harold) looked after him, and one day he said, 'Come and see that boy.' I went up to see him. And we said, 'You have a pain, have you not?' 'Yes.' 'Where is it?' 'It is here, on the right side.' Altogether, he had been going on in this way with these 'bilious attacks' for six years; his abdomen was opened, and an inflamed appendix was extracted from behind his colon. That ulcerated and septic appendix was full of faecal concretions. I guess that that boy had had an escape. But why did he get that dreadfully dangerous appendix removed? Simply because he was put on what I strongly recommend for these patients with doubtful conditions; a period of probation.

I have referred to some of the signs of intra-abdominal inflammation. If any gentleman in this theatre, after what I have said, is deluded by that pain in the middle of the abdomen, he is past forgiveness. The ordinary place for abdominal pain to begin is in the centre of the abdomen. If it is appendicitis, the patient will place it after a while in the right iliac fossa. If the patient has inflamed Fallopian tubes, or an inflamed uterus, or an inflamed ovarian cyst in the pelvis, then, after a while, the patient will place it low down in the abdomen. If it is a septic gall-bladder, the patient will probably place the pain under the right costal arch. Or if it is a septic kidney, with an inflammation of that organ, the pain will be placed after a while in the flank. And in almost all the instances I have given you there is a period in the disease when the patient refers his pain to the centre of the abdomen. There is an obvious reason for that, founded upon a correct appreciation of the anatomy of the solar plexus, a portion of the anatomy of the human body which is singularly neglected, and which will repay any gentleman to work at for his thesis. The locality of the pain may continue doubtful although you have tried to locate it many times. I recall the case of a lady whom I saw yesterday, who was supposed to have had a septic gall-bladder and gall-stones, which was afterwards found to be true. She was asked where the pain was, and she put her hand anywhere down the right side of the abdomen.

But if I had been asked where she put it most, I should say at the region of the appendix. Occasionally, if she was pressed, she put the pain more towards the right flank. After thinking a great deal about the pain of that patient, I should have said she put her pain anywhere down the right side of the abdomen, travelling down the course of the ureter. There were peculiar circumstances. She had never had an attack of jaundice, but she had passed a renal calculus, a uric acid calculus. There was good evidence of that. Whether she had ever had appendicitis I could not tell. As regards her tenderness, it was very difficult to make out where her tenderness was. It might have been in the gall-bladder, over the kidney, or over the appendix, or down the course of the right ureter. I will tell you why I think her tenderness was so difficult to elicit. You will notice I am drawing a distinction between tenderness, or pain on pressure, and the pain which the patient says she feels. I think one of the reasons why the tenderness was so very difficult to locate was that she was exceedingly stout; probably the fat on her abdominal wall was nearly two inches thick. There is no one here who will not be deceived by a fat person. He will be deceived in this way, that he may think the person has fluid in the abdomen, or abscess, when she has not; in addition, he will think such a patient has not got pain and tenderness when she has. The human mind cannot help being scepti-

cal about the existence of pain if it is not accompanied by a corresponding degree of tenderness. I remember being called to an hotel not very far from here, and this was the reason of my being called. They said there was a man in the hotel who had had a very bad attack of abdominal pain and diarrhœa, but that he had recovered and they did not think that it indicated anything serious, but as his friends were far away it was desirable that somebody should share the responsibility of a decision. We found there a very stout person, sitting upon the edge of the bed. He did not look very ill. He had had his breakfast, he had walked across the room, had stood up at the looking-glass and shaved himself, and had just returned to the edge of the bed when I saw him. When he lay down it was difficult to tell whether the rotundity of his abdomen was due to distension, or whether it was due to a huge layer of fat which enveloped it. He said his pain was better. That is a very common story and is sure to deceive. No particular amount of tenderness could be elicited in the front of the abdomen. But he had a pulse of 130, and since the diarrhœa he had passed nothing from the bowels, neither fæces nor flatus. He was a dying man. The person who does not pass flatus from his intestines is, unless something happens, going to die. What did it all mean? That case was cleared up at once. First of all, the pulse being at 130 per minute excited intense suspicion. The finger passed

into the rectum could feel the peritoneum, with nothing intervening between the peritoneum and the finger except the rectal wall. When the peritoneum was touched, it nearly made the man leap out of bed. There is no more tender structure in the human body than the inflamed parietal peritoneum. Perhaps the exposed pulp of a tooth is about as painful, but the peritoneum when inflamed is certainly the most exquisitely tender structure which the body contains. His abdomen was opened and it was found to be full of pus and contained a gangrenous appendix. He died, not many hours after the operation, from septic intoxication. I remember another circumstance in connexion with that case. After the operation, at night, his relatives appeared on the scene, and I said: 'I am going to take you to see your relative. He has had his operation performed, his abdomen has been cleared of the septic material, but he is going to die. Do not be deceived; he may laugh and talk to you with a good voice and be cheerful, but he will suddenly collapse and die. I regret to have to tell you this.' I knew this because he had a pulse which was continuously ascending after the operation. If it had fallen one might have felt hope, but it ascended to 140, and it was certain that his heart would not long endure that strain, and it was equally certain that his cheerful demeanour was due to septic intoxication. I could quote other cases in which the situation of the pain had been ambiguous.

But remember that you must be very careful not to assume that tenderness is absent, for the presence of fat will obscure it, and it may only be elicited by rectal examination.

Now I shall tell you of something else which will hide the local tenderness and which will make it most difficult for you to say whether the patient has got an actually inflamed organ situated in some particular part of the abdomen. Not very long ago I was asked to see a patient who had been exceedingly ill for several days. It was obvious that she was suffering from some form of septic absorption, because she had a continuously high pulse and temperature. No doubt if we had had the Pathological Department present they would have found she had a continuously high leucocytosis. Her abdomen was distended uniformly; her bowels had passed nothing for at least thirty-six hours, no flatus nor fæces. The front of the abdomen was comparatively free from pain and tenderness. It could be moved and pushed and indented; but she had intense tenderness in Douglas's pouch. Directly the finger was passed into the rectum it became obvious that we were dealing with an intensely inflamed pelvic peritoneum. There were two reasons why her abdomen was not painful in front, and this is a point which may deceive you very much. First of all her abdomen was fat, though not inordinately so. Next it was distended with wind. Now, if the inflamed structure, let it be

kidney, appendix, or pelvic abscess, or inflamed ovarian cyst, or inflamed uterine fibroid, or any inflamed structure in the abdomen, be hidden by distended intestines, how can any one elicit tenderness? Why, it is like feeling through a thick air-pad! How, then, shall you elicit tenderness in a structure which is at the back of the abdomen, behind a layer of fat and a cushion of air? The diagnosis in that case was an acutely inflamed and septic organ, either tube or appendix, situated near the right brim of the pelvis, and a septic and perforated appendix was removed, after which she got well. It is sometimes most difficult to decide when to operate upon such cases, but in this the decision was arrived at upon simple and obvious signs. Almost always in such cases it is possible to fix upon what I shall call a determinant symptom. The determinant symptom of this case was that the patient had not passed any flatus for thirty-six hours, and could not be made to do so with enemata; she had inflammatory intestinal obstruction, and though she was not vomiting it was highly probable that the obstruction would continue, and if it did continue she would die. How shall you tell whether obstruction is complete or not? It is very simple to find that out. An irritating enema was given—in fact, I think two of them were given—but there was no passage of anything, not even flatus. So, as these enemata were carefully given, it was quite clear that her obstruction was complete. Some-

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times the vomiting may be a determinant feature. There are degrees of vomiting. I take vomiting in its anatomical order. If a person merely vomits the contents of the stomach, it is clear that the vomiting is not of a very severe character. If he vomits up the contents of the duodenum—that is to say, if the vomited matter contains bile—it is more severe. If it begins to get down to that black material which inhabits the jejunum, it is a decidedly serious form of vomiting, and is of great import. I suppose some people will proceed to allow vomiting to go on to such a degree that at last it gets to the contents of the ileum, in which case it is called fæcal. I do not think to-day such a thing would occur, but I have known practitioners who have said, ‘I did not think it necessary to send for you before because the vomiting had not become fæcal.’ I will not refer to the next alteration of function, namely distension, but you know the function of the intestines, besides dealing with the contents, is to propel them. Inflamed structures do not perform their functions properly, and if the intestines do not propel their contents there will be distension and intestinal obstruction, which are inevitable. I wonder if any one can tell me where the intestinal gases come from. Obviously they are manufactured by the intestinal bacteria. If you grow the colon bacillus in gelatin, you will find over each colony a little balloon of gas, and if you can carry your mind to countless millions

of colonies growing in the intestines, and feeding upon the intestinal contents, such as pieces of vegetable which the patient has been allowed to eat before the illness or during it, you can imagine what a quantity of gas they will produce. But I have often doubted whether that was the whole truth, because I have seen a patient one hour comfortable, and without any trace of intestinal distension, and an hour afterwards with the abdomen tightly distended. Where did all that gas come from in such a short time? There must be other factors besides bacteria in producing flatulent distension, but I think you may safely say that the distension itself is the most important point as far as you are concerned. And you guard against this danger in the preliminary treatment of all abdominal operations by clearing the intestines of the food which the patient has taken, and not allowing any other food shortly before the operation, and in addition by taking care after the operation to see that an accumulation of intestinal gas does not take place.

Now I shall refer to another symptom of intra-abdominal inflammation which is a source of fallacy. It is the muscular rigidity which accompanies any inflammation of the peritoneum, whether visceral or parietal. That muscular rigidity, doubtless, is a protective rigidity. Everyone here has seen, during an inflammation of the hip or knee or any other joint, how firmly the joint becomes

fixed. The muscles become rigid and taut and tend to prevent the inflamed structures of the joint being moved or stretched. Exactly the same thing occurs in the abdomen. The muscles of the abdominal wall become rigid to protect the inflamed tissue underneath. In consequence of that muscular rigidity curious mistakes may occur. I have often seen the protective rigidity which takes place at the upper end of the rectus mistaken for the gall-bladder and for tumours of the pylorus. When this muscular rigidity occurs on the right side of the abdomen it is not unusual for it to be mistaken for appendicular abscess. I think most of these mistakes can easily be avoided, first of all by remembering how fallacious these signs may be, and secondly, by an examination under anæsthesia. Also, if there were a distended gall-bladder there you might possibly find dullness, but carcinoma is also dull; appendicular abscess is as a rule, but it may not be. I think if you exercise a little caution you will not be deluded by that. But when you encounter this abdominal rigidity, which indicates so clearly that there is something inflamed underneath, do not be too sure of the nature of the structure which is underneath. I have seen some very curious mistakes made. On Friday there was a patient whom we knew had a septic kidney. We knew that because there was pus in his urine; we could see the pus emerging from the left ureter. We saw the retraction of the left ureter, which is also an indica-

tion. And then when the patient was placed upon his back and his abdomen was felt, we at once came upon a huge hard mass, which I think I am correct in saying none of us had felt before. So the rigidity had protected the inflamed septic kidney from pressure. And the rigidity in this case was very great because the peritoneum over the kidney was inflamed too. I know that because I have seen it. Curious instances of mistakes due to this form of protective rigidity occur. I saw a very stout woman in the country, who had been seen by a friend of mine, and he concluded that, as she was rigid in the abdominal wall on the right side and it was very tender, she must have acute appendicitis. It was a fair assumption to make as the pain was in the right side. It was clear she had intra-abdominal inflammation, and the pain and tenderness and rigidity were all in the right place for that condition. I was a little more cautious, because I had not seen that inflamed appendix. All I said was that she had some inflamed structure on the right brim of the pelvis, and that as her temperature was continuously high, and her pulse had become increasingly rapid, and she had had rigors, it was clear that unless she had something done she would probably die. The moment she was placed under an anæsthetic a tumour was felt with a line of resonance between it and the right crural arch. Also we could feel the rounded margin of the tumour. My next question to the medical man was, 'Has

she any uterine fibroids?' I thought she might have some uterine fibroids which had become septic. He said, 'No, I am sure she has not.' I said, 'Then she must have a twisted or suppurating ovarian cyst.' Her abdomen was opened, and it was found that she had a suppurating ovarian cyst, which was bulging the right broad ligament. It was very fixed and exceedingly difficult to remove. Clearly in that case the cautious inference was the right one. When you encounter muscular rigidity, remember it sometimes protects malignant growths. What is the common course of malignant growth in the alimentary tract? First, ulceration of the malignant growth, then sepsis from the intestinal contents, and then extension of septic inflammation to the visceral and parietal peritoneum, and to the abdominal wall, with the formation of abscess. The abdominal abscess may be opened by a thoughtless person, who may be surprised to find a fæcal leak which will never heal. Therefore, when a person has a rigid area, do **not** merely say it is due to septic inflammation **pure** and simple, such as from an appendix or from some other organ. You ought to weigh all the possibilities very carefully, and you may ultimately conclude that it is perfectly true you have to do with septic inflammation of the peritoneum, but behind that septic inflammation you may find carcinoma of the colon, such as we saw the other day in the operating theatre, or of the pylorus, or of some part of the alimentary tract.

My time has come to an end. This is a very great topic. Supposing you are brought into contact with a very chronic and mild case of intra-abdominal inflammation, I think the issue is easy. You put the case to bed and give it a period of probation. But the more fulminating cases are more difficult to manage. You will have to decide quickly whether the inflammation is increasing or diminishing. If it is diminishing, the pulse and temperature and leucocytosis will fall and the local symptoms also will diminish. I so often observe that gentlemen do not know how to tell whether the local area of rigidity and tenderness is diminishing or not. If there is any question about it, place the tip of your finger on the abdominal wall where the local tenderness is and give a sharp prod with the tip of your finger at one place. The patient will perhaps say that does not hurt him. Move it about an inch away and do the same, and he will perhaps say it does hurt. Then draw your line round the area of tenderness as ascertained in that way. Next day you will be able to judge accurately whether the tenderness is an inch or two less, or whether it has spread beyond the line. There you have a determinant symptom. Simultaneously the pulse-rate and the temperature will rise. Supposing the patient vomits continuously, you have a determinant symptom. Or if no passage of wind has taken place, you have a determinant symptom; and I think, instead of taking days as your period of probation

in order to ascertain the progress or otherwise of the case, you should take only a few hours. If the patient is getting worse, you will have some determinant symptom to tell you so, and that will be your guide as to whether there should be operation or not.

You may have observed, in the two lectures I have given you, that I have not mentioned any details of operative surgery. My reasons for this omission are simple. To order an operation in the case of grave abdominal disease is very much the same as ordering a dose of medicine, and upon my word it often requires just as much general knowledge and as much trained judgement to order a dose of calomel for a person with serious abdominal disease as it does to order an exploratory incision. You will see plenty of operations performed, and my advice to you is to give the greatest possible care and attention to that which goes before the operation and to that which comes after it—that is to say, the preliminary diagnosis and the subsequent treatment.

IV

ON THE RECOGNITION AND MANAGEMENT OF INTESTINAL OBSTRUCTION¹

I AM sure you will allow that time cannot be wasted when it is spent upon this difficult subject. One might well have chosen a happier topic. There would be some gratification in telling of large numbers of successful appendectomies, ovariectomies, or radical cures. But such narrations are singularly wanting in instruction, and not always a compliment to the intelligence of the audience. As a matter of fact, my own experience of the treatment of intestinal obstruction has been most unsatisfactory, and I ardently long for an improvement. Nor am I singular in experience. Our surgical registrars tell us that in the decade 1892-1901 sixty cases of acute intestinal obstruction (apart from intussusception) were admitted into the surgical wards of St. Bartholomew's Hospital, and that only fifteen survived.

Intestinal obstruction must always be one of the most dire dangers that can befall a human being. Obviously a stoppage in the alimentary tract must

¹ An address delivered October 2, 1902, at the Opening Meeting of the North-East London Clinical Society.

be as fatal—though not so quickly fatal—as a stoppage in the air-passages or circulatory apparatus. But nevertheless, when I look back upon my own fatalities, I cannot but think that a proportion of them ought never to have occurred. Patients have been seen and operated upon after they had reached a desperate state. The delay seems mainly to have been due to uncertainty as to what ‘intestinal obstruction’ is, hope that remedies might ultimately prevail, and doubt whether surgery could succeed. A certain sympathy must be felt for this frame of mind. The literature on intestinal obstruction is, for ordinary purposes, too full of the morbid anatomy of cases elucidated in the deadhouse. Such details ought to be familiar to the operating surgeon, but they are apt to overwhelm the practitioner; and, after all, it is he who is first called in to say whether the patient has or has not got intestinal obstruction. Then as regards the surgical treatment. It must be confessed that hitherto the results have not been encouraging; but the fatalities after operations cannot be attributed to a single cause, such as the operation itself, but to a combination of circumstances. To mention one circumstance, I would instance the want of clear plans for the management of the case from its beginning.

From the clinical standpoint most cases of intestinal obstruction pass through three phases. In the first the question is whether the patient has or has not got obstruction; in the second whether

the obstruction is amenable to medical treatment ; and in the third whether surgical measures should be tried. The chances of successful surgical treatment are oftentimes lost because the first and second phases are so prolonged.

Now let us try to answer these three questions ; but before proceeding let me refer for a moment to the title of this communication. The word *recognition* is used because in so many cases recognition is all that can be achieved. A diagnosis cannot be made except by a surgical operation or post-mortem examination. Now the recognition of intestinal obstruction is a simple matter. It is safe to assume that any one who cannot pass flatus or fæces has intestinal obstruction, and that death will ensue if neither escapes.

At first this proposition may seem rather extreme. A person with constipation oftentimes passes no flatus or fæces for hours or days. But it is not that he *cannot*, but merely that he does not. And how are we to tell whether one who does not pass flatus or fæces has constipation or obstruction ? Let us put aside all those cases in which an obvious cause of obstruction is apparent—such as hernia, an intussusception, a tumour in the pelvis, or a carcinoma of the rectum—and take one in which there is no obvious cause. I myself am accustomed to put the question of the presence or absence of obstruction to the test by administering enemata. Enemata skilfully administered by a medical man

are safe, and often give the clearest information. Let me give some instances which can be recalled. An infant, *ætat.* three years, was sent up with the intimation that she had intussusception. When seen, the clinical signs fitted well with this diagnosis; no flatus or *fæces* had been passed for forty-eight hours. The abdomen was slightly distended; vomiting was frequent and persistent; the child cried out with pain, and put her hands upon the abdomen, where an obscure swelling could be felt. An enema was ordered, and arrangements made to open the abdomen if it failed. A motion followed the enema, and it ultimately became clear that the child had tuberculous meningitis. Doubtless the swelling felt in the abdomen consisted of tuberculous glands. I can also recall the case of an elderly lady who had had for many hours inability to pass flatus or *fæces*. Her breast was the seat of an ulcerated scirrhus, and her lower extremities were paralysed. The abdomen was extraordinarily tense and distended. The obstruction had been preceded by constipation, and had come on gradually. As there was no clear evidence of a permanent mechanical cause for the obstruction, and as there was a possibility of its being paralytic, enemas were tried whilst preparations were being made to perform laparotomy. It was evident that such severe obstruction would only yield to strenuous efforts, and therefore an anæsthetic was administered whilst the enemas were being given. A fair passage

of flatus ensued, and afterwards Drs. Coleman and Colby completely succeeded in relieving the obstruction. I have notes of obstruction complicating paralysis agitans in a man seventy-six years of age. It was relieved by the long tube and enemas. Indeed, these measures may be more efficacious than colotomy in this class of case. For instance, a woman with secondary growths in the liver and peritoneum after carcinoma of the breast was dying of intestinal obstruction. I performed inguinal colotomy, but this gave incomplete relief, and she died on the fourth day. Afterwards no trace of a mechanical cause was discovered.

I believe that enemas given skilfully may be considered a test of the presence or absence of intestinal obstruction; when they fail, surgery is the next resort. As regards the composition of the enema, it will be found that soap and water mixed with turpentine and castor oil is very efficacious. The ingredients are easy to obtain—glycerine may be added, or sulphate of magnesia. In a case of obstruction following the opening of a large acute appendicular abscess, and in which the obstruction was known to be due to peritonitis and not to mechanical causes, an enema of soap and water, castor oil, turpentine, and three drops of croton oil succeeded when everything else had failed. Croton oil is very effectual, but only justifiable when it is known that there is no mechanical obstruction.

The enema is best given with a funnel and tube. Both the amount of force and the quantity can be judged more accurately than with the usual syringe. In addition, there is less chance of introducing air, which, of course, would return, and be likely to mislead.

The temptation is great in cases of supposed intestinal obstruction to put the question to the test by giving purgatives by the mouth. I have learnt to look upon this as a most hazardous and dangerous proceeding, and a grave complication of surgical measures. By increasing the peristalsis of the intestines, purgatives excite pain and turmoil, and may increase the vomiting and exhaustion; the violent peristalsis which they excite is more likely to aggravate the effects of a mechanical obstruction than to overcome it; next, they fill the intestines above the obstruction with fluid and gaseous contents, and aggravate the abdominal distension; and lastly, they are a source of peril when the obstruction is relieved by operation. The following is not an uncommon history:—A carcinoma of the rectum is causing increasing difficulty in emptying the bowels. At first this yields to aperients, but at last a time comes when aperients fail and no flatus nor fæces escape. Stronger aperients are given, and the abdominal distension is increased. Inguinal colotomy is performed. The sigmoid flexure is tight almost to bursting, all its coats inflamed, the peritoneal begin-

ning to tear, the muscular coat paralysed, and the mucous ulcerated. An artificial opening has to be made forthwith. Enormous quantities of liquid fæces escape, and continue to flow until the patient succumbs. Inasmuch as the fæcal discharge does not take place by the natural passage this is not called a death from exhaustion brought about by purging, but is not infrequently attributed to the operation. Now the mortality from inguinal colotomy performed under favourable conditions cannot exceed one or two per cent., whilst under the foregoing it is extremely fatal.

On the other hand enemas are safe. If they fail, the fluid is on the right side of the obstruction, and merely comes back again. The faintness, vomiting, or the erythema which occasionally follow enemas need hardly be taken into consideration; neither a medical man nor an experienced nurse is likely to injure the rectum.

The test may be considered satisfactory if two enemas have been tried without anæsthesia and one during anæsthesia.

In intestinal obstruction the non-passage of flatus or of fæces is speedily followed by abdominal distension—with the advent of distension the gravity of the case is immeasurably increased. Its presence diminishes the chances of relief by enemas. As the late Grieg Smith argued, the tightly distended coils of intestine obstruct one another. Next, it is difficult or impossible after distension

has supervened to localize the seat of obstruction. Even an intussusception becomes obscured, or an intraperitoneal abscess, or a carcinoma of the cæcum or of the sigmoid flexure. Now all surgeons would agree that the chances of a successful operation are vastly greater when the abdomen is slack and when the operation has a clear objective, such as an easily felt tumour. Whilst performing laparotomy I have failed on more than one occasion to find the seat of obstruction, owing to the extreme intestinal distension. But now I feel less dread of extruding and emptying the distended bowels. The distension often necessitates that serious addition to the operation, namely, the incision and emptying of the distended coils. I have had to do that twice this year. An infant recovered after the jejunum and ileum had been emptied. An adult died after a similar proceeding, but his acute obstruction had lasted for eighty hours before it was relieved. Distension has also another evil effect. We are all familiar with the paralysis of the urinary bladder which follows retention of urine. After the stretching the muscular coats refuse to contract, and catheters have to be passed. Now the muscular walls of the intestines suffer in the same way from gaseous and fæcal distension, with the result that after the cause of the obstruction has been relieved by operation an adequate passage of flatus and fæces does not take place, vomiting continues, and the patient dies. When it seems

probable that distension paralysis is established, a temporary opening has to be made in the cæcum or small intestine. Some time ago I operated upon a man with intestinal obstruction and enormous abdominal distension. He was too ill to allow of more than relief by an opening into the cæcum. A few days after this had been made his bowels acted and he recovered. I never heard the ultimate end of his case. Another man's life was clearly saved by an enormous escape of flatus and fæces from an accidental wound in the cæcum. This had been sutured, but gave way, owing to the extreme tension. Not long ago I had occasion to regret that I did not open and drain the small intestine. I saw, with my friend Dr. Courtenay Drake, a patient who had got an appendicular abscess and very severe abdominal distension. The appendicular abscess was with difficulty found and emptied. The appendix was then looked for by separating the ileo-cæcal junction. Whilst this was being done a quantity of gas escaped, and considerably relieved the distension. After the operation relief was obtained by enemata, but not in an adequate degree. I feel sure now that the opening at the ileo-cæcal junction ought to have been enlarged and a drain inserted. Owing to the fluidity of the fæcal flow, artificial openings into the cæcum and ileum are especially repulsive, and only to be made with extreme reluctance. Nevertheless they have a remarkable tendency to close themselves, provided

there is a clear passage throughout the intestines.

I fear there are some who do not recognize that when the passage through the bowels is obstructed, flatulent distension is sure to occur. But a glance at colon bacilli growing in a tube of gelatin would speedily clear up any lingering doubt. At each colony of bacilli is a little bubble of gas produced by the bacilli themselves. The gas is evolved with speed, and continuously. Now think of the countless numbers of the intestinal bacteria and try to imagine the effects of their activity. No wonder the intestines distend so quickly, and no wonder the distension is so continuous and so uncontrollable.

Clearly the speedy onset of distension has to be taken into consideration in the management of cases of intestinal obstruction. If the patient be seen before it has supervened, the seat and cause of the obstruction may be ascertained. A tumour, for instance, may be felt in the sigmoid flexure, the colon, or in the right iliac fossa. And perhaps at this point I may be allowed to interject that the search for the cause of obstruction is sometimes pursued with a lack of method. I myself usually follow the course of the intestinal tract from the anus to the mouth. The rectal and vaginal examinations have demonstrated the presence of impacted faeces, of carcinomatous stricture of the rectum, of intussusception, of carcinoma of the uterus filling the pelvis, or of extensive pelvic inflammation

compressing the bowel. Also of collections of pus in the recto-vesical pouch or in Douglas's pouch. More than once I have felt the recto-vesical pouch filled with collapsed intestines, owing to mechanical obstruction near the jejunum. These collapsed intestines have a very peculiar feel, but their recognition at once shows that it is futile to attempt to give relief by opening any part of the large intestine ; the obstruction is in the small. Median laparotomy is the only resource. After the pelvis has been examined, the course of the sigmoid flexure, left, transverse, and right colon are searched until the cæcum and ileo-cæcal junction are reached ; then the area of the small intestines is felt, the region of the duodenum and stomach, and afterwards the liver and gall-bladder, and other abdominal organs. I am now more reluctant than some to dogmatize about what has been felt but not seen. In cases of obstruction upon which I have operated, the digital diagnosis has often erred. A tumour, diagnosed by others as an intussusception, was a mass of acutely inflamed and suppurating mesenteric glands, undoubtedly tuberculous ; and that which was supposed to be an acute appendicular abscess proved a suppurating ovarian cyst. But although the guess as to the nature of these tumours was wrong, the knowledge of their presence was invaluable, because it afforded a clear objective at which to aim.

I have as yet said nothing about vomiting as

a complication of intestinal obstruction. Early, severe, continuous vomiting is seen in cases which run a rapidly fatal course. Its presence prohibits procrastination. I have seen the worst vomiting in cases of acutely strangulated umbilical hernia, the obstruction being high up in the jejunum, and very acute. Indeed, in such a case I had in 1898 to resect a gangrenous loop of bowel. The strangulation had probably lasted seventy hours. In spite of the unfavourable circumstances the patient recovered. Vomiting, too, was acute and persistent in the case of a young man who had a loop of ileum strangulated to gangrene beneath an adherent vermiform appendix. About forty hours of strangulation sufficed for the death of the gut, which had to be resected. A fatal result ensued on the fourth day. The cause was not ascertained. Likewise in volvulus of the small intestine—a rare event—I have seen early and continuous vomiting. In all of these instances the mere obstruction is not the only cause of vomiting; the injury to the intestinal walls and mesentery and their nerve-supply is a factor, and helps to explain its sudden and violent onset.

Vomiting is a marked feature of a class of intestinal obstruction which in my experience is very common, namely, that which is caused by acute suppurative or non-suppurative peritonitis. Here, again, it is an indication of the dangerous severity of that which is causing the obstruction.

As an indication of peril the character of the vomit is immaterial. Sometimes in a desperate case those in attendance begin by remarking that fæcal vomiting has not set in, as though that in itself were a hopeful sign. All may yet be well if the vomit be not fæcal. Now, in itself the act of vomiting is exhausting and conducive to thirst. First food is ejected from the stomach, then mucus and gastric juice, then bile from the duodenum, grumous material from the jejunum, and at last yellow stercoraceous fluid from the ileum. One would have thought that affairs were becoming desperate long before the jejunal contents had been reached. Vomiting is a distinct addition to the dangers of anæsthesia. I believe some patients die because during unconsciousness vomit enters the air-passages. But I have become afraid of trying to obviate this danger by washing out the stomach. The passage of the tube and subsequent washing has, in the cases in which I have tried it, caused too much exhaustion and collapse. Now this is a very serious matter when a prolonged and dangerous operation may have to be performed. On the whole, the danger of vomiting is less to be feared when the anæsthetist is skilful and does not push the anæsthetic too far, and takes care to keep the head low and well on one side.

I have purposely said nothing about pain or tenderness. Neither of them is indicative of obstruction, but either may afford some slight clue to the

seat of obstruction. Pain, however, is most deceptive. Tenderness accompanied by muscular rigidity is more to be depended upon. But pain has one very evil result. It often leads to the administration of opium or morphia. When a human being is in agony it may seem hard to withhold relief, but the narcotic is sure to accelerate the onset of the intestinal paralysis and gaseous distension. The pain ought to be relieved by the division of the band, the withdrawal of the intussusception, or the evacuation of the abscess.

In the examination of a case of intestinal obstruction it is of course helpful to have in the memory a classification of its possible causes. An anatomical classification, such as is usually given, may be of use in the arrangement of museum specimens, but is not very serviceable at the bedside. My own experience has led me to classify the causes of intestinal obstruction into—(1) mechanical, (2) inflammatory, (3) paralytic, (4) vascular. The last is the only division of which nothing has yet been said. Once I performed laparotomy for acute intestinal obstruction, and found thrombosis of the mesenteric veins. On another occasion the mesenteric arteries seemed to be at fault, having possibly become filled with clot. But even such a simple classification as this has to be used with caution. Take such a case as the following, and one neither unusual nor imaginary. A young woman was suddenly seized with violent abdominal pain,

ceased to pass wind or fæces, vomited, and began to get distended. No cause could be discovered. The intestines were soon in a state of violent peristalsis. With a stethoscope gurglings and 'gugglings' could have been heard all over the belly. The temperature was normal. The pulse eighty-five, and of full volume. The skin was moist. After a while the intestines became stiller, and the distension markedly increased. Then at last the abdomen was very still and quiet. Nothing was heard with the stethoscope. The skin was now dry, the temperature slightly raised, but the pulse 140 and difficult to count. What is the interpretation of such a case? First, a mechanical obstruction; second, a huge peristaltic effort to overcome the obstruction—with the pulse and perspiration which accompany exertion; third, muscular exhaustion and paralysis with dilatation of the intestines; fourth, peritonitis starting at the strangulated loop, which had by that time become gangrenous. As I have already pointed out, the presence of peritonitis causes the most absolute intestinal immobility. I always consider it a hopeful sign if vermicular movements can be seen before the operation is begun. It is clear evidence of the absence of paralysis and of peritonitis.

We now come to the second part of my communication, namely, the management of a case of intestinal obstruction. Some of the most essential points have already been touched upon. And first

it is clear that, inasmuch as intestinal obstruction is inevitably fatal, there should be no pause or cessation in the endeavour to procure relief. I am of opinion that it is the duty of medical men to remain with the patient until relief is obtained. I am fully aware that some may be startled by this proposition, but see how it works out. The patient has passed no flatus and no fæces for some hours or days. Perhaps some of the other signs of obstruction are added. An enema is given, and fails; a second likewise fails. Aid is sent for at once, and an anæsthetic is given; possibly another enema is tried, and fails. Then laparotomy is performed under the most favourable conditions, that is to say, before intestinal distension and paralysis, and, of course, long before the peritoneum is inflamed and cracked, or the mucous membrane ulcerated, or before gangrene has supervened. When this line of management cannot be carried out at the patient's home, an immediate start should be made for the hospital or nursing home.

In about half the cases, by systematic investigation, cause can be found to explain the obstruction. This cause has forthwith to be dealt with. For instance, fæcal impaction may be found, and relieved with enemas. If these are unsatisfactory, they may be tried again after an anæsthetic has been given. We have already had instances of the success in cases of paralytic distension. But obviously obstruction caused by growths in the

bowel, or by pelvic inflammation, is not likely to yield to measures such as these. Surgery ought to step in without delay. When the administration of an anæsthetic has been decided upon, then proper preparations should be made for the performance of an operation. As I have already said, the gravity of the operation depends in a large degree upon the stage at which it is performed. In the absence of distension the abdomen can be systematically explored and the seat of the obstruction accurately ascertained. Accurate knowledge upon this point is of essential importance. Should the distension be so great as to prevent a proper examination, then I believe it is justifiable to extrude the distended intestines through the median incision, and empty them of their fæcal contents. After this has been done a proper search becomes possible. This ought to be conducted in a routine manner; first the ileo-cæcal junction, then the colon, sigmoid flexure, and lastly the small intestines. Sometimes collapsed intestine at once comes into view, and is a guide to the seat of obstruction. I have been entirely deceived by trusting to mere distension of the intestine as a guide to the seat of obstruction. Once it was assumed that the obstruction was high in the rectum; the sigmoid flexure was exposed, found distended with gas, and fixed in the abdominal wound and opened. Gas and a little fæcal matter escaped, but it was afterwards ascertained that the obstruction was due to volvulus of the small intestine.

On another occasion it was assumed that the obstruction was in the hepatic flexure of the colon ; the distended cæcum was opened. At a subsequent operation, when the abdomen was no longer distended, the carcinomatous stricture was found high in the rectum. The cæcal opening was a source of discomfort, and the large intestine got loaded with fæces, so that sigmoid colotomy had to be performed. Whilst one can look back upon a few cases in which a happy guess has been made as to the seat of obstruction, one has also to confess that there are many others in which the guess has been wrong. In the absence of some guiding sign, it seems hardly safe to infer more than that in chronic intestinal obstruction the cause is in some part of the large intestine, whilst in acute it may be in the small. Hitherto I have purposely avoided the use of the terms *acute* and *chronic*. The assumption that a case is chronic oftentimes tends to fatal delay. Chronic cases may at any moment become acute, and even in the chronic irreparable changes are slowly but surely progressing. When the cause of the obstruction has been located it may be easy to decide what to do ; but some of the problems which arise are full of embarrassment. I have already mentioned a case in which, after the removal of the cause of the obstruction, an error of judgement was probably committed in not immediately draining the small intestine through a temporary opening. After the intestine has been distended it

seems as though it cannot be relied upon to propel its contents through the part of it which has been occluded. I remember a desperate case which was under the care of Dr. Wunderlich and Dr. Hutt, and in which acute intestinal obstruction was caused by injury to some coils of small intestine matted together by a long past attack of appendicitis. In this case median laparotomy was performed, and the distended intestines extruded and emptied, and afterwards the matted intestines separated until they became filled with gas. I assumed wrongly that the passage of gas would continue and give relief. Thinking the matter over, three other courses were open: first, a temporary opening might have been made above the entangled mass; second, the intestine above might have been anastomosed with the colon; and third, the matted intestine might have been excised. On the whole, the second of these alternatives seems to offer the best chances of success, and I regret that I did not adopt it. The question, too, of the excision of carcinomatous strictures has to be decided. As a rule, owing to distensions and paralysis, it is advisable to give relief by opening the bowel, and then seize a favourable opportunity for excision. Other problems arise when intestinal obstruction is caused by tumours such as those of the uterus. I remember assisting my colleague, Mr. Harrison Cripps, to remove a fibroid tumour of the uterus under these circumstances. Here, again, the intes-

tines which had been distended were unequal to the propulsion of their contents after the fibroid tumour had been removed. The patient's life was saved by the prompt performance of typhlotomy. Few surgeons could hope to be so successful under such desperate circumstances. In some classes of case it seems now to be well recognized throughout the profession that prompt measures afford excellent chances of recovery. This is notably so with regard to strangulated hernia and intussusception, but it is clear from the figures which I have given that intestinal obstruction in which the obvious cause is not discovered does not yet receive the same prompt attention.

V

THE ESSENTIALS OF A DIAGNOSIS

I DO not know who it was, but some heartless person said, 'It is pleasant to stand upon the shore and watch the mariner struggling with the waves,' and I hope you will find it pleasant to sit upon those benches and watch me struggling with a very difficult problem. However, I venture to think that I shall have a sympathetic audience. I propose to lecture to you to-day upon the essentials of a diagnosis. It seems to me unlikely that you, or I, or any one else will ever make a clinical diagnosis unless before we begin the attempt we are perfectly clear in our minds as to what we mean by a clinical diagnosis. Obviously a correct diagnosis must precede correct treatment, and if that could always be achieved there would be fewer surgical failures scattered about the face of the land. I am not going to try and define what I mean by a diagnosis, for reasons which will presently be made clear to you. Those of you who have read Max Nordau's book on 'Degeneration' may remember that he discusses this question of definition in the most interesting and amusing way, and at last comes to the con-

clusion that there are many words which we cannot define, but which we clearly understand. I belong, as you are already sufficiently aware, to the unlearned branch of my profession. If you belong to the unlearned you labour under grave disadvantages. You cannot resort to the memory for wit, or gather original ideas from ancient books written in ancient tongues. But I have ventured to look into the new Oxford Dictionary which is now being edited by Dr. Murray, and which after twenty-one or so years has almost reached the letter 'P'. Perhaps I am a little prejudiced against the efforts of philologists. I never saw the man, I do not know him, nor was he a friend of mine, who was of the opinion that all words originally came from the earth, because, he said, words had roots, and roots live in the earth. But the roots from which the word 'diagnosis' comes do not particularly help one in one's appreciation of its meaning—'to take asunder, or thoroughly apart', indicating that you desire to take something apart. I have here Dr. Murray's definition, which is, that a diagnosis is 'a determination of the nature of a diseased condition, identification of a disease by careful investigation of its symptoms and history, also an opinion (formally stated) resulting from such investigations'. But I think I shall make it clear to you that whatever definition you may adopt there is more than one kind of diagnosis. And, further, I think that I can make it perfectly clear to you that our ideas

of what a diagnosis should be are varying from day to day, week to week, month to month, and year to year. Indeed, when any new method of investigation is discovered which renders diseases or injuries more objective, so you will find you will have to alter your ideas of a diagnosis. I cannot help observing that some think they have diagnosed a case when they have labelled it with a name. Perhaps I have told you the following incident before—I hope I have, because to my mind it is a kind of revelation of the mental attitude of many towards disease.¹ Two years ago, in the Coburn Ward, my attention was called to an infant whom I perceived to be exceedingly ill, so ill that I thought it could not possibly recover. I asked first whose patient it was, and I was informed it was under my own care. After that I asked what it was suffering from, and I was told it was suffering from cellulitis of the scalp. And there I am sure that that matter would have ended. Those who had given the name of 'cellulitis of the scalp' to that disease were clearly of opinion that they had gone far enough. I remarked at once that there was no such disease as cellulitis of the scalp, and requested to be informed as to what the infection was from which the child was suffering. Afterwards a report came back that in the matter from the cuts in the scalp some form of staphylococcus had been obtained, and also the bacillus of diphtheria. That child was suffering

¹ See page 24, also *Clinical Journal*, January 24, 1906, p. 226.

from the effects of diphtheria bacillus in the scalp. It was treated with the necessary antitoxin, and after a dreadful illness, in which it suffered from paralysis of its ocular muscles, and, I believe, of some muscles of its limbs and the muscles of respiration, it recovered. Perhaps if the nature of that infection had been found out a week or ten days earlier, and if treatment by means of serum had been begun at once, it would not have gone through such a perilous illness. Not long afterwards another child in the same ward was shown to me who was said to be suffering from vulvitis. Clearly, to some minds that was a diagnosis which ought to suffice, but again I requested to have clear and accurate information as to the infection which was causing the vulvitis, and the report came back again that the child was suffering from diphtheria. That child also was treated with the proper antitoxins, and recovered rather quickly. I should say that the disease in the second case was attacked at an earlier stage of its progress. Another condition which is often labelled with what is considered to be a sufficient name is that ordinary state known as gastritis. Not long since we had a patient in Lucas Ward who certainly exemplified the peculiar mental attitude of some towards disease and their lack of appreciation of what a diagnosis should be. That patient first came under my notice suffering from an attack of rather acute intestinal obstruction. In the Midlands she had had her abdomen opened

for intestinal obstruction, and adhesions had been undone, the obstruction had been relieved, and she had recovered, but the cause was not sought for. Some time after that the same condition supervened, the abdomen was again opened, adhesions were again undone, and her intestinal obstruction was again relieved, and again the cause was not sought for. Then again she came under my observation—she was not my patient—with another attack of intestinal obstruction, which I judged was subsiding. I thought that that was not the proper time to perform any operation, and I merely passed her on to the surgeon to whom she belonged. She disappeared from view. However, strangely enough, the same patient came under my care with another attack of acute intestinal obstruction, with which she was very seriously ill, and I then became entirely responsible, not only for her then acute condition, but for her future treatment. After we had got her over the attack of intestinal obstruction I undertook to remove the cause of her trouble, which was, as I guessed, suppuration of both Fallopian tubes. They were removed, and she got well. However, she came again under my care, and I asked, ‘Why is this woman in the hospital again?’ And I was informed that she was not suffering from her old trouble of intestinal obstruction, but from an attack of gastritis. Now, gastritis, to my mind, is rather vague, and I could not help requesting those who were looking after her to diagnose a disease. The

usual steps were taken to diagnose the cause of her pain after food, the tenderness over the region of the stomach, the occasional vomiting, and then it was found that a test meal did not pass through her pylorus within two hours of being given. Next, it was found that the test meal did not go through her pylorus after a longer interval had elapsed. Then it was found that her stomach was very greatly dilated, and further, this dilatation could be overcome by passing an interrupted current through her stomach and making it contract. We had yet to find out what was the cause of this dilatation of the stomach, and perhaps inflammation of the mucous membrane lining it, and then it was discovered that she was addicted to the abuse of alcohol, and that she had the condition which every one is aware is a consequence thereof. Cystitis is another word which is often supposed to connote disease. It used to be thought, and perhaps is still thought, to be sufficient to say, 'So-and-so, who has a high temperature, an accelerated pulse, pain over the bladder, great frequency of micturition, and pus, and perhaps blood in the urine, is suffering from cystitis,' and there the matter is often allowed to rest. But surely that is not sufficient. The cause of the cystitis has to be learnt. Of course it may be stone in the bladder, which would be a comparatively easy matter. But very often cystitis is due to curious, and perhaps as yet not very well understood, causes. For instance, I have

myself of late been brought into contact with a most curious form of cystitis, which one labels 'colon bacillus cystitis'. In one case a lady had a violent attack of cystitis. No cause could be discovered, but the urine was examined and found to be teeming with colon bacilli. She was treated with urinary antiseptics by the mouth, the bladder washed out with antiseptics, and ultimately she recovered. Again, a man became exceedingly ill with great abdominal pain, located chiefly over the region of the bladder; he passed urine perhaps every hour; his temperature was 105° F.; he was delirious, and obviously suffering from some very acute form of inflammation of the bladder. There, again, a bacteriological examination of the urine showed that the urine was teeming with the colon bacillus, and after a very long and serious illness, from which it was thought he might die, he ultimately recovered. And so I might go on. I might tell you of another case of colon bacillus cystitis which I have recently seen, but I want to call your attention to this form of colon bacillus cystitis; there may be some one here who is eager to find some subject for clinical and scientific work, and in this I am sure he will find a most interesting and instructive topic. There is a curious thing about this colon bacillus cystitis, for the two patients of whom I spoke to you were husband and wife. And now again the third case, which I said I had recently observed, was seen by a medical man who

was a friend of his, and I have just of late heard that the medical man has been laid up with an acute and rather severe attack of colon bacillus cystitis. It is a curious coincidence, at all events, and worth taking into account.

Now as to words which are supposed to connote a disease, and which do not. I said there was not one form of diagnosis but several kinds. Surely there is a kind of diagnosis which, as yet, is a purely clinical diagnosis. If I were asked to adduce an example of a purely clinical diagnosis, I think I should take an ordinary case of syphilis, because there you have a period of incubation after exposure, which, of course, helps clinically. That is what the dictionary means by forming a diagnosis, from the history, the history helping in the diagnosis. Then there is, in that disease, the local eruption, which may consist of one or many ulcers of particular characters, perhaps in places which clinical observation has taught us are always followed by an outbreak of syphilis. I mean that if a person has a venereal sore upon a cutaneous surface, it is certain to be followed by an outbreak of syphilis. Next, there will be the general lymphatic engorgement, and next the general eruption of syphilis all over the surface of the body and perhaps upon some of the mucous membranes. Next, there may be alopecia, and, above all, the syphilitic anæmia. On clinical evidence of that sort a very positive diagnosis may be arrived at. But I do not know

whether your minds have observed this, that that diagnosis is lacking in important particulars. When I was in charge of the venereal wards, it was somewhat a surprise to some to be informed that anæmia was an essential and invariable attribute of syphilis. We had blood-counts made of all the patients who were in the early stages of syphilis, and we found invariably that there was about one million diminution in the number of red blood-cells. I do not know whether the hæmoglobin was estimated, but if so I have no doubt it was found to be diminished also. So there, to our clinical diagnosis, we have a thread of science attached. But more still is wanting to make that a proper and complete diagnosis. A little while ago, a patient came who, on reasonable clinical evidence, might have been supposed to have had a syphilitic sore in his urethra, where it could not easily be seen, and where it did not easily make its presence known. I thought I would see what the scientists could do to help the clinical surgeon on an occasion such as this, and therefore I sent on this patient to the bacteriologist and asked him to report to me on the presence or absence of the *spirochæta pallida*. I had his report back saying he could not find it. Of course if the *spirochæta pallida* could be easily discovered it would reduce the diagnosis of syphilis under difficult circumstances to an easy matter, and it would remove it from the list of clinical diagnoses and put it into the list of scientific diagnoses, such

as glanders, anthrax, tubercle, or diphtheria. So there is such a thing as a purely clinical diagnosis. If I had time I would adduce the diagnosis of an ordinary attack of appendicitis as a purely clinical diagnosis. If you listen to all that is said about the diagnosis of appendicitis, and if you hear the opprobrium which is cast upon surgeons for removing healthy appendices, you will begin to doubt the value of a purely clinical diagnosis. But if on scientific evidence you could say to patients, 'You are suffering from ulceration of the mucous lining of your appendix,' or 'You have a constriction at the root of your appendix close to the cæcum, and the appendix is full of septic contents', he would ask you to remove it at once. But until we obtain some more scientific method of diagnosing such a disease as appendicitis it must be obvious that the diagnosis cannot be invariably accurate. However, to those who are accustomed to making diagnoses, the diagnosis of appendicitis is extremely accurate, even on clinical grounds. Perhaps the day will arrive when a man of genius will invent some method for bringing the soft tissues of the body within the range of our vision, just as the X-rays have brought the bones into sight. If I were asked to give you an example of a diagnosis which is purely scientific, I should adduce anthrax. Consider the condition of a patient who comes with a small inflamed place, say upon the face, with this small lump acutely inflamed, having upon

it vesicles and pustules. The surrounding tissues are œdematous and the neighbouring lymphatic glands enlarged. It is clear that the inflammation is spreading, that it is attended with pain, and that the patient is ill. On clinical grounds it would be hard to say that this was anthrax, but on scientific grounds a little fluid removed from that so-called anthrax pustule might, and probably would, show the large square-ended bacillus, which would be quite distinctive. In addition, if a little of that fluid were placed in the body of a mouse, within forty-eight hours it would slay that mouse, and in its spleen and blood some anthrax bacilli would be found. Such a scientific diagnosis as that, of course, can be made apart from the patient, and the person who makes it need never have seen the patient. I need hardly remind you that the scientific diagnosis of diphtheria is made in this way nearly every day.

But there is still another kind of diagnosis, and that is the kind with which you will all your lives be concerned, a diagnosis which in the first place is clinical and in the second place is scientific; and that I would call, for the sake of brevity, a combined diagnosis. If what I am telling you is true, what does it mean? The day will never come when it will be easy to make a clinical diagnosis—the day will never come when the lack of properly trained powers of observation can be atoned for by the test-tube or the microscope. You will have to train your minds in

every possible way to learn to observe, and think, and make a preliminary clinical diagnosis, and you will have to be well educated medical men and know what methods of scientific diagnosis can be utilized.

I said, also, that our ideas or concepts of a diagnosis must of necessity be constantly changing. I have given you several instances which will have conveyed to your minds some idea of the truth of this proposition. For instance, take the ordinary examination which is made every day of the results of injuries, such as fractures, and how our notions of them have altered since the X-rays were first placed in our hands. I can remember the days very well when fractures were diagnosed with extraordinary certainty and very positively, and the unhappy student was in serious trouble if he could not divine the guess which the examiner had made about something which neither of them had ever seen. But I observe that many are still unaware that the diagnosis of a fracture is incomplete until a proper examination has been made of it by means of the screen or the photographic plate. The other day the following happened. A lady was driving; the dog-cart overturned, she was thrown out, the splash board fell on her leg, and the small bone was said to be broken. The reason that was said was a purely clinical one; there was great bruising about the junction of the middle and lower third of the fibula. Next, pressure upon the fibula high up caused considerable pain

where the bruising was ; therefore it was concluded the fibula must be broken at that spot. At the end of three weeks she was no better. She, being an intelligent person, had begun to have doubts as to the diagnosis, and asked to have the leg photographed. She was told it was unnecessary ; so she came up to town, and had not been there many hours before the screen showed there was no fracture of the small bone of the leg. In a few hours a photographic plate also showed there was no break. Now, I venture to think that is not creditable. Surely educated medical men ought by this time to have thought the problem out and to have been sure that an essential part of the diagnosis of a fracture is a proper X-ray photograph.

I might give many instances of the influence of new inventions upon our ideas of a diagnosis. The bladder endoscope has done a great deal ; it has almost revolutionized bladder surgery. The use of the endoscope is a long step towards the diagnosis of some forms of cystitis. I remember an unfortunate student of this hospital who ultimately succumbed to genito-urinary tuberculosis. His bladder was examined endoscopically because he had cystitis. His urine had been examined over and over again by bacteriologists who had failed to discover tubercle bacilli in it, and therefore it was supposed that his cystitis could not be tuberculous. However, with the aid of the endoscope, tubercles were seen in his bladder. Tubercle bacilli

were not seen in his urine because the tubercles were not yet softened, and so no bacilli had passed into the urine. Later on he got most acute and severe tuberculous ulcers of the bladder, tubercle of the prostate, and tubercle of the vesiculæ seminales, and ultimately of his kidneys, and died of this dreadful, and I venture to say preventible, disease.

Another instrument of precision which has altered our views very much, or ought to have so done, is the separator. There are various forms of separators to separate the urine of the kidneys. We had a most instructive example of the value of that a little while ago, because a patient was sent up with a diagnosis of stone in her left kidney. The stone was said to be in her left kidney, and I think anybody would have accepted that diagnosis as being correct, because her left kidney was enlarged and very tender; but if she had stone in her left kidney it seemed to me to be important to know, not only what was going on in her left kidney, but also whether her right kidney was competent to carry her through such a severe operation as nephrolithotomy. An X-ray photograph was made, and that revealed the fact that she had a stone in the left kidney and also a very large one in the right kidney. Therefore, this was the position of things: stones had to be removed from both kidneys. And if so, both at the same time? Or if one, which one? We proceeded in the following way with this new

aid to diagnosis: we passed a Luys' separator, and found the urine which was coming from the left kidney was merely a thin, watery fluid with only a bare trace of urea in it, so it was clear that the left kidney was hardly working at all, but the right was excreting urine with plenty of urea in it. Our course was clear as to which kidney to operate on. I removed as rapidly as I possibly could a stone from the left kidney. No special efforts were made to tie small vessels or to close the wound. In about a quarter of an hour the patient was taken from the theatre and put back to bed. She got over that crisis, and afterwards came back to have the stone removed from her right kidney. You are aware that an operation upon any kidney may make it strike work and cease to secrete urea. So if her left kidney had not begun to excrete then it was very perilous to operate upon the right. Under such circumstances it might have been better surgery to have left the stone in the right kidney. However, after again passing the separator we found the left kidney had begun to function again and was passing a reasonable quantity of urine, and in that urine a reasonable quantity of urea, and so we proceeded to remove the stone from the right kidney, and she got well. I venture to present this to you as a very excellent example of the influence of new methods of diagnosis upon your ideas of what a diagnosis should be.

Now, what conclusion may be drawn from the

various examples that I have given you of inflammation of the scalp, inflammation of the vulva, and, we will say, of the bladder? I think the conclusion is this, that you may lay it down as an axiom that the diagnosis of an infective condition is not a diagnosis until you know what that infection is. That means a great deal. It means that we are brought in contact with an inflamed condition due to an infection: not only have we to proceed on clinical lines to determine what organ is inflamed and to what extent, but we have to determine what the cause of that inflammation is; and that means rather a difficult, perhaps a prolonged and extensive, line of investigation. And I cannot help remembering that not infrequently when I have ventured to argue that this was right, I have been met with the argument that these things are impossible in practice. But I put it to you, gentlemen, if you think they are right you know they are not impossible. There are excellent clinical laboratories now springing up in all parts of this great city, and, indeed, in all parts of the country, and so it is not impossible. It may be troublesome, and it may be expensive, but surely if you, after thinking over the matter, have made up your minds that it is right, I am quite sure you will carry it out. I trust I have made myself clear, but I have been attempting what, to an individual like myself, is particularly difficult, and that is to discuss the meaning of a medical term.

VI

SECONDARY INFECTION OF THE LYMPHATIC GLANDS IN MALIGNANT DISEASE OF THE TONGUE¹

I AM going to lecture to you to-day on the subject of secondary infection of the lymphatic glands in malignant disease of the tongue. I have chosen this topic for two reasons. First of all, it is a dreadful disaster to the patient for any one to overlook a gland which contains cancer. If but one is overlooked, whatever else may be done, the patient is left with cancer in his body, and that cancer inevitably grows along the lymphatic channels into other glands; it grows within the capsule of the gland until the whole capsule is filled with cancer, and then it grows through the capsule of the gland and into the surrounding tissues. By the time this has occurred there is so much cancer in the patient's body that removal is almost certainly out of the question. To attempt it may end in

¹ We are indebted to Messrs. A. Constable and Co., Westminster, for the illustrations accompanying this lecture. They are from the authorized English edition of *The Lymphatics* translated by Mr. Cecil H. Leaf.

disaster, and the attempt is sure to end in making the patient desperately ill ; it is almost certain to end in increasing his sufferings. Now there is another reason why I think it right to speak upon this subject, and it is to draw your attention to something which you can do now, and which you cannot easily do hereafter. You will agree with me that your knowledge of the anatomy of the lymphatics and of the lymphatic glands is vague. First of all, the subject is not very fully treated of in the text-books of anatomy usually placed in your hands ; and in the next place, in the dissecting-room the lymphatics are not easy to see. You may discern the largest lymphatic glands, and you may see the smaller ones if they are enlarged, but many of the lymphatic glands which are apt to be infected in the course of cancer are not seen at all in the dissecting-room. They may be too small, and as I shall presently show you, they are in numerous out-of-the-way places. Next as regards the lymphatic channels. Who sees the lymphatic channels ? I venture to think that there are many gentlemen in this room who have passed their College examination, and perhaps their Fellowship, and probably the only lymphatic channel they have seen has been the thoracic duct. And I venture to think that they have not seen that very easily and well. The reason is, that the lymphatic channels are exceedingly small, they have very thin walls, and when they are collapsed they are very difficult to

see and find. The knowledge of the distribution of the lymphatic channels is gained in two ways. One is by injecting the lymphatics with something. Sappey was one of the first who really made a mark at this subject, and he worked assiduously at the lymphatics by injecting them with mercury. And if any one is in Paris they should go to the Dupuytren Museum, where they will see his beautiful specimens. When I was a pupil of Dr. Klein, many years ago, we used to inject the lymphatic glands with Prussian blue; a hypodermic needle was passed into the gland and a solution of Prussian blue was forced into it, and it went along the lymphatic channels. Later the method of injecting dyes has been further elaborated by Gerota, who has added considerably to our knowledge. But now I am going to tell you another way in which you should acquire a knowledge of the lymphatic glands, and often in my experience it is better and more useful than anatomical methods. I mean the clinical method. For instance, I shall shortly tell you about a lymphatic gland which you ought always to look for, but which in the latest work on the lymphatic glands is only marked in one of the pictures as, I think, a lymphatic nodule on the track of the lymphatics (vide Fig. 2, e). And if any of you wish to pursue this very important subject further, I would draw your attention to a book which was issued towards the end of last year on the subject. It is called *The Lymphatics*, and it is

part of a great work on anatomy which has been prepared under the superintendence of Professor Poirier. This particular part of the work has been exceedingly well translated by Mr. Cecil Leaf. The general anatomy of the lymphatics was written by Dr. Delamare. Observe as regards these lymphatic glands that your opportunities are now. If you do not seize every possible opportunity you now have of looking for lymphatic glands, and investigating them, and knowing where they are situated clinically, you will not hereafter have the opportunity; and if your attention is not drawn to the matter, as I am endeavouring to do, perhaps you will never look for them. You may not think it is a necessary part of your profession. I will tell you one other reason why I think this knowledge of the infection of lymphatic glands is so important. It is this. Supposing a person has a sore upon his finger or toe, the lymphatic infection from that sore pursues a definite course. You trace it probably along the lymphatic channels, and you would look for it with certainty in the nearest lymphatic gland. For instance, for the arm it would be the epitrochlear gland, in the leg one would feel in the popliteal space, and so on. But in the case of the lymphatic glands of the tongue, it is perfectly true that as a rule they are infected in proper order, that is to say, the nearest ones are infected first. But you cannot in the least degree rely upon this, because infection of the lymphatic glands in carcinoma of

the tongue may be quite erratic. For instance I will narrate to you a case which may help to impress the matter upon your minds. A very old lady came with epithelioma on the edge of her tongue, far back. I think she was seventy-nine years of age. The epithelioma, as far as could be ascertained, had not infected any of the neighbouring lymphatic glands. The pillar of the fauces was cut through, and the epithelioma was removed with a margin of three-quarters of an inch all round. For a year afterwards nothing happened, and this very old lady appeared to be quite well. But a year afterwards she came, not with a growth of epithelioma in her tongue, and not with a growth under the angle of the jaw, which would be the nearest part, but on the opposite side of the neck. I will tell you of another case which may help you to think of this. A man had had a tooth extracted because of toothache, and then something grew from the socket of the tooth. The growth was accompanied by intense pain. A portion of the growth was removed and examined by an expert, who wrote back saying that the growth was epithelioma and that it ought to be removed at once. I mention that little point to you as a warning, because this expert was going beyond his province; he was asked to give an opinion on that which he could see: he was not asked to give an opinion on a thing which he had never seen. Had he seen the patient he would have known that he had on

the opposite side of the neck a mass of enlarged glands, which without doubt were epitheliomatous too, and no operation could have been done for his benefit. So unless you are aware of this fact you may, given a person with a small epithelioma of the tongue, be guided by your ordinary rules in the examination of the patient, and pay great attention to the nearest lymphatic gland, and forget to look on both sides of the neck. I have often observed that difficulties were encountered in the diagnosis of epithelioma of the tongue. As you are aware, epithelioma generally appears upon the side of the tongue. My own idea is that epithelioma is some form of infective disease; that the first step towards infection is a sore or abrasion, so that the infection can get in, and that when the infection gets in it produces its result as an epithelioma. One has so often been struck by seeing people who have had chronic syphilitic ulcer of the tongue for several years, and who have gone on sometimes getting a fresh ulcer and sometimes the old ulcer healing, and then at last getting an epithelioma. I remember a patient who came with a chronic ulcer on the dorsum of his foot, and said he had had it twenty years. To look at it was an ordinary ulcer which had been invading the skin, and had become attached to the bone. It was epitheliomatous. He had a huge mass of epitheliomatous glands in his popliteal space, and others in Scarpa's triangle. Confronted with such

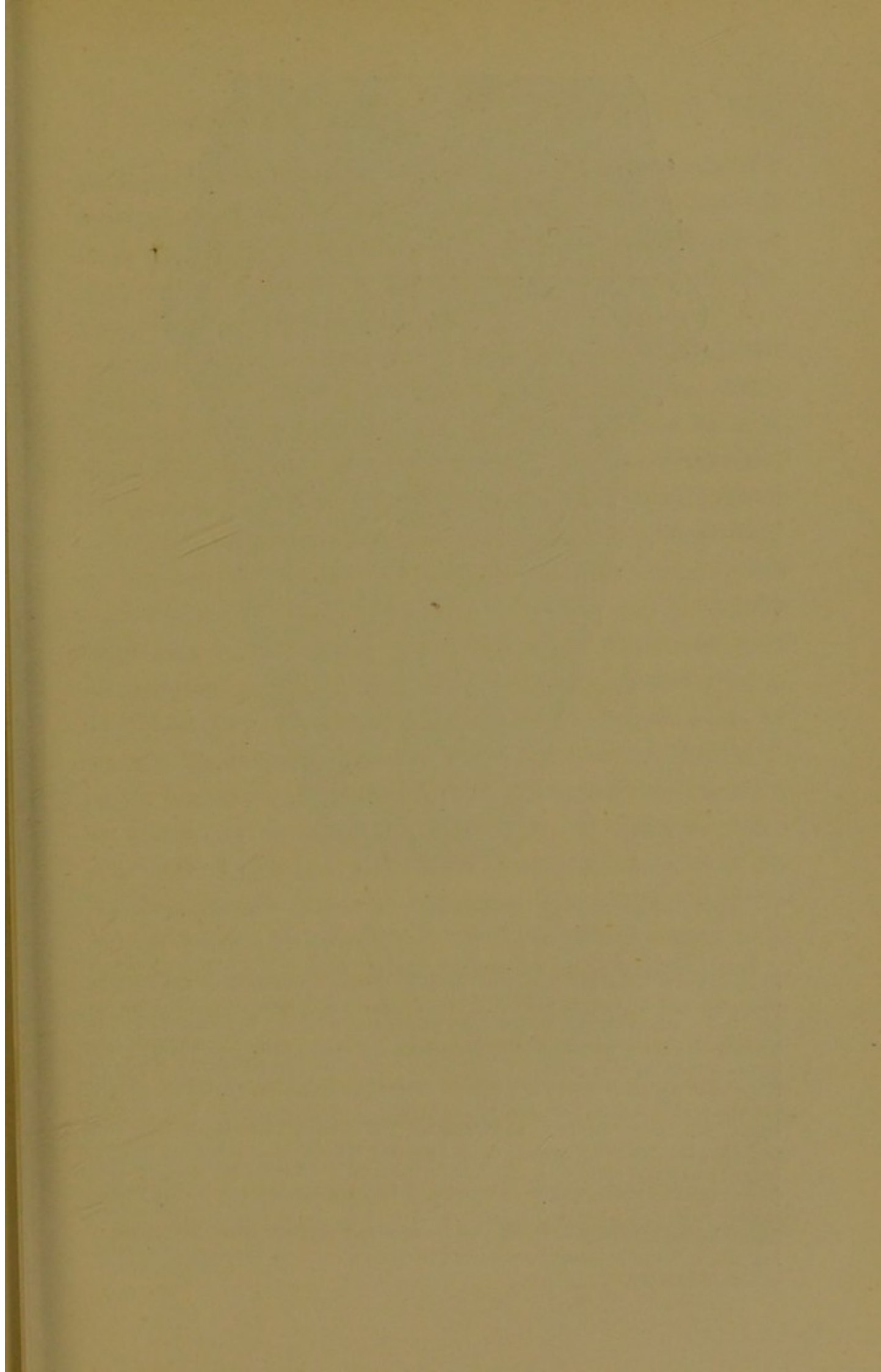
an epithelioma, I have often observed that students were not clear upon one fundamental point. One asks, 'Has that man got epithelioma?' The answer is yes. Why? Because he has got an ulcer with undermined edges, or something of that kind. But have not you a better reason than that? The reason is that he has got a *tumour*, and until you can satisfy yourself that he has got a growth how can you say that he has got an epitheliomatous growth or an epithelioma, or that he has got an ingrowth of epithelial tissue? He must have a tumour; and you must be able to recognize that tumour in its very early stages. If you do not, the growth of it is inevitable; its extension is inevitably along the lymphatic channels, and there will be a fatal ending unless you diagnose it soon. So your responsibilities are very great in this respect. How would you proceed if the tumour were very small and the ulcer slight? Remember it is so important to diagnose it early. In the out-patient department you must have seen how we proceed over and over again. A little cocaine is put on the edge of the ulcer, and a piece snipped off and examined microscopically. Think if you diagnose epithelioma in its early stages how much simpler and safer the operation becomes. At this early stage a portion of the tongue can be cut out wide of the disease, and in the early stages the mouth can be properly prepared, and if the operation is well managed the wound can be closed, and,

as a rule, will heal by first intention. I am now telling you something which is not quite in accordance with that which is usually taught. There is still a tradition that you ought not to close wounds in the mouth. In my experience you can safely do that with catgut, provided you take great pains in disinfection and in antisepsis by using iodoform. The danger used to be lest such conditions as erysipelas and cellulitis of the floor of the mouth and of the pharynx should be set up, or cellulitis of the neck. But that is not a danger which is present in people's minds now. What other clinical characters can you look for in epithelioma of the tongue besides the presence of a tumour which degenerates and ulcerates? It has the usual characters of malignant disease. A growth which began in the mucous membrane, if it has attained any size, will have spread into the muscle of the tongue, where you can feel it. Next, being very vascular it bleeds a good deal; it is sure to bleed. And next, perhaps because it grows into the nerves themselves, it is attended with great pain. If you find this particular character in an ulcer surely you must be very suspicious as to its nature. One asks students why they think it is an epithelioma, and they say that the ulcer is epitheliomatous because the glands are enlarged. But glands are enlarged in so many diseases that that is not a very helpful sign. They are probably enlarged in epithelioma for two reasons—first, because along the lymphatic

tracts of the epithelioma some septic absorption takes place and the glands become septic ; secondly, the usual reason is that they have got cancer growing in them. There are a few troubles in the tongue which you ought not to mistake for epithelioma, but it is often exceedingly difficult not to mistake them. For instance, gumma of the tongue. That ought not as a rule to be mistaken for epithelioma. The gumma is a lump in the substance of the tongue, and at the beginning covered with mucous membrane, and the centre of this gumma is in a condition which indicates degeneration. The gumma spreads to the mucous membrane, and the mucous membrane undergoes degeneration ; then the contents run out and leave a sort of crater. Occasionally tubercle of the tongue occurs, but I do not think you often mistake that for epithelioma. It is usually very acute, and it will usually be possible to find the tubercle bacillus. In addition, the people I have seen with tubercle of the tongue have got tubercle elsewhere—in the pharynx, in the larynx, or in the lungs. Still, there are occasions on which this condition may be very hard to diagnose. I will not mention any of the rarer conditions, but I would draw your attention to a source of error which occurs very often, and has occurred only in the last fortnight. If you look at the margins of anybody's tongue you will see the foliate papillæ, and you can trace them in front of the anterior pillar of the fauces, and can see there on the edge of the

tongue some deep grooves with the projections between. I do not know whether, as Krause suggests, these are the continuations of the circumvallate papillæ of the tongue, or whether, as is said in the last edition of that most excellent and most accurate book, *Quain's Anatomy*, they are an analogue, or at any rate resemble the papillæ foliatae of the rabbit. Sometimes, if you look at the margin of the tongue far back you will see these projections rather red, and perhaps ulcerated. They get into this condition in people who do not keep their mouths particularly clean, and who in addition are wearing ill-fitting false teeth. The diagnosis is easy. When you feel the epithelioma it is exceedingly hard; it is a tumour consisting of epithelium, and therefore you can understand it being very hard. Have you ever thought how hard epithelium is? The surface of the body is covered with a very thin epithelium which turns aside scratchings, rubbings, antiseptic lotions, and so on, it is so very tough. You can easily believe that when a portion of this grows down into the substance of an organ like the tongue, you can feel this hardened, thickened epidermis, almost cartilaginous in its hardness. I should be inclined to think epitheliomatous tumours anywhere are hard; certainly cylindrical epithelioma of the rectum is very hard.

You are sure to have opportunities in the various departments of looking at the mouths of out-



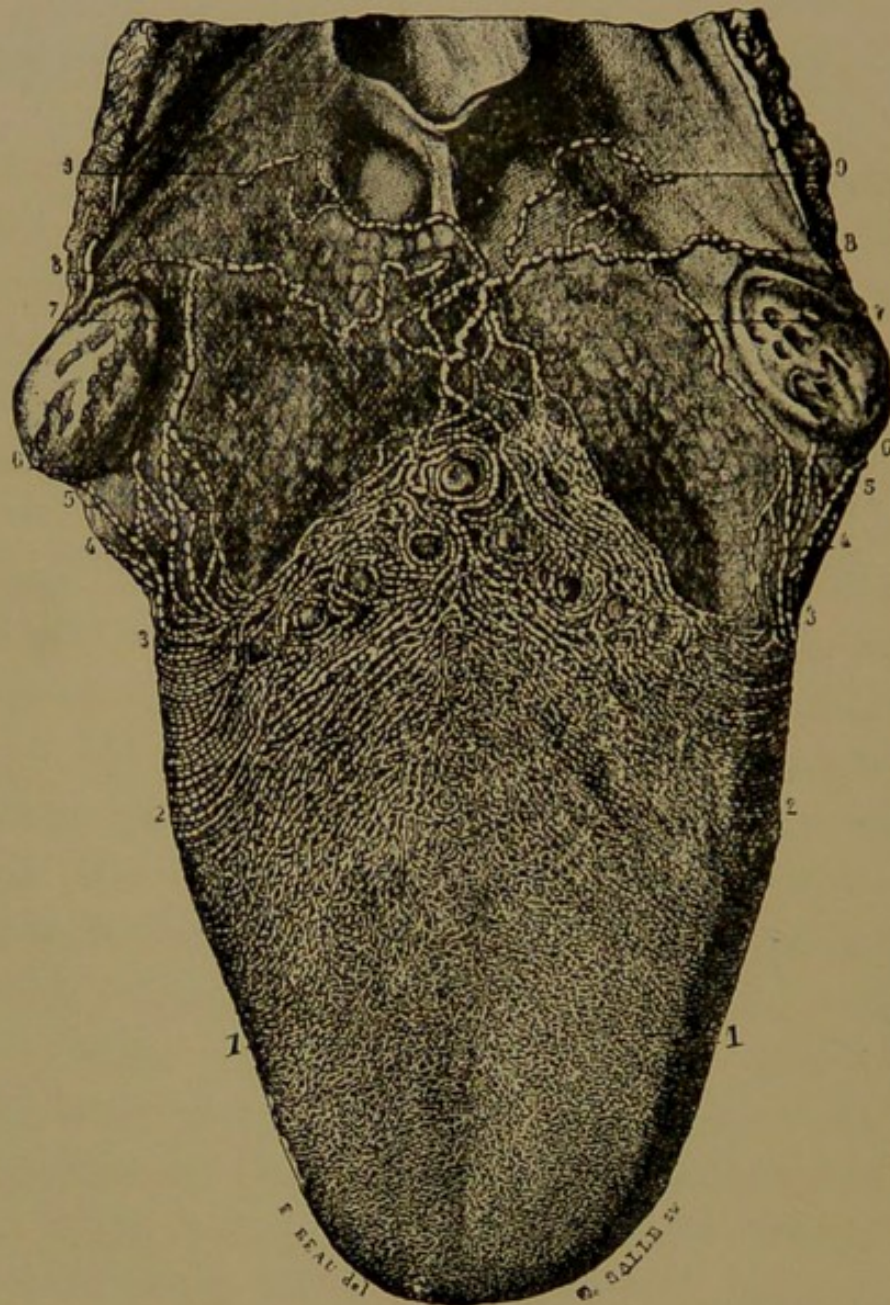


FIG. 1. LYMPHATIC VESSELS OF THE DORSAL SURFACE OF THE TONGUE (POIRIER, AFTER SAPPEY)

1, 1. Lymphatic network of the anterior third of the tongue, formed of radicles of extreme tenuity. 2, 2. Lymphatic network of the middle part, formed by larger radicles, especially on the borders of the tongue, converging from behind forwards, and from without inwards, and following the same course as the papillary grooves. 3, 3. Network which corresponds to the calciform papillae: it is composed of much larger truncules which creep round and encircle these papillae. 4, 4. Lymphatic trunks which arise from the lateral parts of this network. 5, 5. One of these trunks which runs external to the tonsils, and passes into the middle glands of the neck. 6, 6. Anterior lymphatic glands of the soft palate anastomosing with the lateral trunks of the dorsal surface and forming with them a small plexus. 7, 7. Another lateral trunk which passes internal to the corresponding tonsil. 8, 8. Trunks which run from the middle part of this plexus. 9, 9. Other smaller trunks, stretching from the preceding, and disappearing like them through the walls of the pharynx.

patients, and most of them have exceedingly well-marked papillæ foliatae. The papilla foliata, when it is enlarged, is soft, not hard. Another thing is that there is also one on the opposite side of the tongue, so no one ought to make a mistake.

I said just now that the enlargement of lymphatic glands did not conform to anatomical rules. Perhaps it is a little hard on anatomical rules to say that, because it may not be the fault of the rules. As a matter of fact, I am going to try and give you a rule for the distribution of the lymphatics of the tongue, and you will see as I proceed with my clinical remarks upon the distribution of cancer that the cancer does conform very much to the anatomical rule. I will put on the board a drawing of a tongue. It is very difficult to realize what is meant by a lymphatic area. In my mind the lymphatic area of the mucous membrane or of the skin of the body is pictured as a sort of sponge; the spaces are the lymphatics and the solid matter in between represents the tissues. The only question in my mind is whether I am sufficiently accurate, because I believe that the abundance of lymphatics in the skin and in the mucous membrane is scarcely correctly conveyed to the mind by such a simile as this. The lymphatic area on the surface of the tongue intercommunicates everywhere, so that infection starting at one side of the tongue will speedily spread a considerable distance (vide Fig. 1). Presently I will show you how it will spread to the

other side of the tongue, and I will explain what happened in the case originally mentioned. You will see that there are lingual lymphatics which run from the tip towards the frænum of the tongue and empty into a lymphatic gland which is situated underneath the mental process of the lower jaw (vide Fig. 2, *j*). That is the sub-mental gland. Think of removing the tip of the tongue for an epithelioma and forgetting to examine carefully under the mental process to see if the mental gland was enlarged. In removing enlarged lymphatic glands from the neck I always make my incision so that I shall be able to get at that gland. I imitate Mr. Butlin and turn back a flap with its apex at the mental process and its base at the anterior border of the sterno-mastoid, and I do not think I have quite done my duty until I have looked at the sub-mental gland not only on the affected side but also on the opposite side. It is easy to get at them, and it is an essential step in the operation. I am now coming to another gland. Take the lymphatics in the middle third of the tongue. You know, lying on the side of that, is the sub-lingual salivary gland. I am not concerned with the salivary glands at the present moment. I am speaking now of a gland which lies on the hyoglossus lower down, and it lies close to the posterior edge of the muscle forming the floor of the mouth (vide Fig. 2, *e*). The floor of the mouth is formed by the mylohyoid, which is sometimes called the diaphragm of the mouth.

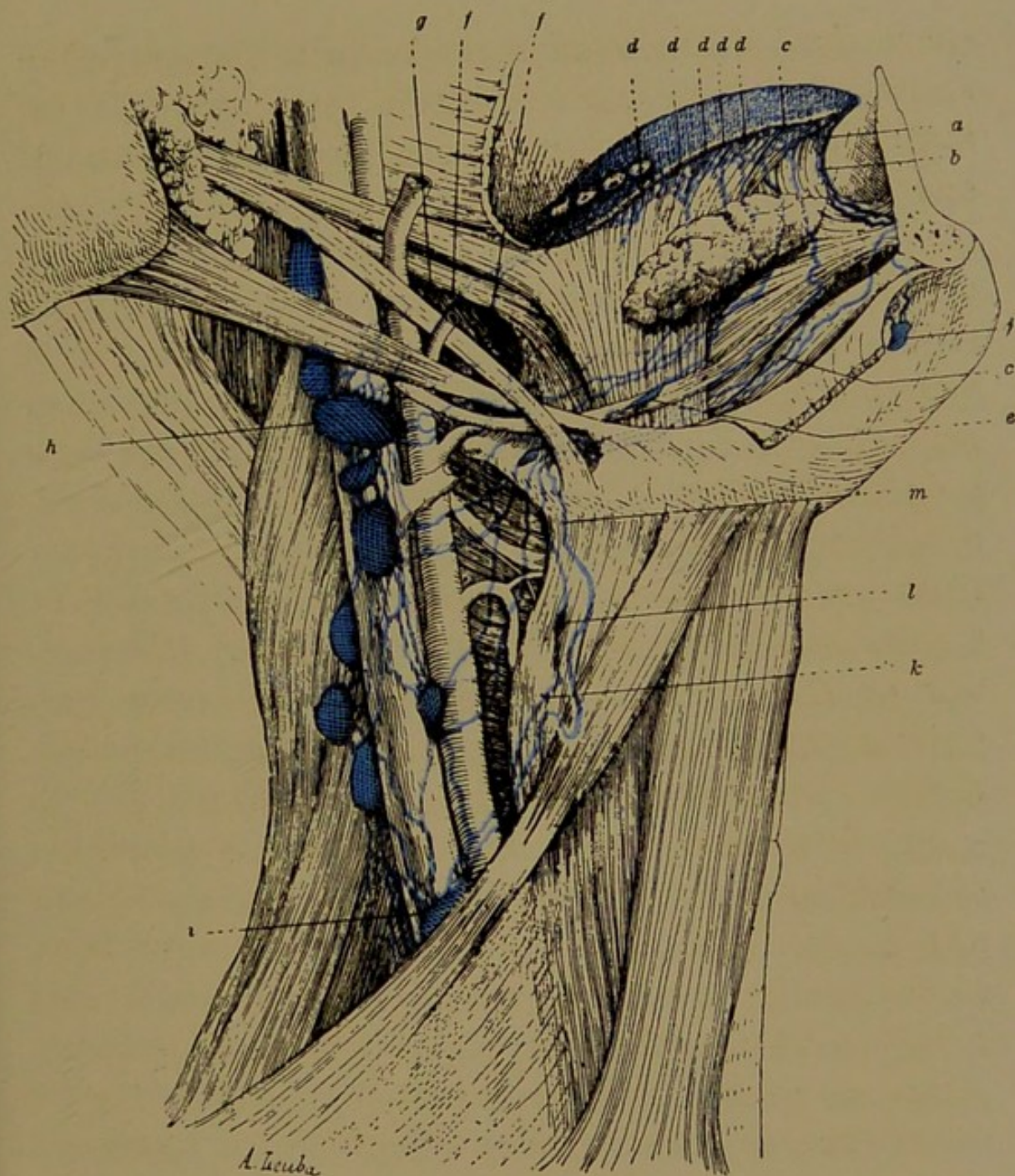
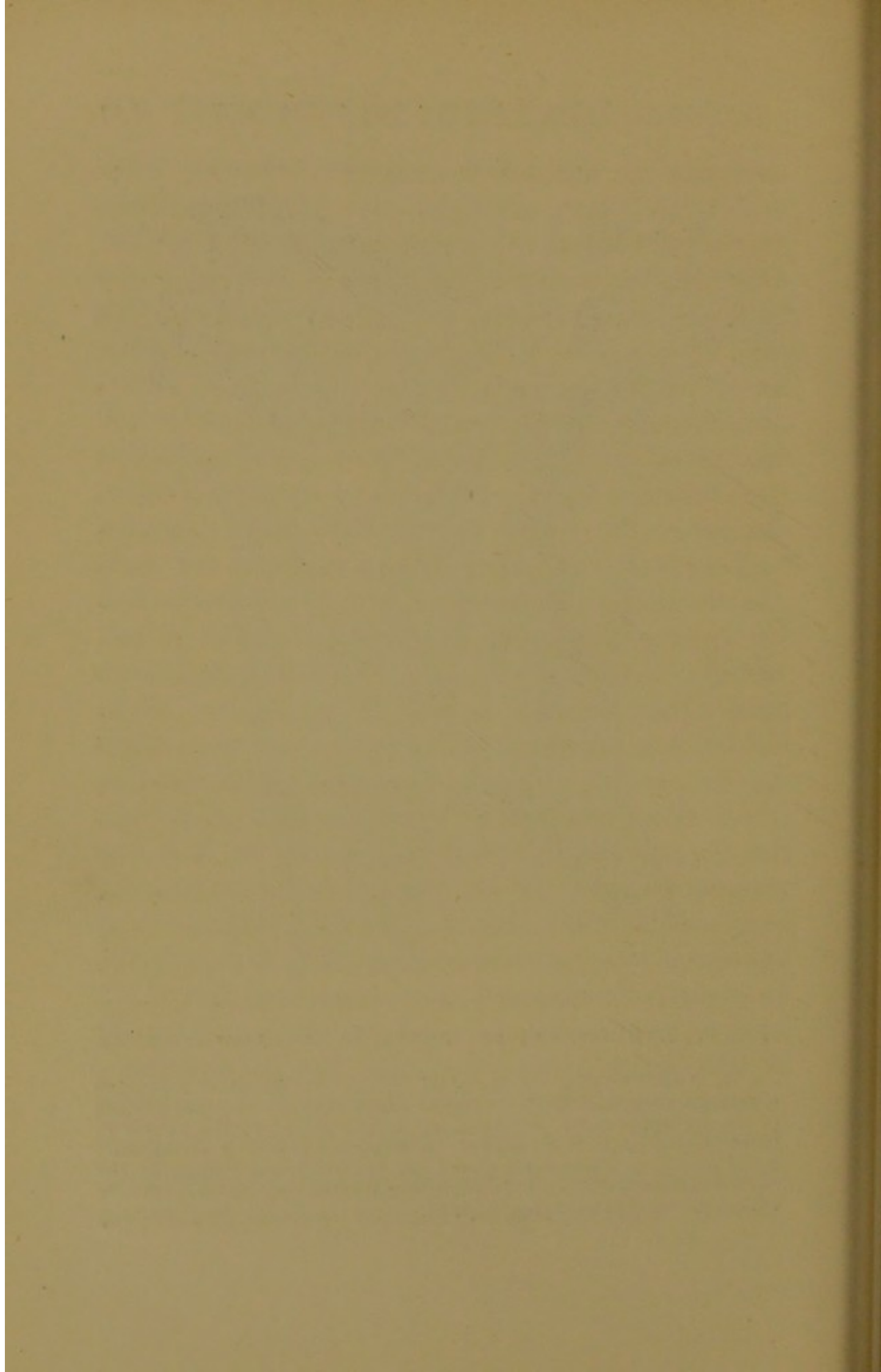


FIG. 2. LYMPHATICS OF THE TONGUE, LATERAL VIEW (POIRIER)

a, b. Apical trunks. *c.* Marginal trunks. *d.* Marginal collecting trunks running with the hypoglossal nerve over the external surface of the hypoglossus. *e.* Interrupting nodule placed in the course of these vessels. *f, g.* Basal trunks. *h.* Principal gland. *i.* Supra-omo-hyoid gland. *j.* Submental gland. *k.* Central trunk ending in the supra-omo-hyoid gland. *l.* Interrupting nodule. *m.* Marginal trunk, ending in the supra-omo-hyoid gland.



And this little gland lies close to the edge of the mylohyoid. Over and over again I have seen these glands overlooked in epithelioma of the tongue, they are overlooked because people are not aware of the extreme difficulty of feeling lymphatic glands. As a rule, lymphatic glands, when they are enlarged, are easier to see than they are to feel. A patient was brought to Kenton Ward the other day who had recurrent epithelioma of the floor of the mouth and enlarged lymphatic glands on both sides of the neck. Many could not feel the lymphatic glands which were situated in the left side of the neck, but they could see them when the man was placed in a proper light ; they could be seen as little projections beneath the skin of his neck. This little gland on the hyoglossus ought to be felt for as a routine proceeding in the case of an affection of the tongue, and I have often shown you how to feel for it. The patient is sat up on a chair, and one finger is passed inside the mouth, close to the tongue, and the other is put outside the mouth at the sub-maxillary triangle. The fingers are then squeezed together, and then you will probably feel the enlarged gland slip away between the fingers. That manœuvre ought never to be overlooked in an examination of the glands in connexion with epithelioma of the tongue. If you merely put your finger outside you will never feel anything, because you push any enlarged gland which there may be up into the mouth ; you will not feel it

against your finger. You must have something to push it against.

I want to draw attention to another thing in connexion with secondary infection from malignant disease of the tongue. In Poirier's book, which is really a most admirable work, there is an explanation of a fact which I was aware of, and had observed clinically, and have had borne in upon me in operating for removal of tumours of malignant character in the neck. In one part of this book there is a picture of the lymphatic glands of the neck. I cannot show you specimens because there are none in the Museum, but if some individual here will produce some proper specimens of lymphatics for the Museum they will be invaluable to him and to those who come afterwards. In this picture there is shown a lymphatic channel arising from the tongue and running down over the hyoid bone, and passing down over the thyroid cartilage¹ (vide Fig. 3, *g*, and Fig. 2, *l* and *k*). It is depicted making a loop, which lymphatics sometimes do, and emptying itself in a lymphatic gland opposite the cricoid cartilage. This gland is usually situated upon the carotid artery opposite the cricoid cartilage, and at the place where the omohyoid crosses the common carotid artery. See how easily you might have been

¹ As this loop crosses the ala of the thyroid cartilage it sometimes has a small lymphatic gland in the midst of its course. In cancer I have seen this enlarged, and easily felt it against the ala of the thyroid cartilage.

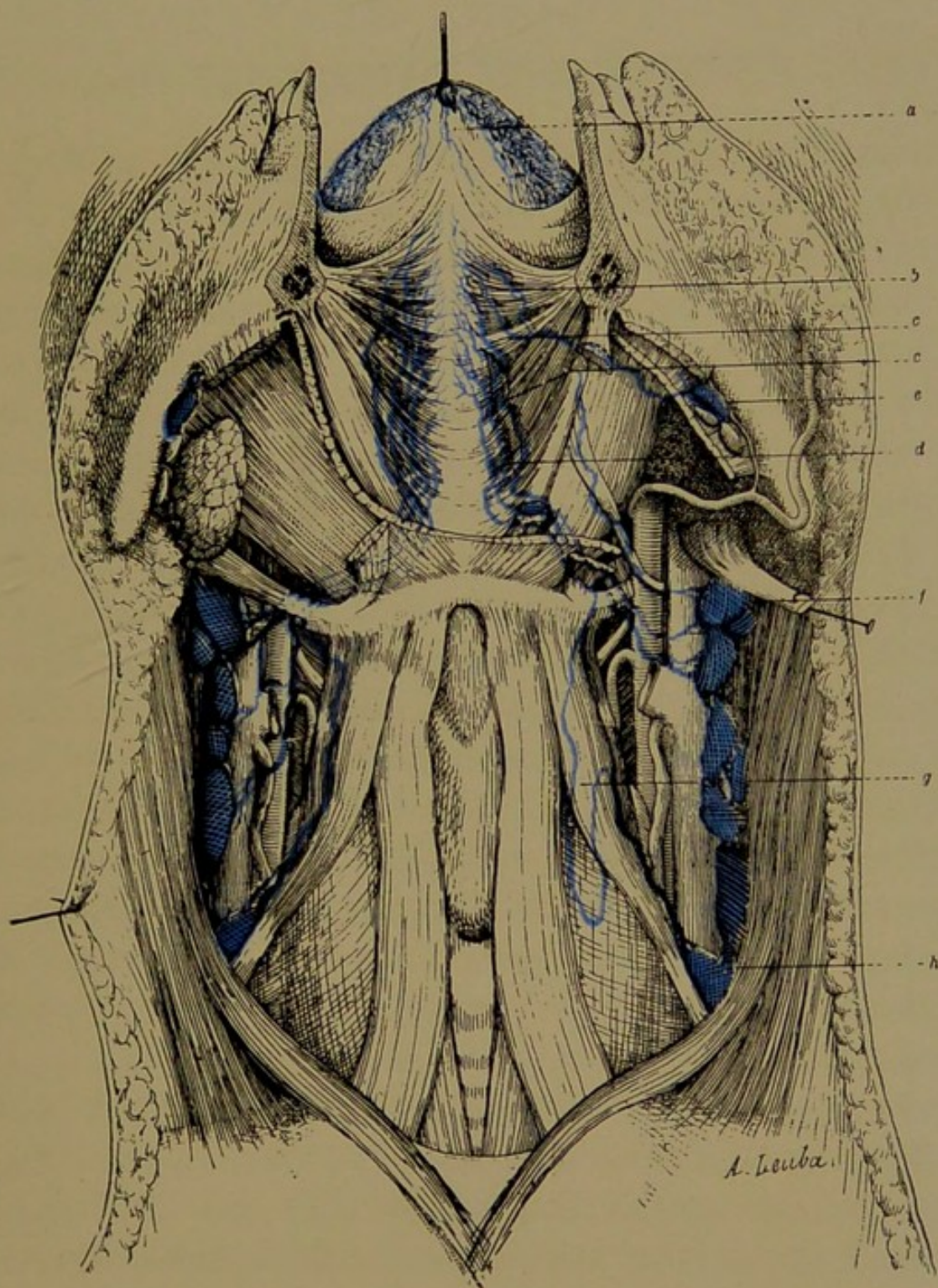
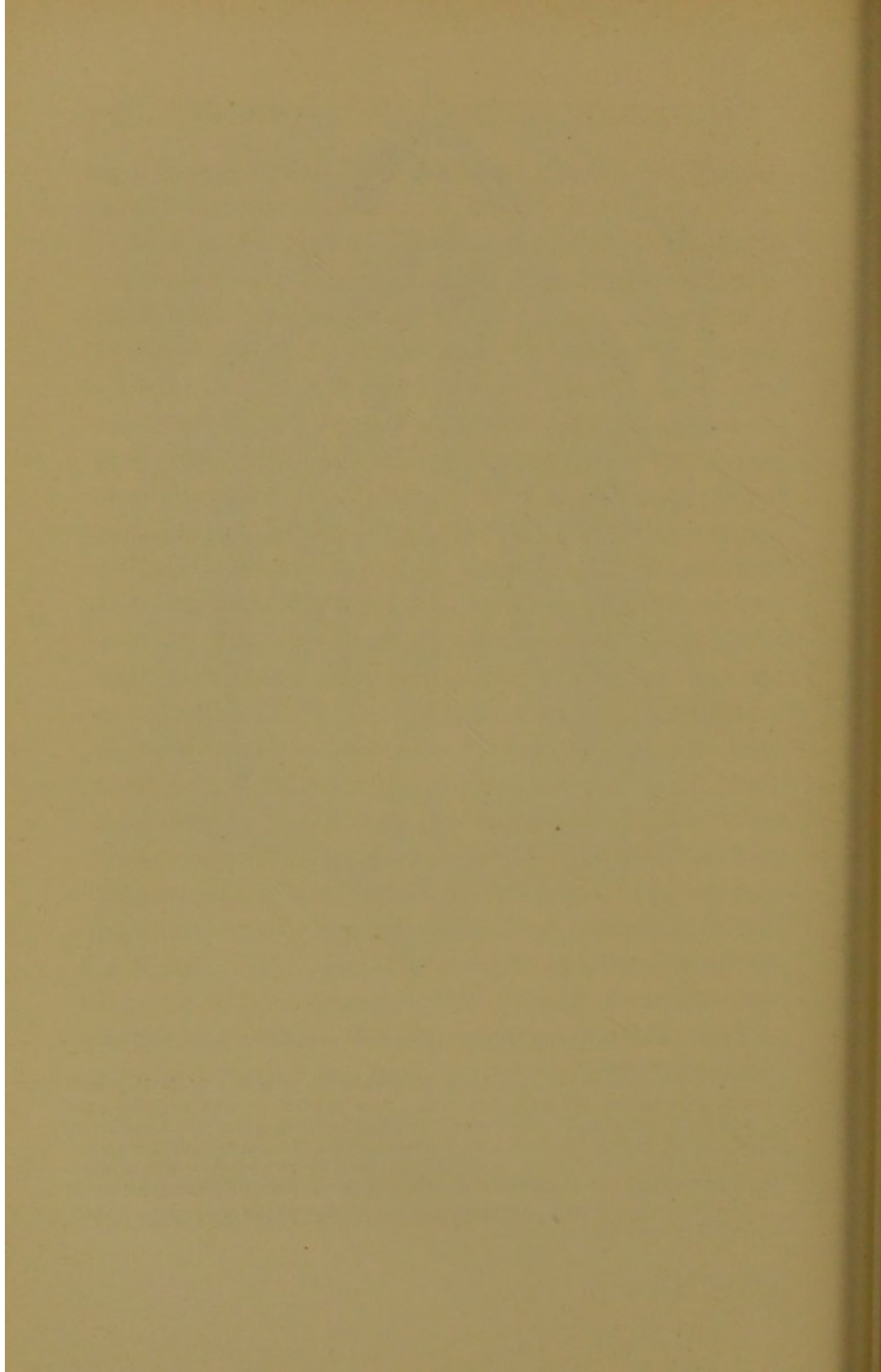


FIG. 3. LYMPHATICS OF THE TONGUE, ANTERIOR VIEW (POIRIER)

The inferior maxilla has been sawn through in the middle line, and the two halves drawn aside. The two central trunks may be seen running between the two genio-hyoglossi which have been removed. *a.* Apical trunk. *b.* Inconstant trunk ending in a submaxillary gland. *c, c.* Central trunks. *d.* Intralingual glandular nodule. *e.* Submaxillary gland. *f.* Principal gland. *g.* Central trunk forming a loop in the supra-hyoid region before ending in the supra-omo-hyoid gland. *h.* Supra-omo-hyoid gland.



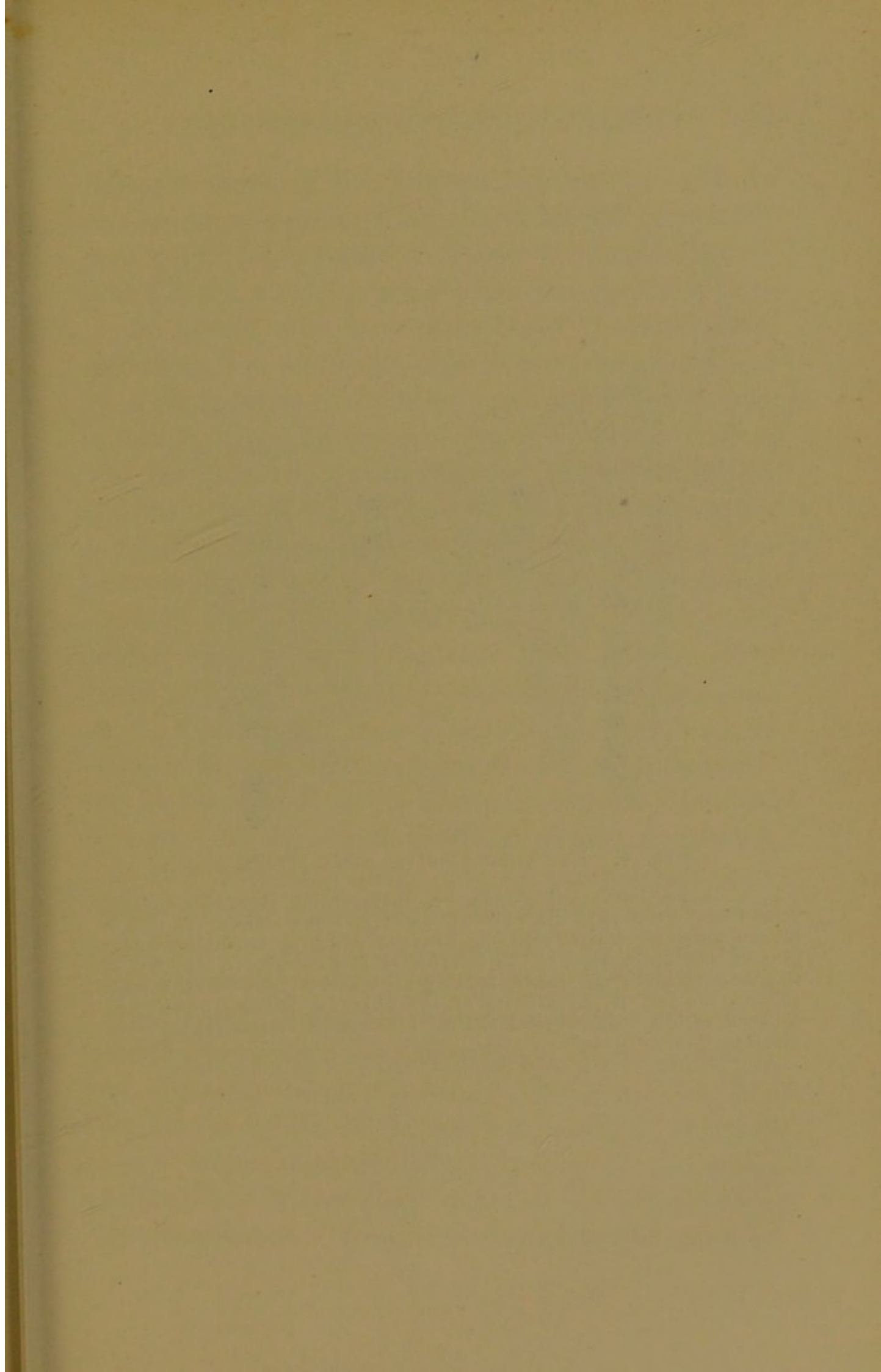
deceived. The lymph from the side of the tongue and floor of the mouth is passed down until it gets opposite the cricoid cartilage, or may enter a gland still lower down. Think of the gentleman who says to a patient, 'Oh yes, you have a small growth on the side of your tongue; it is quite small, and I will come and take it away for you.' And then the man will say, 'Will that cure me?' 'Oh yes, you will be quite cured: you may rely upon that.' Then think of the man coming at the end of six weeks and saying, 'Doctor, will you tell me what this lump is down near my collar-bone?' He had had it all along but it had been overlooked. Your position in the matter ought to be different from that; you ought surely to have looked for that gland. I am now coming to another point. The lymphatics of the tongue empty themselves into this sub-mental gland which you are very apt to overlook. They also empty themselves into a gland at the side of the tongue, and they sometimes skip the lymphatic glands at the sub-maxillary triangle altogether, and pass down and empty themselves far down the neck, opposite the cricoid cartilage. I shall now point out to you some other glands which epithelioma may infect, which are most deceptive, and, if overlooked, entirely vitiate the operation. I do not know whether you have seen them, for there is no specimen in the Museum. But it has gradually dawned upon one's intelligence that there are a number of lymphatic glands on

the deep surface of the sub-maxillary salivary gland. I took the trouble to look at some of the books on anatomy which I possess. It would be unjust to say that these glands (vide Fig. 3, e) are never mentioned by the anatomists, but they are merely mentioned in a cursory way, and possibly in small print at the bottom of the page; the anatomist is not a surgeon, and it does not occur to his mind in the same way at all; he does not think of removing an epithelioma of the tongue; but if he were to do so and overlook malignant disease in the lymphatics hidden away in the sub-maxillary gland he would have done his patient very ill service and not any good. These particular lymphatics to any one who is going to operate are of the utmost importance. I have seen one of these lymphatic glands firmly attached to the under surface of the sub-maxillary gland, and perhaps one or two in its substance. Anatomists may not have seen them; I have seen them because I have seen them enlarged in malignant disease. If the anatomist has injected for them and investigated the matter, he will be aware of their existence. If you are not aware of their existence, somebody will suffer. Supposing an operation is begun for the removal of malignant disease in glands in the neck, you will observe you are going a long way; you have to remove the sub-mental gland, you have to look for this gland on the side of the tongue, you have to remove the sub-maxillary gland, and you cannot tell whether

the gland in its substance is enlarged unless you remove it. Then you have to go down the neck to the level of the cricoid cartilage to see if there are glands in the carotid sheath. Remember, we are planning out our incision for removing the glands in the neck on definite scientific lines. The incision must go forward to the mental process, otherwise you will overlook the mental gland; it must go down to the level of the cricoid cartilage, otherwise you will overlook the gland at the level of the cricoid cartilage; it must go back as far as the sterno-mastoid, otherwise you cannot remove the next glands which we come to, the *glandulæ concatenatæ* (vide Figs. 2 and 3). Concatenated means chained. And there is a chain of glands which extends from the base of the skull towards the root of the neck, along the carotid sheath; they are all round the internal jugular vein and the common and internal carotid arteries. When the glands are enlarged at the base of the skull it is a desperate business, and it is practically useless to endeavour to remove them, in my experience. It may be right to try, but I think whoever tries will be foiled in his endeavours.

Do you remember I said that the old lady who had an epithelioma rather far back on her tongue, close to the pillar of the fauces, had the recurrence situated on the opposite side of the neck? Clinically no return could be discovered on the side on which the original epithelioma had been, but when she

returned there was no question about a considerable mass of glands in the opposite triangle, underneath the opposite angle of the jaw, around the internal carotid artery. How do those glands get infected? These authors to whom I have referred have reproduced a very excellent picture (vide Fig. 1) from one of the finest works on anatomy ever published, and that is a book which you will find in the library, and which was written by the celebrated Sappey. If you want to become a very original anatomist read Cruveilhier and Sappey, and every one will think you are a most original person. This picture of Sappey's, who injected the lymphatics of the tongue with mercury, throws a great deal of light on the matter. I will make a sketch showing how the lymphatics of the tongue go back. The lymphatics of the side of the tongue, as I have repeatedly said, pass down the side into the sub-mental, sub-maxillary, and the *glandulæ concatenatæ*. Here on the dorsum of the tongue are many lymphatics which emerge in front of the epiglottis, and then pass off through the wall of the pharynx in relation with that lymphatic structure, the tonsil. Some are depicted passing across the middle line of the tongue, to reach the opposite side of the pharynx. That is the course which the epitheliomatous infection took in the case of the old lady. Thus you see that the glands on the opposite side of the neck can be infected almost as easily as those on the same side. When once the lymphatics of the



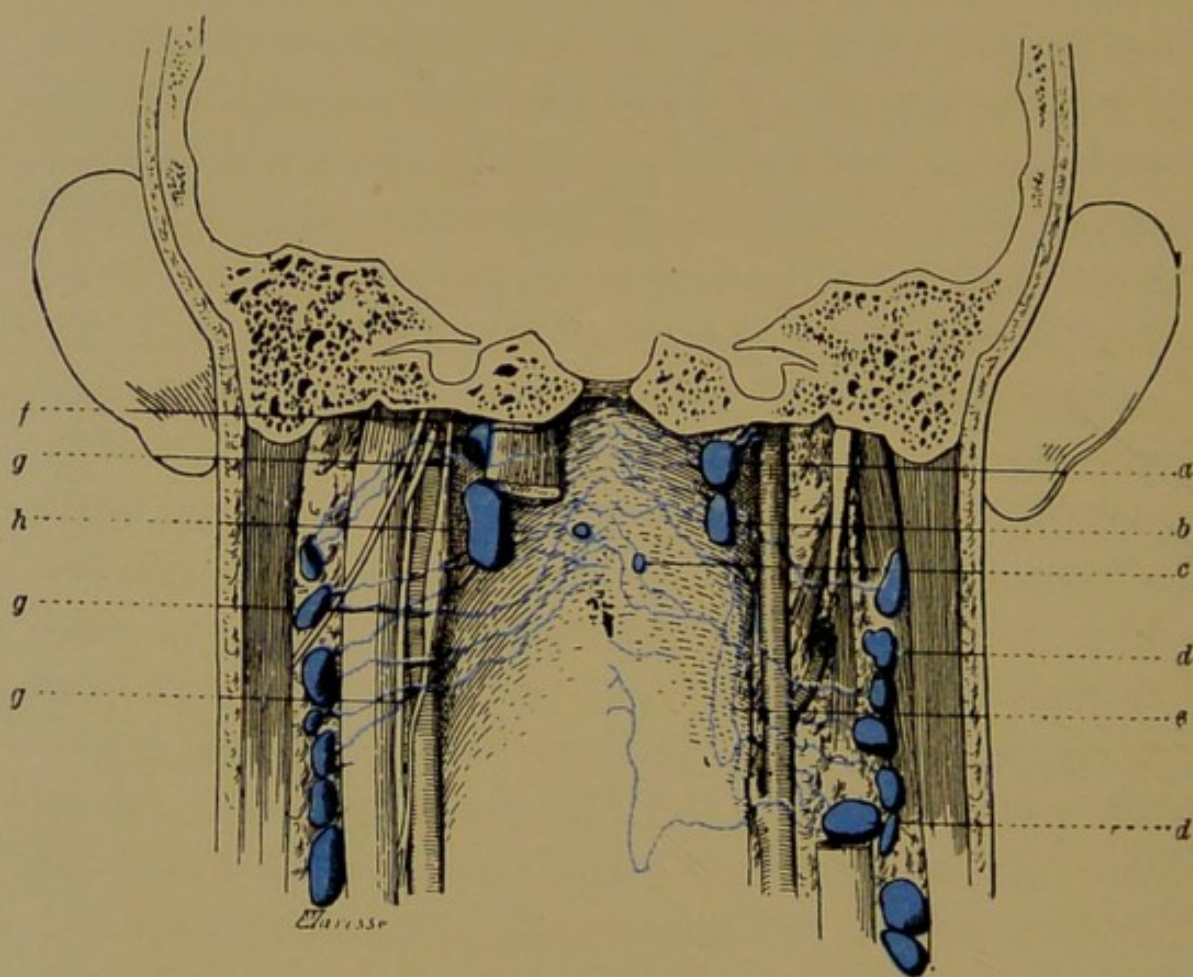


FIG. 4. LYMPHATICS OF PHARYNX (POIRIER)

a, b. Retro-pharyngeal glands. *c.* Interrupting glandular nodule, placed in the course of the afferent vessels of these glands. *d.* Gland of the deep cervical chain. *e.* Efferent vessel of retro-pharyngeal glands, passing in front of the internal carotid artery. *f.* Afferent of the retro-pharyngeal glands, passing behind the right rectus capitis anticus major. *g.* Lymphatic of the pharynx, passing directly to a gland of the deep cervical chain. *h.* Afferent of retro-pharyngeal glands.

posterior part of the tongue become the seat of cancer there is another way in which the lymphatic glands of the opposite side can become infected; and I want to draw your attention to these lymphatic glands, and for you to begin to study them, otherwise you will some day be very much puzzled by a case which ought to be perfectly clear to your minds. Here is a picture of the posterior wall of the pharynx showing the base of the skull and the lymphatics there, and you can see the lymphatics emerging from the naso-pharynx close to the pharyngeal spine (vide Fig. 4). Here are lymphatics crossing the middle of the neck from side to side behind the pharynx. So that there is free intercommunication from side to side behind the pharynx itself. One more word, and that is to call your attention to those lymphatic glands which lie between the posterior wall of the pharynx and the vertebral column. In this book there are depicted on each side two large ones and two or three small ones. Those are the retro-pharyngeal glands, and I commend the retro-pharyngeal glands to your attention just as earnestly as I have commended the others. Those retro-pharyngeal glands get infected with tubercle, or in any infective disease of the pharynx, naso-pharynx, or of the tonsil. You know, as has been shown by Dr. Walsham, how common tubercle of the tonsils and lymphatic tissue of the naso-pharynx is. It is a very common place for tuberculous infection. What does tubercle do to these

glands when it gets there? The first thing that the bacillus does is what it does in any part of the body—at first inflames it; the gland is palpable and slightly tender. After a while this inflammatory tissue grows and undergoes degeneration, and becomes converted into a substance which is compared to cheese, and called caseous material. The caseous material may remain quiescent for some time, or may calcify. But more commonly it softens and becomes converted into tuberculous pus.

I will cease now, but remember, please, that epithelioma is an inevitably fatal disease unless dealt with early; it is certain to pass along the lymphatics; it will in all probability infect the nearest lymphatic gland first, but you cannot reckon on its doing so. You do not, as a matter of fact, know which the nearest lymphatics are, and therefore you must make up your mind that it will be erratic in its progress. And remember that in epithelioma you have to pay attention to the little gland on the hyoglossus, the sub-mental gland, the sub-maxillary gland, the cricoid gland, down near the cricoid cartilage; to pay great attention to the gland on the opposite side of the neck, and to the retro-pharyngeal glands.

VII

CARCINOMA OF THE BREAST, AND ITS SPREAD INTO THE LYMPHATICS¹

IN spite of all the good work that has been done by anatomists, pathologists, and surgeons, I have, during the course of many operations for cancer of the breast, met with much that has not been explained. It is to be feared that we may not hope for clear light until the etiology of cancer is known. But, in the meantime, each addition to our knowledge of the manner in which cancer of the breast spreads adds to the efficiency of operations done for its extirpation.

Without this knowledge it is difficult to see how a complete removal can be accomplished. The spread of cancer along the lymphatics is, however, an erratic and complicated process, and I propose to tell you of the things which I have noticed. I think you will agree that some of them ought not to be dismissed without at least a passing thought.

The rapidity with which cancer spreads into the lymphatics depends to a large extent upon the position of the primary growth. When that is situated within a hollow muscular organ such as

¹ An Address delivered before the Reading Pathological Society, 1905. *J. B. M. S.*, Jan. 27, 1906, p. 181.

the urinary bladder, or gall bladder, the pause may be a long one—months or even years. But when the growth is in the mammary gland, which has no capsule, or the tongue, or in the pharynx, there is, I believe, hardly any interval of time between the onset of the growth and its spread into the neighbouring lymphatics.

For many years I have noted down the condition of the lymphatics whenever operating in malignant disease, and the above is not, I venture to think, an over-statement of the case. Some of the operations, too, have been done at unusually early stages, because, as I have said elsewhere,¹ it is my habit to make an immediate microscopical examination of the suspicious tumours. In this way many are, of course, met with at their very earliest appearance, and even then the lymphatics have been involved.

For instance, a female had a small indurated ulcer of the left nipple. The ulcer looked and felt like a small Hunterian sore, but proved on microscopical examination to be cancerous. No enlargement of the axillary lymphatic glands could be seen or felt. After the whole mammary gland and both pectoral muscles had been removed, several enlarged lymphatic glands were found; the largest one was highest up, and adherent to the upper

¹ The Early Microscopical Diagnosis of Tumours. By C. B. Lockwood, *British Medical Journal*, July 2, 1904, p. 5. Also Lecture XIII, p. 260.

part of the axillary vein. It would not have been seen had the pectorals not been removed.¹ To those who have not realized how soon and how extensively the lymphatics become involved even in the earliest stages of malignant disease, the removal of the whole mammary gland, both of the pectoral muscles, and all the axillary fat and fascia, may seem to have been an extensive procedure to adopt for a small carcinomatous ulcer of the nipple; and yet the end justified the means.

Here is another instance in which a very minute cancer was proved to be accompanied by rather extensive growth in the neighbouring lymphatics. A woman came complaining of slight occasional mucous discharge from the right nipple, which was slightly retracted. The whole of the right breast was larger than the left, but nothing unusual could be felt in it, except at the lower part of the areola. Here a very small soft nodule could with difficulty be felt. Slight pressure made this insignificant lump almost disappear, with the escape of a little mucus from the nipple. An incision brought to light a dilated duct ampulla, and the microscope there and then showed that it contained a small carcinoma. The little sac which held the growth was not more than three-quarters of an inch long and not much more than a quarter of an inch across

¹ On this point see also Sheild: The Results of Operation in sixty cases of Malignant Disease of the Breast. *Lancet*, March 8, 1902, p. 643.

its widest part. The minute growth projected into this from a small area of its wall.

The whole of the mammary gland and the pectoral fascia were then removed. A subsequent microscopical examination showed that this was necessary, because—as I suspected—the gland contained other commencing duct carcinomata, although none could be seen with the naked eye. The tumour beneath the areola—the only one which was known to exist when the operation was done—seemed too small and too immature to have yet spread into the neighbouring lymphatic glands; besides, it was enclosed within the walls of the ampulla. Nevertheless, the axilla was explored, although no enlargement of its lymphatic glands could be seen or felt. Rather to my surprise, several enlarged ones were met with, and the microscope there and then revealed that they contained duct carcinoma. I removed as much of the axillary fat and lymphatic glands as could be done without taking the pectorals away. But on consideration, I regret now that those muscles were not removed. Without that step it is impossible to reach the lymphatic glands about the uppermost part of the axillary vein. These, as I shall presently describe, may become cancerous as soon as, or sooner than, those lower down. Thus, in the smallest carcinoma of the breast that I have ever seen, the neighbouring lymphatics were already cancerous. They probably had in them deposits which taken together

equalled or exceeded in quantity the parent growth.

I believe that there is an idea abroad that these duct cancers are less malignant than the other kinds, which oftener grow in the breast. But my own clinical and pathological experience leads me to think that that is a dangerous delusion. For instance, a woman had a small duct carcinoma of the right breast. This, together with a part of the mammary gland, was removed by a surgeon, who considered that he was dealing with a favourable form of carcinoma. Two years later a recurrence necessitated the removal of the remainder of the breast, together with a part of two ribs and the subjacent pleura. The large gap was closed by bringing up a flap from the abdomen. The patient bore this severe operation very well indeed, and the pneumothorax must have departed in a very short time. This case had a curious sequel, for three months later a nodule of colloid carcinoma appeared in the left breast, which I removed, and afterwards there was reason to suppose that the right pleura was affected. Or, again, a part only of the right breast was removed in a London hospital for a duct carcinoma. Three years afterwards I had to do a complete operation for an extensive growth.

Further, another woman, upon whom I had done an extensive operation for duct carcinoma of the left breast, had a recurrence eight years afterwards in and about the scar. This has been called a recur-

rence, but in most cases it is more rational to say that the cancer left behind went on growing. Perhaps some think that duct cancer is more favourable, simply because it is removed earlier than other kinds. The sanious discharge from the nipple alarms the most careless, and impels them to seek advice.

Another instance shows not only that the lymphatics are involved at the earliest stages of cancer of the breast, but also that the growths in them may soon grow bigger than the one from which they sprung. A woman had a small, hard, movable nodule beneath the lower margin of the left areola. It was explored, and found to be a carcinoma. The nodule was barely half an inch across, but it sent out small prolongations. Some fullness could be seen in the axilla, and moderately enlarged glands could be felt beneath the great pectoral muscles. But when the breast and pectoral muscles had been removed, a very large mass of cancerous glands was unexpectedly met with.

Such cases as these prove that from the earliest beginnings of cancer of the breast the disease has attacked the lymphatics. Although I have for a long time looked upon a carcinoma as a sort of lymphatic sponge with cancer cells growing in its meshes, yet the early spread into the surrounding lymphatics did not become clear to me until I had begun to make immediate microscopical examinations of even the smallest and most innocent-looking tumours of the breast, and to follow this

up by a similar investigation of the neighbouring lymphatics. I am now aware that in no other way can the earliest stages of cancer of the breast be detected. The ordinary attempts at guessing usually succeed in advanced cases, but in the early ones lead to error, delay, and incomplete operations. An instance, which I have mentioned elsewhere, made a profound impression upon my mind.¹ A young woman, aged twenty-nine, was assumed to have a small chronic mammary tumour. This was removed and the microscope revealed that it was a cancer. Then a fresh operation had to be undertaken and, as had been suspected, cancer was found in the breast near the scar tissue of the original operation. The axillary lymphatics were also enlarged, but contained no visible carcinoma. Such instances cannot be very uncommon. I am acquainted with two exactly similar ones, and in which an immediate microscopical examination would have saved the anxiety and distress of a second operation.

The method is so easy and accurate that I wonder it is not more frequently practised. An anæsthetic, an incision, removal of the small tumour, a frozen section stained with an aniline dye, and within from five to ten minutes a scientific—that is to say, a certain—diagnosis. Should the small tumour happily prove to be innocuous, the end is a limited incision, a small scar ; but should it prove malignant, then a thorough operation can be done forthwith.

¹ Loc. cit. p. 27.

The operator having seen the dread disease with scientific certainty, is nerved for a long and arduous toil, for it is requisite to take away the entire mammary gland, the pectoral muscles and their sheaths, the axillary fat and lymphatics, and the axillary fascia.

Having discussed the time, let us now turn to the manner in which the lymphatics become involved in cancer of the breast. The presence of growths in the lymphatic vessels themselves cannot be seen during operations, except occasionally, in very advanced cases. More than once in neglected ones, and upon which I have been reluctantly compelled to operate, I have seen small, hard, white, knotted strings running through the intercostal spaces towards the anterior mediastinum. But, ordinarily, the lymphatics are removed along with the muscles, fasciæ, and fat, and without any microscopic evidence of cancer within their channels. Afterwards microscopic evidence of its presence may be, and often has been, obtained.

We turn, therefore, to the effects of the carcinoma upon the glands along the course of the lymphatics. Probably the first and most obvious effect produced is enlargement. Now, there is to my mind something very strange about this enlargement. I have over and over again chosen the largest from a group of enlarged axillary glands from a case of cancer of the breast, and have failed to identify cancerous cells or structures within its capsule, and this is quite an ordinary experience. It has been recorded

by Stiles¹ in his admirable essay. My colleague, Dr. F. W. Andrewes, one of the most accurate of pathologists, allows me to say that he quite agrees that in cancer of the breast the axillary glands are sometimes enlarged without showing any microscopic evidence of cancer, and he adds that it is impossible to assert positively that cancer is absent from a given gland without examining serial sections of the entire gland, several thousand sections in number—which is practically impossible. But the ordinary routine methods give, he thinks, a fair presumption of the absence of cancer. Here is some additional evidence upon the same point. The left breast, pectorals, axillary fascia, and axillary contents were removed for carcinoma. The axillary lymphatic glands were sent for examination. Mr. Ernest Shaw, whose histological skill is acknowledged, reported that the large glands did not contain cancer, but that some minute ones did. This, too, Mr. Shaw tells me, is an ordinary experience.

Mr. Bilton Pollard,² during his very successful operations for cancer of the intestine, had a similar experience. But here another thing may have happened. As every one knows, the almost inevitable course of cancer is growth, degeneration, ulceration, and sepsis.

In Mr. Pollard's cases the form of carcinoma was

¹ *The Surgical Anatomy of the Breast and Axillary Lymphatic Glands, &c.* Edinburgh, 1892, p. 24.

² *British Medical Journal*, January 23, 1904, p. 175, *et seq.*

one which is particularly prone to early ulceration, and was situated in the midst of sepsis, which may or may not have caused the enlargement of those lymphatic glands in which no cancerous growth could be seen. But it is obvious that a living organism, such as a cancer is, must conform to physiological rules, and that its products must escape by its veins or by its lymphatics.

Taking into consideration the fact that a cancer is a sort of ductless gland opening freely into the lymphatic system, it is reasonable to fix upon the latter as the outlet. That that which enters the circulation through the lymphatic is very harmful is suggested by the marked accession of health and vigour which often follows the removal of the tumour and of the cancerous glands. To me this cannot be altogether accounted for by the relief of the mental anxiety and foreboding.

The naked eye is an untrustworthy guide, both to the nature of most tumours and to the growth in lymphatic glands. Whilst removing a carcinoma of the breast the lymphatic glands were found enlarged and hard as high as the subclavius tendon, and in the subclavian triangle. From these naked eye appearances it was assumed that they were filled with cancer undergoing degeneration. However, the subsequent microscopical examination showed that they were full of tubercle. This combination of tubercle and cancer has been occasionally met with. What is the meaning of it?

I now come to another question, namely, the order in which the lymphatic glands become cancerous. It is natural for us to assume that cancer follows the course of the lymph stream upon its way from the breast to the nearest group of lymphatic glands. But to turn this generalization to account we must know the direction taken by the lymphatic vessels themselves, and then the direction of the lymph stream within their channels.

Further, we have to assume that the cancerous particles or cancerous lymph, or whatever it may be, passes along the lymphatic channels in the direction of the assumed course of the lymph stream. But my own clinical observations have led me to think that it is quite unsafe to begin with preconceived ideas on these three points, or, in other words, to assume that the cancer will spread in some preconceived direction, and linger in some particular group of lymphatic glands. For instance, our earliest knowledge of the arteries and veins tells us that they anastomose but scantily across the middle line. Hence we easily infer that the lymphatics do the same. But clinical observation soon teaches us the untruth of such an inference. There can be little doubt but that the lymphatics of the breasts intercommunicate across the sternum.

This allows, perhaps, of an explanation of such a case as this: A long-standing carcinoma of the left breast was removed, together with the pectoral muscles, the fasciæ, and all the axillary fat and

lymphatics. When this was done I could discover nothing wrong with the right mammary gland, but nine months afterwards a minute nodule was felt near its sternal edge, exactly opposite the tumour which had been removed. This minute growth was examined microscopically by the immediate method, and was a carcinoma of the same type as that which had been removed from the other breast. Is it right to think that that cancer had crossed over the sternum along the lymphatics from one mammary gland to the other? In this particular instance this, to me, seems the likeliest explanation; but soon another case of bilateral cancer will be described in which this explanation seems less likely, because the bilateral tumours were not of the same histological type.

I suppose that in the above instance the carcinoma may have grown from one breast to the other in the lymphatics which cross the sternum, or have been forced along them by the cutting and handling during the operation, or carried over before the operation was done in some regurgitant stream set up by the obstruction of the opposite axillary lymphatics. But this mysterious disease is full of puzzles, and will be until its etiology is known.

It would not be easy to explain the following instance of bilateral cancer by supposing that it had grown along the lymphatics or been carried as an embolus. I removed the left breast and pectorals and all the axillary lymphatics, together

with a great quantity of others from the posterior triangle of the neck, for the ordinary form of scirrhus carcinoma. Eight months afterwards, the patient was much improved in health, and the left side was quite free from disease, but a small movable nodule had appeared in the right breast just beyond the outer edge of the areola.

The part of this nodule nearest the nipple was chronic mastitis pure and simple; that nearest the axilla was cancer. Although none could be felt, the axilla contained numerous cancerous lymphatic glands. Now the mammary lymphatics are so abundant and intercommunicate so freely, that it is possible that the cancer had passed along them over to the side of the breast furthest from the original growth; but to me it does not seem likely. Then which came first, the carcinoma or the mastitis? Did the carcinoma cause the mastitis or did it grow in a lobule already inflamed? An answer to these questions may be found in a similar combination of chronic mastitis and carcinoma which was met with in another case. Several of the breast lobules were enlarged and indurated. A nodule of cancer was found in one only. Obviously a constant cause ought to produce a constant effect, and cancer should have been found in all of the inflamed lobules. Probably the chronic inflammation only prepared the ground for the carcinoma in the breast, as it does sometimes in the tongue and elsewhere. Those who attempted to make a correct clinical diagnosis

in this case of mastitis and carcinoma were much at variance.

Here is another fact which ought to make us cautious in thinking that the onset of cancer in the other breast is always due to lymphatic transmission, though it may be. I have told you about a case in which a portion of the chest wall was removed for recurrent duct carcinoma of the right breast. Within four months a carcinoma had grown in the left breast, but it was a spheroidal-celled carcinoma undergoing colloid change. Here it is difficult to suppose that a particle of the duct carcinoma had been carried across. But then, again, it may be quite wrong to look for a particulate cause for the various histological kinds of cancer. And here for a moment I may perhaps be allowed to digress, and say that it is rash to infer that when tumours grow in both mammary glands they will be of the same variety.

For example, I removed from the right breast, a cystic fibro-adenoma. Five years later another small tumour grew in the left breast. Although clinically it was exactly like the previous one, yet it proved to be a spheroidal-celled carcinoma. Again, a cancerous tumour had been removed from the right breast by a partial and futile operation. The tumour speedily grew again, but seemed yet within the possibility of complete removal. But whilst the carcinoma was growing in the remains of the right breast a tumour had appeared in the left.

The outlook seemed as bad as could be, if, as seemed certain, both breasts were cancerous. But an immediate microscopical examination showed that the tumour in the left breast was a cystic adenoma, so after it had been taken away a complete operation was done for the carcinoma of the right side. More than a year afterwards this operation seemed to have been effectual.

What can be the meaning of these incidents? Why in the same person should one breast bring forth an innocent growth, and the other a malignant one? It might save trouble to say that these were mere coincidences, but that would never help us to solve the riddle of this horrible, mysterious, and almost unknown disease.

Thus it is probable that cancer may cross in the lymphatic channels from one breast to the other, but it is evidently rash to assume that it may have done so in any particular instance of bilateral tumours.

I have already mentioned that lymphatics from the inner side of the breast may carry carcinoma into the mediastinal glands. I have only seen this in advanced cases, such as ought not to occur without an effort having been made to extirpate the disease in its earliest and more amenable stages. The admirable work of Mr. Handley¹ has also gone far to prove the growth of carcinoma from the

¹ The Dissemination of Mammary Carcinoma, by W. Sampson Handley, *Lancet*, April 8, 1905, p. 909, *et seq.*

breast along lymphatic channels which join with those of the abdomen mainly through the epigastric angle. Any one who realizes this must of necessity remove the lymphatic-bearing fascia which lies over the juncture of the great pectoral muscle and aponeurosis of the rectus abdominis. Indeed, in my later operations the rectus aponeurosis itself has been removed, bringing into view the fleshy origins of the muscle from the costal cartilages. This entails but a trifling addition to the extent of the operation, and would be amply repaid if the risks of intra-abdominal growth be lessened by it. The mammary gland is so widespread that it is not very far from the epigastric angle, and therefore, as every scrap of gland tissue has to be removed, but a slight extension of the incision is necessary.

I now come to the spread of carcinoma of the breast into the axilla. All will agree that this is usually as follows : First, it spreads into the pectoral group of axillary lymphatic glands, and which are so easily felt when enlarged at the lower edge of the pectoralis major. Thence the growth passes onwards into the glands about the uppermost part of the axillary vein, and thence onwards into the subclavian. As the growth progresses, other groups become involved—that about the subscapular artery and vein, that about the lower end of the axillary and upper end of the brachial vessels. But in practice it is unsafe to assume that the lymphatics become cancerous in regular anatomical sequence.

On many occasions after having taken away the pectoral muscles, I have found considerable enlargement of the uppermost axillary glands, although the lowest pectoral ones seemed to have escaped. This anomaly can be explained upon anatomical grounds, for although the main bulk of the lymphatics of the breast enter the pectoral group of axillary glands, nevertheless others run upwards through the fibres of the pectoralis major, or between the sternal and clavicular heads, to empty into the uppermost axillary or subclavian glands. This important accessory lymphatic channel is described and depicted in Poirier, Cunéo, and Delamare's standard work on the lymphatics,¹ which has been well and clearly translated by Mr. Cecil H. Leaf, who has himself worked at this subject. I have found in Professor Poirier's work many anatomical explanations of the course of cancer along the lymphatics, which before I had only known from clinical observation.

I have been especially indebted in the case of the lymphatics of the tongue.² Grossmann³ succeeded in injecting the accessory mammary lym-

¹ *The Lymphatics*, Poirier and Cunéo, translated by Cecil H. Leaf. London, 1903, p. 215, *et seq.* Fig. 85, p. 235.

² Secondary Infection of the Lymphatic Glands in Malignant Disease of the Tongue, by C. V. B. Lockwood, *Clinical Journal*, June, 1904, p. 113, *et seq.* (Lecture VI, p. 103 *ante*).

³ Quoted by Poirier, p. 223, who also says this channel has been seen by Rieffel.

phatic trunk which runs direct into the uppermost axillary or subclavian glands thrice in thirty subjects. I am inclined to think, on clinical grounds, that this may be too small a proportion. Disease may more readily disclose lymphatic channels than any method of injection, even than the excellent one of Gerota. Although after the removal of the pectorals it is rather the exception than the rule to find the uppermost axillary glands enlarged without any enlargement of the pectoral ones; nevertheless, it is by no means unusual to find them by far the biggest. This may, of course, be capable of other explanations, but one cannot help thinking that they must have got an early and independent supply of cancer *short-circuited* to them by a separate channel.

I may say that enlarged glands are not commonly met with superficial to the costo-coracoid membrane. Once, however, I met with a large mass lying upon it, and on several other occasions I had encountered two or three.

Enlarged cancerous glands may be met with in rather unexpected places. For instance, in a very advanced case one was found outside the coracoid process beneath the interval between the pectoralis major and deltoid. This gland was situated upon one of the trunks which collect lymph from the arm. According to Poirier¹ it was first described by Aubry, who says one, two, or three may be

¹ Loc. cit., p. 234.

found, but not constantly (fourteen per cent. of cases according to Grossmann). It is rather difficult to guess how any of these become involved in cancer of the breast. Perhaps regurgitation takes place, or, what is much more likely, there are loops of communication between these delto-pectoral lymphatics and those of the axilla.

It is hardly necessary to dwell upon the bearing which the above has upon clinical diagnosis and surgical treatment. Obviously, when the mammary gland contains a carcinoma—a fact which can easily be learnt by an immediate microscopical examination of the tumour—it is quite unsafe to infer that, because the lowest axillary glands are not enlarged, therefore the uppermost are not. In the beginning the enlargement of the uppermost axillary glands cannot be detected except by taking away the pectorals. In the later stages it usually causes a slight fullness of the infraclavicular fossa, but this can only be seen in those who are thin. It is not a sign to be waited for. Fortunately the removal of the pectorals is borne exceedingly well, and is followed by slight impairment of the usefulness of the arm.

Returning again to the axillary lymphatic glands, I propose now to speak of an almost outlying group which on more than one occasion has nearly been overlooked, although enlarged by cancerous growth. This small group is situated about the end of the axillary and beginning of the brachial vessels, and

belongs, therefore, to the arm rather than the axilla. As a rule, the outer end of the incision made for removing the breast, pectoral muscles, and axillary vessels falls short of these brachial glands, so that, unless the incision be especially carried down the arm for a little way, they will not be brought into view.

Nor can we reckon upon the presence of a continuous chain of enlarged glands along the axillary vessels to point the way to them. Deceptive intervals may intervene between groups of cancerous glands. There is a danger, too, that enlargement of the glands might not have been suspected in a place so remote from the breast tumour. Besides, the casual discovery of cancerous glands by the fallacious sense of touch, is not to be implicitly relied upon. Thus, more than once, towards the end of a long operation, when, as a last precaution, the incision has been prolonged down the arm, and the enlarged brachial glands have come into view, I have felt a qualm at the thought lest they might have been overlooked. That would, in all probability, have rendered the whole proceeding futile. Such an incident has occurred but recently. The case was one of Paget's disease. The lymphatic glands were extensively involved, some being adherent to the axillary vein close to the subclavius tendon. The lowest lay over the brachial artery, and these were the ones which were almost overlooked. In another instance it is noted that the

brachial glands could not be felt, although greatly enlarged, because the patient was so fat.

It is to be supposed that these brachial glands became cancerous by a process of growth from the axillary and pectoral ones. This would imply that the growth had taken place against the lymph stream, and past a number of lymphatic valves.

But there is another possibility. In Poirier (p. 238) we find it said :

‘It is not rare to see a vessel, no matter from which of the lymphatic territories it may spring, end in a glandular group other than that which represents the usual terminus of the collectors of the territory in question ; thus Nagel has seen one of the trunks coming from the subareolar plexus send a branch to a humeral gland before terminating in the supero-internal thoracic glands.’

Presumably this is not a very common occurrence, but neither is marked enlargement of the humeral, or brachial glands as they are better called.

There is yet another group of axillary glands situated over the subscapular vessels. But these are not at all likely to escape notice, and are removed as an invariable part of a modern operation. I have not seen the subscapular glands separately enlarged. But the growth of cancer in them may add to the difficulties of the operation, and render it less efficacious.

I have already spoken of a case in which a cystic adenoma was removed from the left breast and

carcinoma from the right breast and axilla (p. 136). A futile attempt had been made to take away the carcinoma. In the axilla the disease was far spread, and the axillary vein had to be taken away with the adherent glands. But the most difficult and embarrassing step in the operation, and attended with much serious hæmorrhage, was the removal of the adherent subscapular glands, and of cancer which had grown far backwards in the spaces above and below the triceps tendon. Through the upper quadrilateral space lymphatics from the deltoid and dorsal scapular muscles accompany the posterior circumflex vessels.

Here again is an instance of carcinoma spreading against the lymph stream. After the lapse of a year this operation seemed to have been successful; but, should the growth reappear near the neck of the scapula, nothing short of Berger's interscapulo-thoracic amputation would suffice. Truly that would be a heavy penalty to pay for a thoroughly inefficient operation, which the first one was.¹

After a time cancer of the breast spreads into the glands of the neck—usually a late step in the disease. Doubtless the growth nearly always spreads beneath the clavicle, from the uppermost axillary glands and lymphatics into those in the subclavian triangle. But there is a possibility that it may find a shorter way through lymphatics which cross in front of

¹ This has since been carried out, and was followed by a rapid recovery.

the clavicle along with that morphologically interesting vein, the jugulo-cephalic. According to Poirier, lymphatics have been seen by Grossmann passing over the clavicle from the uppermost axillary glands into the subclavian. Mascagni, too, has seen some of the lymphatics which ascend with the cephalic vein (delto-humeral), cross the clavicle and enter the cervical glands.¹

When we consider that some of the mammary lymphatics run straight upwards towards the clavicle; and, further, when we consider the abundant intercommunications between contiguous lymphatics, it is not difficult to believe that occasionally some of the mammary lymphatics may short-circuit themselves into the cervical by crossing over the clavicle. Clearly the subclavian lymphatic glands demand careful attention in every case of mammary carcinoma, whether the axillary are or are not enlarged. Indeed, some surgeons carry their distrust so far as to remove the subclavian glands in every case. I myself have not yet reached this stage. I content myself with a thorough dissection carried above and below the axillary and subclavian veins as far as can be reached by raising the clavicle upwards.

In most cases this dissection brings to light a gland which was hidden from sight underneath the great vein just where the subclavius muscle crosses it. Enlarged glands are occasionally found

¹ Loc. cit., pp. 240, 245.

above the vein. Once I found several above and none underneath.

The sense of touch is a fallacious guide to the presence of enlarged glands in the subclavian triangle. Thrice I have explored it to seek for an enlarged gland, which I, and others, too, thought could be felt, and thrice have failed to find any. It is almost certain that the deception was caused by the posterior belly of the omo-hyoid in a state of rigid contraction. Mr. Sheild¹ says that the transverse processes of the lowest cervical vertebra have also led to error. One of my patients is well ten years after exploration, so that it is unlikely that an enlarged gland had been overlooked.

This brings us to another point—what ought to be done when carcinoma has spread to the cervical glands? I believe that opinions vary upon this point. Some take a pessimistic view, consider the patient doomed, and refrain from operating; others do not shrink from very extensive dissections of the neck.

But, after all, the dissection of the posterior triangle of the neck is not a very formidable undertaking for either the patient or the surgeon. It is true that it demands, on the part of the latter, skilled experience and thorough anatomical knowledge; but, granted these, the difficulties are by no means serious. After a flap has been turned off the triangle—for small cuts are useless and danger-

¹ Loc. cit., p. 353.

ous—a very searching dissection can be made. Next, the patients seem to suffer little from extensive dissections of either the anterior or of the posterior triangles of the neck. Moreover, the process of repair is rapid, and attended with little pain or constitutional disturbance. Indeed, I cannot help thinking that the difficulties and dangers of this step have been overrated. Even were this untrue, 'the greater the evil, the more vigorous the remedy.' Besides, cancer is a disease which varies greatly in its rate of progress. In the thin and abstemious it is often slow, and these are the ones in whom lymphatic enlargement is easiest to detect and easiest to remove.

For example, the right breast, pectoral muscles, the fasciæ, the axillary contents, and subclavian lymphatic glands were removed from a spare woman, aged forty-nine. At its commencement the small cancerous nodule had been seen by a surgeon. An immediate examination would have revealed its nature, and it would not have been allowed to spread into the cervical glands. Three years after the extensive removal this patient was quite well, and apparently free from disease. Another was not in some respects quite so fortunate. She had an ulcerated carcinoma of the left breast, and enlargement of the axillary and cervical lymphatic glands. A gland near the top of the posterior triangle of the neck was removed for immediate microscopical examination, and was seen to be full of

carcinoma. Being so far from the breast, we thought it might have been tuberculous. The patient was young and vigorous, and anxious that an attempt should be made to relieve her from the pain and misery of an ulcerating carcinoma, and, one might add, from the imminent risk of dying of sepsis. During the dissection of the neck but few glands were met with along the internal jugular vein, nor about its junction with the subclavian. In other words, the state of affairs was not so bad as might have been expected. Eight months afterwards this unfortunate woman came with a small, movable nodule in the right mammary gland.

This I have already mentioned, for it consisted of chronic mastitis and of carcinoma (p. 135). The woman's health being good, and all the left axilla and neck free from any trace of disease, an amputation of the right breast was done. Six months afterwards her condition was quite satisfactory.¹ But in yet another case the result was less propitious. At the age of thirty-four the right breast had been amputated for carcinoma. Then the disease grew in the lymphatic glands of the axilla and posterior triangle. In the infraclavicular fossa the mass of glands was immovably fixed. I might have been deterred by this had I not succeeded in removing a similar growth, and which had not returned after the lapse of three years. It took two hours

¹ She has since had an extensive outburst of her disease upon the chest wall.

and forty minutes to clear the axilla and posterior triangle. The subclavian veins and artery had to be removed to get away the fixed mass.

The operation was well borne, but the patient died three months afterwards without any local recurrence, but with evidences of growth in the osseous system. Dr. Casey informs me that in the house in which this patient lived her grandmother had died of abdominal cancer, and her grandfather of cancer of the lip.

Sometimes, too, these immovable glands are not so fixed as they seem to be. A woman, who nine months before had had the left breast amputated for carcinoma, had a large gland apparently adherent to the head of the left clavicle, behind the attachment of the sterno-cleido-mastoid muscle. When that muscle had been reflected the gland was easily removed. She lived for more than three years, but at last died of cancer. The subclavian lymphatic glands were not enlarged, so that it is not easy to tell how the growth had got into that solitary gland almost in the upper aperture of the thorax: probably by some unrecognized short-circuit. Finally, I doubt whether any rules can be laid down for these operations in cervical cancer. That question has to be weighed and considered and decided for each individual case.

VIII

VARICOSE VEINS

LAST time I lectured we considered the extension of carcinoma to the lymphatic glands around the breast, a rather complicated subject. Nevertheless, it is one which everybody here must know thoroughly. However, I thought that to-day I would give your minds a rest, and lecture upon a simple subject, namely, varicose veins. The internal and external saphenous veins are those which commonly become varicose. But of course, as you are aware, other veins are also affected—for instance, the hæmorrhoidal and spermatic; and I have also seen the veins of the labia enormously dilated in cases in which the iliac veins had become obstructed. But to-day we shall confine our attention to varix of the internal and external saphenous veins. I will tell you why I have chosen this topic. First, because varix is a very common affection; and next, because it is an affection which causes a good deal of anxiety to the patient; and, lastly, it is a very good subject from this point of view: I have always told you that it was not sufficient that you should know that a person has

a disease and that you should be able to name the disease, but I have also told you that it is essential that you should be able to put yourself in the patient's place and have an idea in your mind how the disease is affecting the patient—in other words, have an idea what he feels like, and what trouble the condition is giving to him, how much it is interfering with his happiness and comfort. I have so often asked the students to tell me what effect these varicose veins have upon the patient, and have been unable to elicit an answer, that I suspect I am right to-day in discussing it with you.

First of all, let us take the matter of the diagnosis of varicose veins. It seems so simple that you must all think that no question could arise. But I have myself seen a patient with varix of the internal saphenous vein wearing a femoral truss, and I have afterwards operated and removed the dilated vein. As you are aware, the internal saphenous vein, before its entry into the common femoral vein, has an ampulla or dilatation upon it, and under certain circumstances, sometimes in females who have been pregnant, the vein at that point becomes locally dilated. Here is a swelling in the region of a femoral hernia which swells out when the patient stands up, departs when she lies down, has an impulse when the patient coughs, and, in fact, is exceedingly like a femoral hernia. I do not think that anybody ought to mistake a dilatation of the ampulla of the saphenous vein for a femoral hernia,

provided they would carry out the rule in diagnosis which I have always laid down for you—that is, that you should examine the whole of the diseased limb or structure. I suspect what probably happens in these cases is that the patient appears and says he has a rupture. The word ‘rupture’ excites the mind of the persons who hear the word, they get a sort of obsession, and cannot get the idea of rupture out of their minds. They look at the swelling, and they see it is in the femoral region. They tell the patient to cough, and they see the impulse. But they have omitted to examine the whole of the limb, and they do not observe that lower down there is a varix of the saphenous vein. An additional source of error, perhaps, is that between the dilatation of the ampulla and the vein below there is a part of the limb in which no varix can be seen to exist, perhaps in the upper third of the thigh. So it is very like a femoral hernia. The other day I saw an exceedingly difficult case. A patient came who was known to have varix, and was wearing an elastic stocking. But it was thought that in addition he had a hernia. In the region of femoral hernia, in Scarpa’s triangle, he had what I should say was a most close resemblance to femoral hernia. He had a swelling which appeared when he stood and disappeared when he was lying down, which had a great impulse in it, and which could be pressed back, and which felt to me very like a hernia.

I knew all this time that he had varix in the veins in the lower part of his leg, so I set to work to discriminate. I had no doubt it had been carefully examined, because it was sent to me by a competent man who had a strong suspicion that it was hernia, so I had to discriminate very carefully. These are the things I observed. But first, I want to point this out to you : whenever you see a surgeon or physician examining a patient, watch with the greatest possible care to see what he does. If it is a question of doubt in diagnosis you will often observe that one man solves the doubt when the others could not, and the way in which one man happened to solve it is this : he applied to the diagnosis of the case some method of examination which the others had not applied. So I suspect in this case two things were applied which had not been applied before. First of all, when the patient was lying down the sac emptied. So would a femoral hernia. Pressure was made on the external iliac vein above, and the sac did not fill. So might not a femoral hernia. That could also be retained in that way by the pressure of the finger above. But then it was observed that when he stood up, and then had the pressure removed, the sac gradually filled, and so did the upper part of the varicose saphenous vein, although it had been compressed, so as to prevent it filling from below. That seemed to settle the point. But there was one other test which I applied, and I venture to say there is nobody

in this theatre who can guess what it was—I may be wrong, but I think I am right ; I am sure it had not been applied by the gentleman who had examined the case before I did. I went one step further, and applied a stethoscope to this swelling in the groin. When I did so I heard a very pronounced venous hum, which is not unusual in a big nævus or any large dilatation of the veins. This swelling had a marked venous hum. I have pointed out that the diagnosis of varicose veins is not always quite so easy as you might be led to suppose from the accounts of it which are given you in your textbooks. I have observed another curious thing about varicose veins. You will hardly believe it, but once a patient came to this hospital who had on the inside of his knee a tumour which was by some thought to be a sarcoma. You can easily imagine that there was a very clear and obvious tumour. It felt hard, and had well-defined boundaries. Its surface had irregular projections upon it. Nevertheless, that was a mass of varicose veins with surrounding inflammation, and with clots in the interior of the veins. It is almost incredible that inflamed varicose veins should be mistaken for sarcoma, but the people who saw it were people with quite ordinary intelligence and more than ordinary experience. On another occasion I remember seeing a surgeon operate upon a hard tumour in Scarpa's triangle. The diagnosis of that tumour was obscure. I am speaking of years

ago ; but if I remember rightly, it was thought to be probably an irreducible and inflamed femoral epiplocele. When exposed it was of a deep purple colour and very hard, and when it was dissected out it was found to be attached to a common femoral vein by a narrow neck. It was the ampulla of the saphenous vein, dilated and full of hard, old clot. I had never seen anything like it before. But that dilated ampulla of the saphenous vein is obviously a thing to which you ought to turn your attention, and be careful you do not mistake it for a femoral rupture.

Leaving now these cases of difficulty of diagnosis I will now come to another matter, namely, the effects which these veins produce upon the patients. First of all, as regards the mental position of the patients who have varicose veins. I have always observed that the patients are very anxious about the veins ; they think that their dilated and enlarged veins may burst and that they may in consequence bleed to death. Of course such veins very rarely do burst, and I think the patients would never, nowadays, bleed to death. But what has that to do with us medical men ? That is what the patient thinks. So let us drop our egotism and try to put ourselves in the patient's place and take his feelings into consideration. And it will be a great comfort to the people with varicose veins who come to consult you if you are able to assure them that they have varicose veins with very thick walls,

and that the veins have not formed adhesions with the skin, that there is not a very thin membrane between their venous system and the exterior of their bodies, and that therefore they may put out of their heads the idea that the veins are going to burst and that they will bleed to death. Further, one might inform them that if they were to have bleeding from their veins they would only have to lie down and apply a pad to the bleeding-point and that they would then be safe.

Another trouble which people with varicose veins suffer from is pain in the veins. I have always been a little puzzled by the pain in varicose veins. It is not the big ones which are painful, of that I am sure. I have seen gigantic varicose veins which caused the patient no pain whatever. On the other hand, I have seen quite small veins, perhaps running over the sharp edge of the tibia, which the patient said were exceedingly painful. I wonder why that is. Inflamed varicose veins are also very painful. Veins which have become varicose are very liable to become inflamed and thrombosed, much more so, of course, than healthy veins. But all inflamed veins are painful, and perhaps the most painful kind of inflamed veins are those which occur in gouty people. Whenever you see a patient with phlebitis you may make up your mind that he is suffering a good deal of pain. Patients with varicose veins suffer from many other troubles. Their legs feel exceedingly weary and heavy,

especially at night. Perhaps you do not attach much importance to that. But still, to ordinary human beings it must be very trying to work all day long, and then to have to proceed home, with legs which are so weary that they can hardly drag them along. At all events, if you recognize that you will be a more sympathetic medical man than the person who is unaware of the existence of the weariness and the weight, and therefore attaches no importance to them. It is often said that the legs feel so heavy that the patient can hardly lift them from the ground. Patients with varicose veins often say that the legs feel very hot and full, and as if they were going to burst. In addition to that, as you can easily understand, patients with varicose veins say their legs swell at night. The blood in varicose veins is passing on, though it may be slowly; but it is almost stagnant, and if it is stagnant you can imagine that the fluids collect in the tissues, and that the limb becomes œdematous. Patients are a little alarmed about what they call 'dropsy' of their legs. Dropsy is a terrible word with the public, because they do not discriminate between the various kinds of dropsy. So you may reassure them upon that point. And there is one clinical complication of varix which does not seem to receive the attention that it deserves. That is the cramp. I was not aware that those who had varicose veins suffered from very severe cramp, until one day a German came into the

surgery and I asked him what he complained of. He said, 'I have the cramp veins.' And then it dawned upon me that many patients I had seen with varicose veins had complained of having had cramp, having suffered very severely from it at night. A person who has to earn his living, perhaps in an arduous profession or vocation, and who does not get a proper night's rest, becomes ill and unhappy, and it is therefore a matter of very great importance to that person to have his veins cured and his cramp removed. So in considering any case of varix remember you have to take into your consideration the question of pain; the weight and weariness; the discomfort, in which must be included the sensations as of bursting; swelling of the limbs; and last of all cramp, which may be a very serious matter to the patient. People with varix are apt to have various complications. One of the complications which they get is eczema of the skin of the legs. I call it eczema, but I often wonder what the word eczema means. However, these patients sometimes get chronic inflammation of the skin of the legs. That may be a cause of considerable discomfort to them. In addition they get thrombosis of the veins. It used to be a moot question as to whether the clotting caused the inflammation, or whether the inflammation of the veins caused the clotting. That is a question I cannot answer for you. I can only tell you that people with varix are very apt to get clotting in

the veins of the legs. Of course that is a serious complication. A person who has phlebitis and clots in the veins of his limbs ought not to go to work, but lie up. But the ordinary wage-earner does not wish to lie up, and perhaps he cannot afford to, so it will be a matter of considerable pecuniary loss to him. In addition to that, you may say clots in veins may tend to escape and pass on in the circulation and cause pulmonary thrombosis. I have never known a person with non-septic phlebitis and thrombosis of the veins of the limb die. I remember a surgeon of great repute who told me this. He said, 'I remember once when I had thrombosis of the veins of the leg, and a doctor came to me and told me I should die. I said, "No, you silly man, I won't," and I did not die, I am here now, and that is fifteen years ago.' So I innocently said to him, 'How did you know that you would not die?' He said, 'Oh, nobody ever dies of thrombosis.' He is not quite right, because sometimes a clot is displaced from the uterine or other veins, but death from phlebitis or thrombosis, without septic trouble, is exceedingly rare. Once I was passing through the wards of a hospital when the house-surgeon said, 'You would like to see this patient; he has phlebitis, and he is going to get gangrene and die.' So I glanced at the man. He had phlebitis of the veins of his leg, which was enormously swollen and purple and blue, and to all appearances he had a very

terrible state of affairs. But I said, 'He will not get gangrene, and he will not die.' He did not get gangrene, and he did not die, he got well. And I was asked why, after all the faculty had made the statement that the man would get gangrene and die, I knew that he would not. I was going upon the remark which was made to me by that very able friend, which was that people with phlebitis did not die. I do not think I have exaggerated the matter. I think you are safe in assuming that people with phlebitis and consequent thrombosis rarely die. But you have to remember that these patients have very long illnesses, and they take a long time to get better, and not infrequently their limbs do not completely recover.

Another complication which people with phlebitis get is ulceration of the skin of the leg. You may make a few generalizations about ulceration of the skin of the leg. You may say this, that ulceration due to varix occurs at the inner side of the thin part of the leg. If you see any ulceration at any other part of the body, be a little suspicious about it. Ulceration due to varix occurs on the inner side of the thin part of the leg. Ulcers due to injury occur on the front of the leg, where injuries can, of course, be easily inflicted. If you see an ulcer on any part of a limb or other fleshy part of the body, be suspicious of that, because it may be due to syphilis, or tubercle, or to malignant growth. At all events, it is something which must be taken very seriously

into consideration. I have not time to go into the question of the causation of ulceration now, but, nevertheless, you ought to begin to consider the subject. I think that in varix the way in which the ulcer occurs is this: I have already remarked that the blood in varicose veins stagnates and does not pass on. It stagnates, not only in the main vein, but if you look at the ordinary case of varix which you see in the surgery, or in the out-patients' department, you will see the patches in which it is stagnating in the small veins. Now, an individual, in order to live, must have a supply of food; he must have a supply of oxygen, and be able to excrete his carbonic acid gas, and so on. But the existence of the cells and tissues of the body depends upon the same conditions being fulfilled. If the blood is stagnating in the veins, how can the tissues get food and oxygen or get rid of their waste products? Of course they asphyxiate, and starve, and poison themselves. A person either dies as a whole or he dies in little pieces. If he dies in little pieces, that is called ulceration; if he dies in little lumps, you call it sloughing; if the whole limb dies, it is gangrene. We are concerned now with ulceration, in which it dies in little pieces.

There are some other complications which we have already considered. We will speak next of the question of hæmorrhage. Hæmorrhage is not rare. It may occur from an ulcer, but often from a vein which is so thin that it is able to burst.

What bearing has this upon the last question which we have to consider, namely, the treatment of varicose veins? To go back, I can make it clear to you as to how treatment of varicose veins would be determined in your mind. The palliative treatment of varicose veins, of course, is simple. It is rest, elevation of the limb, perhaps a cold lotion applied to the limb at the inflamed part, and, lastly, by some sort of support. It may be necessary to try more than one kind of bandage or stocking before you find out what form of support suits the patient best. But palliative treatment may not be quite within the range of practical politics; you may have to proceed to some form of active treatment. What would lead you to suggest that a patient with varicose veins should be operated upon? The question of occupation always arises, because, as you are aware, the public Services will not accept—I think I am right in saying that—people with varicose veins of the legs. First of all, a man with this condition will have his legs feeling weary if he has to march twenty-five miles a day for six or seven consecutive days. Next, he gets hideous cramp at night, and may not get any rest. Consequently, he will feel very ill next day, and not be able to march. Next, towards evening he may say, ‘My legs are so tired, and weary, and swollen that I cannot lift them up properly.’ So, naturally, the Army and Navy will not have people who are the subjects of varicose veins; they will

not have people with even small degrees of varicosity; they have learnt from experience what I have repeated, that it is not people with very big veins who suffer most. Sometimes I have seen quite honest people who say they have suffered very great pain from a very small varicose vein over the front of the tibia. The Services cannot afford to have men who cannot march because a small vein is painful. There is also a further matter bearing on the ordinary elements of human nature. The lawyers have an excellent expression which I strongly commend to you: 'You must take human nature with its infirmities.' Now, one of the infirmities of the human being is that if he is told to march against the enemy and take his chance of being shot he may discover that his varicose vein, or varicocele, has become extraordinarily painful, and if he has something objective, something which he can show, it is difficult to contradict him and say, 'You must go out and get shot in spite of the pain.' So I think the Services are quite right in absolutely refusing to have to do with a person who has a defect to which he can point. This subject has been much discussed in the papers lately. It is one of the topics which you will do well to think out for yourselves.

The conditions which will determine you to operate, or advise operation, on a case of varix, would be such as I have mentioned to you. But there are some additional ones which render the

operation one of necessity rather than of expediency. For instance, the vein may be exceedingly thin and be on the point of bursting, and it may burst under unpleasant circumstances, when proper treatment is not at hand. Or the patient may have on the thin part of the leg a deep purple patch, where the tissues are becoming suffocated, as popularly understood, and where an ulcer is already forming. Or, again, you may observe that the patient with varicose veins has repeated attacks of phlebitis. How many months or weeks in a year ought a patient to be incapacitated by attacks of phlebitis which in themselves are not a danger to him? Such attacks are certainly a serious inconvenience.

I wonder whether I have made it clear to your minds how you are going to first diagnose your cases, and then treat them. I am talking all this time about things which you can see every day in the out-patients' room. I am sure many of you must have seen men in the out-patients' room with varicose veins; you must have seen them with phlebitis and with ulcers. I put it to you, have you thought what that means to the individual? How much does it take away from his wage-earning capacity, how much inconvenience is it to him, and how much terror does it instil into his mind? How much danger and inconvenience has this ulcer been to him? You must learn to think in that way, and I maintain that this ordinary subject of

varicose veins is an excellent one for you to begin to practise upon. Supposing it comes to this, that you think you ought to perform an operation upon a person with varicose veins. Be careful how you proceed. Fortunately, many of those who are afflicted with varicose veins are young people in excellent health. Whether their varicose veins are due to some developmental defect which they have inherited or not I cannot guess. It is easy to say, but it is difficult to prove. But it may be so. And these are people who, of course, will bear prolonged anæsthesia. The best kind of patients to operate upon are those who have a small group of varicose veins in one vein which can be easily removed through a few incisions. The bad kind are those who have a sort of nævoid condition of the veins of the leg, innumerable small varicose veins where it is obvious you could not remove them all, and it is difficult to tell which are the proper ones to proceed upon. And, last of all, remember that an operation for varicose veins may occupy a very long time. Looking through my notes, I observe that the longest time I have occupied on varicose veins of both legs has been two and three quarter hours. Now, to bear anæsthesia and exposure to the cold for two and three quarter hours means that that patient must have a very good tripod of life, as it was called long ago, namely, that the lungs, the heart, and the kidneys must be sound. The nervous system ought to be

included, though it is perhaps of less importance in this connexion. Therefore before you proceed see that the patient has good lungs, that the heart and circulation are good, and that he has not any form of nephritis. Provided that is so, you can proceed with your operation.

I do not propose, in a clinical lecture such as this, to go into all the minute details of the operation. I will simply say this: that at this hospital the operation which has been found to be the best is that by small multiple incisions one or one and a half inch long. The vein may be exposed and pulled out, clamped, excised, and tied with fine silk, or whatever you find suitable, it having, of course, been rendered sterile, and then the vein should be allowed to spring back underneath the tissues. One thing I advise you to always do in making that incision, but which I do not always see done, is to make the incision oblique, because the veins are not always easy to find. If you make an oblique incision over the course of the vein you are more likely to find it than by a straight incision made over what you think is the course of the vein. I think the best place at which to begin is at the top, not at the bottom. I have already said that the blood in varicose veins is usually running very slowly indeed from below, but it not infrequently runs from above, because the vein has not any valves in it, or else its valves are incompetent. But assuming it is running from below, if

you tie a vein high up first, it is perhaps a little easier to find those below, because they are full of blood. Some people have obtained very good results in the treatment of varicose veins by performing the operation which is called Trendelenburg's. In it the saphenous vein is tied close to its entrance into the common femoral. Personally I prefer to tie well above the varix, but do not cut down and tie the saphenous vein close to its junction with the common femoral vein unless at that point it is actually varicose. But you will remember that it is wise for you to prefer to go high rather than to content yourself with tying veins at the inside of the knee and in the calf.

As regards the subsequent treatment of those wounds, you may have observed that the cut is made, the vein clamped, a piece pulled down or up, and a few inches cut out, and it is tied. Then the vein springs back. Perhaps the wound is immediately closed with a continuous suture, and that is best, because it prevents infection getting in. When that is over, you will find it advantageous to put a pad over the wound, consisting of cyanide gauze, and immediately put pressure upon it with an ordinary antiseptic bandage. This bandage is usually made of cyanide gauze. You might have said, why not after that merely apply the gauze without any suture? Because at this hospital, that was frequently done about four years ago. The wound was made, the vein twisted, and that bandage

was wound round the limb, bringing the wound together. I tried it on many occasions, but found that on applying the bandage the small wound was not accurately brought together, and in addition sometimes when the bandage was taken off the wound either was gaping and had to heal up by granulation, or it gaped after the bandage was taken off. So obviously if that is true it is wise, after making the cut and tying the vein, to bring the wound together with a continuous suture of fine silk.

It used to be said that if you operate upon the varicose veins and tie them, then the blood has to return by the deeper veins, or that other superficial veins have to dilate to carry the blood back. I have kept on saying that I believe the blood in these varicose veins is stagnating, and in removing the varicose veins you are removing veins full of stagnant blood. There is no blood to go back by other tracks. And my experience of operations upon varicose veins has been that, in subjects which have been properly chosen and thoroughly operated upon, the results have been exceedingly good, not from my point of view, but from the point of view of the patient. I have forgotten to tell you what I believe to be the most essential part of the operation for varicose veins, and that is, before you perform the operation to make the patient stand up in a good light and sit down before him, and mark out every possible vein that he thinks is a trouble

to him, also mark out every vein which you can see and which looks likely to give trouble. If you do not do so, this is what will happen. After the operation, in which you have taken a great deal of pains, and everything appears to have gone well, when the patient begins to walk about he comes back to you and says, 'I wish you had removed that little vein which ran over the front of my shin. Why did you not do that? It is very painful.' Having had that hurled at my head, I take the greatest possible pains myself. I have the patient standing up and I say, 'Let us mark out your veins,' and I mark them in, any which I can see, and I say to the patient, 'Are there any other veins which cause you trouble? If there are, point them out.' He points them out and I mark them also. Then I proceed to the operation and tie them. If you fail to do that the patient is angry, and rightly so, for he has not been cured by your operation and has not got a proper return for all the trouble and anxiety, and a sojourn in bed for a fortnight or three weeks.

Here are a few specimens which have been brought down from the museum to show some of the complications of which I have spoken. There is a clot in the ampulla of the saphenous vein. I do not know the history of this case. Here is a saccule, from the saphenous vein with a clot in it, and you see how easily it might have been mistaken for epiplocele. I suppose the word 'varix' is derived

from *varus*, which means bent. Here is the varicose vein from the inner side of the knee, where the veins often become tortuous. These cases are exceedingly good ones for operation, because you can make either one single long incision or a semilunar incision and remove the whole of the tortuous varicose veins.

IX

SWELLINGS ABOVE, BELOW, AND WITHIN THE NECK OF THE SCROTUM

LECTURE I

ALMOST the commonest clinical case that you will be called upon to diagnose is one in which there is a swelling in the neighbourhood of the inguinal canal, of the scrotum, or of the neck of the scrotum. I have observed that this class of case is exceedingly difficult. Many mistakes are made concerning them, mistakes which are often very serious as regards the patient, and also as regards the reputation of the man who makes them. You may remember, if you attended the first clinical lecture which I gave, that I attach far more importance to methods of thinking and methods of seeing than to anything else. The sense of touch affords information, but it is not of a very reliable character. Still, with regard to the diagnosis of the tumours which we are about to consider to-day, the sense of touch forms an important part, and it has to be used methodically. Before I proceed further, I want to draw your attention to the parts which are con-

cerned. First of all, I would ask you to pay especial attention to the part which I will call the neck of the scrotum—that is, the constriction where the scrotum joins the trunk. It is situated just below the external abdominal ring and near the root of the penis. The neck of the scrotum is not an anatomical landmark mentioned, as far as I am aware, in any book on surgical topography. Nevertheless, it is one of the most important landmarks in the whole of this region. Obviously, if there is a tumour in this region it must be either above the neck of the scrotum, in the neck of the scrotum, or below it. We will begin to consider the tumours which are situated above the neck of the scrotum. Clearly, the moment you cast your eyes upon a swelling in this region, you can say whether it is or is not above the neck of the scrotum, and you know then fairly well the class of tumours which you have to investigate. If it is in the neck you have further information, and if it is below it you have still further information. Not only is the neck of the scrotum to be looked at, but it is also to be felt. What constitutes a normal neck of the scrotum? Observe I use the word ‘normal’, and I do it intentionally. I know there are many who use the word ‘normal’ and have no clear idea in their mind as to what the word really means. Now, what constitutes the normal neck of the scrotum? There *is* a neck at all events, and you notice that the scrotum on one side appears the same as it

does on the other; the skin is not red, it is not inflamed, it is not apparently altered. When you feel the normal neck of the scrotum you feel in it the spermatic cord and nothing else. How often have I asked gentlemen to feel a cord and they have not known the proper way! Observe how you proceed and then practise doing it. Stand upon that side of the patient on which you are going to feel; that is to say, if you are going to feel on the right side, stand on his right side. Pass the index finger underneath the neck of the scrotum, opposite the root of the penis, and pinch the thumb down upon it, and slip the constituents of the scrotum through your finger from within outwards. You ought to feel the vas deferens, which is hard like whipcord. If the sense of touch is good, you feel the spermatic artery beating. You will feel a number of other small cords, and strings, and fibres, which you cannot define. The veins and lymphatics I do not think you can feel. You possibly may be able to feel the nerves of the cord, more especially the genito-crural and branches of the ileo-inguinal, but I think the fibres which you feel are probably the fibres of the cremaster muscle. Unless you can feel those things clearly and accurately, you are not feeling a normal spermatic cord. What constitutes a normal inguinal canal? First of all, the inguinal canal is situated opposite the inner third of Poupart's ligament. You find the most extraordinary differences of opinion as to the exact

extent of the inguinal canal. It is limited above by the so-called internal abdominal ring (which I have never seen), below by the external one, which every one knows and is familiar with. The internal abdominal ring is outside the epigastric artery. The term 'internal abdominal ring' is merely the name of a locality. The inguinal canal ought to have no impulse in it. I am now going to tell you something which, to my mind, is of the very gravest importance for you to realize. It may not be true, so accept it critically. I am strongly of opinion that impulses can be better seen than felt. If you see an impulse, be very cautious what you say about that patient. If you do not feel an impulse, do not attach too much importance to your failure to do so. In the normal inguinal canal you cannot see any impulse. When a patient stands in front of you in a good light, you should see no impulse. Of course, the whole abdominal wall may heave, but there should be no localized impulse in the inguinal canal. Next pass your finger towards the inguinal canal by tucking up the scrotum. When the tip of the finger is near the neck of the scrotum, feel for the spine of the pubes. The external abdominal ring is situated inside the spine of the pubes; perhaps it is more correct to say that it is above the spine of the pubes. What ought a normal external abdominal ring to be like? I cannot tell you. As a rough standard, it should barely admit the tip of your index finger. When

a patient coughs it should close, and squeeze out the tip of your finger, which means that its boundaries are strong and taut, and not weak and flaccid. You can infer from what I have said that if you pass your finger up the external abdominal ring into the canal, that that is an abnormal canal. Well, I have often operated and explored canals of that sort and found nothing in them, but I am always very suspicious when the external ring is large enough to admit the index finger and enable me to explore freely with my finger the inguinal canal. The next structure which you ought, I think, to be clear about after the inguinal canal is the contents of the scrotum. What should you feel in the scrotum? Sometimes the scrotum contains swellings which are not testicles, but which are mistaken for testicles because the examiner is careless. But unless you can feel the spermatic cord coming down to join the epididymis, and unless in front of the epididymis you can feel the elastic body of the testicle, you should be careful what you say about it. Also, in the scrotum you may be able to feel the pampiniform plexus with a certain amount of ease. But, because you can feel the veins, it does not necessarily follow that you should diagnose a varicocele. Where the normal pampiniform plexus ends and varicocele begins I cannot tell you. But you must not condemn everybody as having varicocele if their pampiniform plexus is fairly easy to feel. Assuming all this is clear in your minds, I will

proceed to consider some swellings above the neck of the scrotum. The patient having been stood up in a good light and told to cough, we will assume that you see a swelling in the region of the inguinal canal; that is to say, it is opposite the inner third of Poupart's ligament. When the patient coughs there is a sharp, distinct impulse. The probabilities in your mind in a case of that sort would be that the tumour is a hernia. But I advise you not to put that proposition first. I advise you to say to yourself that there is a swelling above the neck of the scrotum and in the region of the inguinal canal, and then say to yourself, I will now try to ascertain whether that swelling is outside or inside the inguinal canal. You will be very puzzled some day with a swelling in that region which is not in the inguinal canal, but outside it. It is, of course, a foolish mistake to begin to operate on a supposed swelling in the inguinal canal and to find that after all you are dealing with, say, a chronic abscess over the inguinal canal, or an enlarged gland. There may be other things over the inguinal canal. For instance, besides the tumours I have mentioned, a femoral hernia, not infrequently after leaving the saphenous opening, turns up over Poupart's ligament and lodges over the inguinal canal. Another structure which sometimes passes over the inguinal canal is the testicle itself. You may have seen a boy operated upon last week in whom we thought this had happened. On the left side his scrotum

was empty. In the left inguinal region he had a swelling, and the question was, Is that swelling inside or outside the inguinal canal? We suspected it was a testicle. I guessed it was outside the inguinal canal. We thought we could feel the aponeurosis of the external oblique underneath the swelling. At the operation this proved to be the case. Whether the testicle had ever passed into the scrotum I cannot tell, but it had turned back over the aponeurosis of the external oblique, and covered over the inguinal canal. As you may be aware, the first step in the transition of the testis is the passage of a sort of test-tube of peritoneum down from the abdomen into the scrotum, and then the testicle follows that, descending into the test-tube. Sometimes the test-tube, or processus vaginalis, passes back over the external oblique without the presence of the testicle at all. For instance, on Monday I operated on a small boy who had his testicle in the abdomen, but the processus vaginalis had passed down the inguinal canal, and turned up again in the external oblique towards the anterior superior spine of the ilium. To revert to the question of femoral hernia over the inguinal canal, a condition which is not always very easy to diagnose. In a thin person it may be easy, because you can feel the neck of the hernia running outside the spine of the pubes and below Poupart's ligament into the saphenous opening and towards the femoral canal. But if you cannot

feel the neck you must be exceedingly cautious in your diagnosis. I have seen the late Luther Holden begin to operate on a case of supposed inguinal hernia in the female, and before he had proceeded far with his operation find that he was dealing with a femoral hernia which had passed upwards. A few years ago I began to operate on a female patient for the radical cure of an inguinal hernia, and found the sac of a femoral hernia over the inguinal canal. In fact, I had mistaken a femoral hernia for an inguinal hernia. How shall you avoid such mistakes? I will tell you how you can easily find out whether a tumour is outside or inside the inguinal canal, if you are able to perform the manœuvre. You cannot always do it on fat people or on females, but if you can pass your finger through the external abdominal ring and feel the tumour above the external oblique, you need have no doubt about your diagnosis. I dare say there are other structures which may be discovered outside the inguinal canal which I have not mentioned to you. I do not think it would be right for you to endeavour to carry in your minds a list of the structures which might be found outside the inguinal canal. The thing to carry clearly in your mind is this: that when you are confronted with a swelling in the neighbourhood of the inguinal canal, your first thought should be, Is it inside or outside the inguinal canal? Let us proceed a little further with the diagnosis of tumours which may be met with inside

the inguinal canal. There is a swelling in the neighbourhood of the inguinal canal which has a sharp impulse when the patient coughs. The finger passed up the scrotum passes into the external abdominal ring, which is loose, and when a cough is given something is felt to be striking against the finger. This something goes back when the patient lies down. That, I suppose, would be the simplest possible case of an ordinary inguinal hernia. If, in addition, you felt it gurgle as it went back, you would think it was probably intestine containing air. With the exception of the case in which it gurgles, I have seen those signs over and over again simulated by a condition which is common, but I think seldom talked about or recognized. The condition I refer to is that which I have called an inguinal varicocele. In future do not speak of varicocele as I did at the beginning of this lecture, but speak of two kinds, scrotal varicocele and inguinal varicocele. When once you have realized that the veins in the inguinal canal may be greatly enlarged and varicose, and bulge out the canal, there is no difficulty in understanding that that is very frequently indeed associated with ordinary scrotal varicocele. I do not think a case of varicocele in which you could feel the enlarged veins passing up into the neck of the scrotum, in which you could feel them passing through an enlarged inguinal canal, and in which you saw a large swelling in the inguinal canal corresponding

to its boundaries, would be a source of difficulty to your mind. You would say such a man had got a varicose condition of the veins in the neck of the scrotum and in the inguinal canal.

Now, consider another condition which I have often seen. I have seen many times no scrotal varicocele, no varicocele in the neck of the scrotum, but a marked varicose condition of the spermatic veins in the inguinal canal. And this is a very puzzling condition, and the diagnosis of it is exceedingly difficult. The possibility of its presence is not recognized, and I suppose, therefore, it is not diagnosed. I wonder how many people are wearing trusses upon inguinal varicoceles, and I further wonder how many people are complaining of the pain and discomfort which that entails. It is of the greatest importance to diagnose inguinal varicocele. For instance, I remember seeing a patient who had an inguinal varicocele. I guessed that he had inguinal varicocele for these reasons. First of all, he had a little swelling in the region of his inguinal canal. This swelling became greater when he stood up. Next, the swelling had an obvious impulse when he coughed, so that it was in that respect very like a hernia. Next, the swelling went away when he lay down, but slowly. Next, he had a dilated inguinal canal. I might have said it was an inguinal hernia, but he had some slight dilatation of the veins in the neck of the scrotum and, I think, also in the veins of the pampiniform plexus.

The advice given was to have the inguinal canal opened and the veins removed. I attach the greatest importance to opening the inguinal canal when a patient has got an inguinal varicocele. Over and over again, when we open canals in which there is an inguinal varicocele, we find that the patient has got a small hernial sac there. Perhaps it is an inch long, and, there is something very curious about the sac; for it is rather pointed. Within the last ten days a man has been operated upon by me in the theatre. On the left side he had a large varicocele and dilatation of the veins in the neck of his scrotum, also an impulse and swelling in the left inguinal canal, with dilatation of the external abdominal ring. Obviously an operation done on the scrotum would not have cured him. I can tell you of an instance of that. The patient I was telling you of a moment ago had almost a similar condition, and was advised to have his inguinal canal opened and the veins removed. He was operated upon some time later, and the operation was done in the ordinary manner through the scrotum. I remember seeing him a year later. He said: 'I have come to see you again because I have had my operation done, but I have still got my pain and discomfort.' 'What operation have you had done? There is a scar upon your scrotum. You have not had the operation done which you required, which should have been done higher up, where there is still a swelling, and an impulse on

coughing, and enlarged veins.' I might have added that the surgeon who did it ought to have looked for a hernial sac, which is so frequently present. The man you have seen in the ward had on the left side a huge dilated vein and a small sac. On the right side, when he stood up in a good light, he had also a faint impulse on the upper part of the right inguinal canal. I said that that side also ought to be explored, because he had either got some dilated veins there or possibly a hernial sac. When we explored we did not find dilated veins, but a hernial sac at least one and a quarter inch long by more than three-quarters of an inch in diameter ; just the condition necessary for him to get a rupture. I am very suspicious of inguinal varicocele. I cannot help thinking that when the veins in the inguinal canal are varicose they dilate and weaken the canal, and that they are an advance guard for the hernial sac which you will so frequently find there. Inguinal varicocele, when it is present, is often accompanied with fat in the inguinal canal, which also bulges it out, and has an impulse. When the testicle makes its transition out of the abdomen into the scrotum it is preceded by the processus vaginalis, or test-tube of peritoneum, to which I have just referred. But peritoneum has fat upon it, and therefore it is easy for you to understand that in infants you find about the processus vaginalis some lobules of this so-called subperitoneal fat. That fat accompanying the testicle in its transition

does not always wither away or remain small, but sometimes grows, so that I have removed fatty tumours of considerable size from the inguinal canal, and also occasionally from the upper part of the scrotum. Now I am going to tell you a very curious thing about these fatty tumours of the spermatic cord. You may observe I myself attach a good deal of importance to that which I can see; it is quite natural for anybody to think when he has seen fat in the inguinal canal that he has seen fat—in other words, that fat can always be recognized with the naked eye. Well, I know that very strange things are found in the inguinal canal, and I have been in the habit of having what I have removed from the inguinal canal examined microscopically whenever I could; and I confess I was decidedly surprised when one day, after saving up some fat for examination, the pathologist sent me back a specimen of suprarenal body. If you are interested in the matter, you will find a photograph of the section in the Pathological Society's *Transactions* of about seven years ago. And you will also find the photograph of a human embryo which throws some light on the point. In the human embryo in one complete section there is at the top of the abdomen the proper suprarenal body, and there is passing over the front of the embryo kidney the lower part of the suprarenal body, becoming continuous with its epididymis. In other words, the top part of the Wolffian body has become suprarenal

body, the lower part has become epididymis. Now I shall set you thinking, because there must be men who have got, or will soon have, inquiring minds. Why does not somebody investigate the fate of the part of the Wolffian body which lies between the suprarenal body and the epididymis? I very strongly suspect, first of all, that the part in the hilum of the kidney accounts for those curious malignant adenomata of the kidney which are exactly like pieces of Wolffian body. Next proceed along the course of the ureter. I have removed curious cysts which otherwise appeared to be unaccountable. They are very like ovarian cysts, but they occur along the ureter and almost in the flank. I believe they are to be found also in males, though the two I have seen have been in females. This is a branch of pathology which is worth working up, and which I am sure will afford valuable results for any man who will pursue it. There are other things to be found in the inguinal canal besides fat. I have spoken of males so far, but I might have mentioned a case of lipoma in the female. The female has a smaller inguinal canal than the male, and it is less liable to disturbances. But occasionally odd things are found in the inguinal canal of the female. For instance, I remember a lady who complained of pain in the top part of the inguinal canal. When she stood up before a good light—and a good light is essential for the diagnosis of these conditions—the first thing that

could be seen was a minute elevation of the skin at the upper part of the inguinal canal. When she coughed there was an impulse at that point. The opinion was expressed that she had something in her inguinal canal, and that as she had a pain there, and it was interfering with her comfort, and she hated wearing trusses, it was better that it should be explored. The result was that a lipoma of some size was found in the top part of the inguinal canal, and it was removed. She then dispensed with the truss which she had worn for some time. Another tumour which is sometimes found in the inguinal canal of females is constituted by the ovary and Fallopian tube. I have twice met with an ovary and Fallopian tube in supposed inguinal hernia of females. One was in a child, and I remember it very well; it is very vivid in my mind. It was a child who was thought to have a strangulated hernia in the right inguinal region. It had a swelling which was tense, red, exceedingly painful, and would pass very well for strangulated hernia. But the child was not vomiting and its bowels acted, so that was out of the question. When this swelling was examined, I found an intensely inflamed ovary and Fallopian tube. The reason they were inflamed was that they had become twisted and full of blood. That case has been described somewhere, and others have been met with since, but the condition is one of extreme rarity. The presence of the ovary and tube in the

inguinal canal is not particularly rare, but still it is far too rare for you to diagnose. Supposing you make up your mind that a patient had got a rupture ; do not make up your minds that you know what is inside the sac. I think it was Petit who, about two centuries ago, said no wise man pretends to know what is inside a hernial sac. I remember hearing a surgeon make a speech when he was about to operate upon a female to remove the ovary, which had (he said) descended into her inguinal canal. I watched to see what did come out, and the thing which came out was a lump of hard, inflamed omentum. It is hopeless to guess what is inside hernial sacs ; if you do you are almost certain to be wrong.

Another structure which is met with inside the inguinal canal is hydrocele of the cord. The processus vaginalis ought to be closed from the so-called internal abdominal ring down to the head of the epididymis ; but its closure is often incomplete. Fluid may collect in a piece which has failed to close. If this be situated in the inguinal canal, a small globular tumour results. This may be taken for a rupture, for when the patient coughs there is an impulse. When you put your finger up the external abdominal ring, which is usually dilated, you feel a swelling and impulse. There is a great temptation to call that a hernia, and you are apt to do that unless you pull it down by pulling on the cord below it. You will observe then that

it moves completely with the cord. If you can pull it down sufficiently far to get your fingers above the top part of the globular swelling, you are reasonably sure of your diagnosis. If you were to get one of those lamps which are used for the diagnosis of fluid in the frontal sinus, you would see that the swelling in the inguinal canal is not opaque, but translucent. The treatment of hydrocele of the inguinal canal is simple: you perform an ordinary radical cure of hernia and get quit of it.

I will very briefly recapitulate the points to which I have drawn your attention, and to which I believe I draw your attention in the wards once a week, and that is the various steps in the diagnosis of tumours in the region of the neck of the scrotum and of the inguinal canal. First of all, inspection. The fault often committed is that the patient is not put in a proper light. I do not know of any class of case which wants more careful visual inspection than these tumours in the inguinal region. So the patient is put in a strong light, and the swelling is seen. Mark its position. If it is above the neck of the scrotum in the region of the inguinal canal, you have to proceed to investigate the inguinal canal and its surroundings. Do not assume that the swelling is inside the inguinal canal. Your first instinct must be to say, 'I see a swelling in the neighbourhood of the inguinal canal, and I am going to ascertain whether it is inside or outside the inguinal canal.' Swellings outside the inguinal

canal may be exceedingly misleading. But if you can pass your finger into the canal and get behind the swelling, you cannot have any doubt that the tumour is outside the inguinal canal. If it is a tumour outside the inguinal canal, it may be an inflamed gland or an abscess, or it may be that most misleading thing—a femoral hernia which has ascended. If it is a tumour inside the inguinal canal and you are sure of it, begin to try to diagnose the different possibilities. Do not be led away by that dreadful fallacy of diagnosing what is commonest. I cannot imagine anything more silly than that. Unfortunately for you, there is no 'common' case; every case is one for original observation and will necessitate an original conclusion on your part; otherwise you may find you are very wrong. The thing most apt to mislead you is, I think, an inguinal varicocele. If you mistake an inguinal varicocele for a hernia, you may condemn your patient to wear a truss. If you do not condemn him to that, you permit him to go on with these dilated veins, perhaps in pain, perhaps in discomfort, till at last he develops a hernia which he should not have developed. The proper course is to open the inguinal canal and to get rid of the veins.

The other things in the inguinal canal which I have mentioned are rarer, but still they are of importance. Fatty tumours of the canal are occasionally found. When you meet one in operating, explore the interior of it and then very likely you

will find a hernial sac. When the inguinal canal contains the testicle a number of questions will arise.

The following is a by no means unusual state of things : A child has an undescended testis. Perhaps it has reached the inguinal canal. There is sure to be a communication between the testicular sac in the inguinal canal and the abdomen. It is so rare not to have a communication between the processus vaginalis and the abdomen that it is hardly worth while taking it into consideration. If he has a communication with his abdomen, he may develop a hernia. If he does, the condition is rather dangerous. The processus vaginalis often has a very tight neck. On the whole, therefore, it is by far the best to operate. That will prevent or cure the hernia. What is to be done with the testicle when it is in the inguinal canal? Is it to be taken away? It may have to be, and you ought to have permission to do that before you proceed. Or is it to be pushed back into the abdomen? Why not? I have seen more than one patient who has had a testicle pushed back into the abdomen. One complained of pain in the iliac fossa and others did not. If you push a testicle back into the abdomen it will never grow—it will remain half its proper size; and if you were to examine it microscopically you would find that it contained no spermatic cells. Why not put the testicle down into the scrotum? You cannot always promise to do that. I think you could

promise to put it outside the abdominal ring, but it is not a very satisfactory place to put it into. But often you can put it down into the scrotum. When the testis is turned back over the external oblique it goes well into the scrotum. What becomes of the testicle when it is put into the scrotum? If the testicle is in the inguinal canal, it never develops spermatic cells. If it is put down into the scrotum, what happens to it? I have got this far, that I have seen such a testicle grow bigger; of this I am sure. But that is very different from saying it develops and contains spermatic cells and spermatozoa. To settle the question would be very difficult. To do so somebody would have to put the testicle down into the scrotum without tension, and see it grow larger, and then remove it, and see if it contains spermatic cells and spermatozoa. And that I am afraid will not be done. Observe this is a very important problem, and that practically a great many questions will turn upon it. For instance, there was an heir to a very fine estate and title, whom it was discovered had both testicles in the abdomen. He was a cryptorchid. First of all the question arose, If his testicles remained in the abdomen could he beget children? That is an important matter for one who is going to succeed to a title and estate. As long as the testicles are in the abdomen he will certainly not beget children. I know that cryptorchids have claimed to have begotten children, but it depends on your frame of

mind as to whether you believe it or not. I am rather sceptical, so I disbelieve it.

Next, cannot the testicles be brought down? It is highly improbable, because if they are in the iliac fossa the cord is too short. If they were in the inguinal canal, the cord might be long enough to let them come down into the scrotum. If that were to be done, if they were to be got down in that way, would the testicles then develop? Nobody can answer that. But there are a great many curious, interesting, and important questions which arise concerning tumours and swellings in the inguinal canal such as we have been considering to-day.

I think next week I will proceed to consider with you the swellings in the neck of the scrotum, and the swellings in the scrotum itself, because quite recently we have had some swellings in the scrotum and in the neck of the scrotum, which have been of the most perplexing and curious character; and as you, gentlemen, are attending the out-patients' department and the wards, you will be constantly brought into contact with cases such as those we have been considering. I shall be interested to know from any of you whether I am correct in my statement that that important disease, inguinal varicocele, is not mentioned in the ordinary textbooks. And if not, you may take it as a warning and remember that the best textbooks are the patients in the out-patient rooms and wards of the hospital.

X

SWELLINGS ABOVE, BELOW, AND WITHIN THE NECK OF THE SCROTUM.

LECTURE II

You will remember that last time we dealt with the methods of ascertaining what a particular patient was suffering from. We said that from each thing seen or felt something was inferred, so that at last you had in your possession several inferences, and from those you drew a conclusion which was your diagnosis. Now we will proceed. Last time I essayed swellings above the neck of the scrotum, and naturally you might expect to-day that I should proceed with swellings in the neck of the scrotum. So I might, but instead will proceed with swellings in the scrotum. And I do so for this reason : that most of the swellings which are in the neck of the scrotum either have descended into it from the inguinal canal or they have ascended into it from the scrotum. So obviously for the elucidation of swellings in the neck of the scrotum it is first of all necessary to pass in review the

kind of swelling which may have descended and also the kind of swelling which may have ascended. Clearly, it is necessary to know both before you can proceed with the swellings in the neck of the scrotum. We will assume that the swelling is in the scrotum itself. You will begin with your eyes, and inspect the skin of the scrotum first. It is red, it is shiny, and from that fact you will infer that there is beneath the skin of the scrotum an inflamed structure of some kind. And that will make you exceedingly cautious; because the inflamed structures which are met with in the scrotum are often of exquisite sensibility, and if the unwary proceeds to feel incautiously, or squeezes the cord to see whether it ascends into the neck of the scrotum (and perhaps has not observed that there is a urethral discharge at the same time), he will probably make an enemy for the rest of his life. So it is, both to you and to the patient, of the very greatest importance that you should look at the scrotum as a beginning, with the view of seeing what you may venture to do. Let us suppose you have looked at the scrotal swelling, that the neck of the scrotum is the same on that side as on the other; you will then see what shape or kind of swelling you have to deal with. Suppose that at the bottom of the scrotum you see a globular swelling, perhaps lying horizontally or obliquely; you may assume that that is the testicle, and you will probably be right. You will then apply your

test to it. If that swelling at the bottom of the scrotum is testicle, you will be able to feel the spermatic cord descending to it. You will recognize the cord because you will be able to feel the vas deferens, and perhaps the spermatic artery and those small cords will slip about in your fingers. Next, the swelling itself will have an epididymis which you can feel—and you ought to make yourselves quite familiar with the exact feel of those structures when they are healthy. Let us suppose that, in addition, there is to be seen above the testicle a pyriform swelling which is irregular in outline, which disappears when the patient lies down, and fills up again when he stands up, and can be felt to consist of thickened vessels; then obviously you have to do with varicocele of the scrotum. That ought to be very easy to diagnose, and very few people would fail in diagnosing a scrotal varicocele. This is where so many fail; they fail to observe that the scrotal varicocele is continued up into the neck of the scrotum, and thence into the inguinal canal, which is dilated, and exhibits a faint impulse on coughing. I mentioned such an one last time, and said that an unobservant surgeon operated upon the veins at the neck of the scrotum but omitted to observe that the external ring was dilated and there was an impulse in the inguinal canal. The patient afterwards came back, complaining bitterly that he had not been cured, and expressed great sorrow

that a further operation was necessary to put him right. That is a position in which anybody may be placed unless he is an observant person, and unless he proceeds with his diagnosis in a methodical manner.

Now let us proceed to consider another swelling in the scrotum, which is almost always quite easy to detect with the naked eye, and that is an oval swelling in the scrotum covered with natural skin, and which in part of its length shows a slight, broad constriction. Notice, I did not say it was a pyriform swelling, but an oval swelling which in some part is slightly constricted. If I were to compare it to a jargonelle pear you might expect the constriction to be one third the distance from the thin end, but the constriction is often half-way down. Now, in making generalizations, I would say that collections of fluid in the tunica vaginalis become constricted. If you go to the top gallery of the museum, you will see a specimen of hydrocele which has been dried and blown up, and if you look critically at it you will observe its shape and that there is a slight constriction, and I think you will agree that at the situation of the constriction there are a few transverse fibres in the processus vaginalis. What these fibres are I do not know, but they are always there in some part of the tunica vaginalis, and they are the fibres which produce this constriction. I have further evidence that it is so from my own experience, because over and

over again when operating for the radical cure of hydrocele I have looked for these crossing bands and divided them, and as I have divided them I have observed the constriction disappear. Now, I did not say hydrocele only : I said any collection of fluid in the tunica vaginalis, because I have seen hæmatocele—that is to say, a collection of blood—in which this applied. And when I saw patients in the out-patient department, there were two cases of fluid which had all the appearance of pus when tapped. I think I remember that the Pathological Department reported to us that it was not actually pus. I confess I should have been surprised if it had been pus. Has anybody here seen pus come out of a hydrocele or out of the tunica vaginalis ? It must be of very rare occurrence. Why is hydrocele so little given to suppuration ? I have seen dreadful trocars pushed into hydroceles, and I have known hydroceles given every possible chance of suppurating and yet they have not. In fact, I suspect it is very hard to make them suppurate, because one of the old methods of treating hydrocele was to lay the sac open and fill it full of gauze, and another was to put setons in, and these rarely succeeded. To proceed with the hydrocele, I should say it is full of traps for the unwary. Let me give you an instance. A Governor of this hospital sent his gardener to the surgery with a note from his doctor, saying : ‘ I have attended this man for what I believe to be hydrocele, which I have tapped, and

nothing but blood came out, and therefore I have sent him up in order that you may remove a malignant testicle.' This is not exactly what he said, but it is the summary of it. That statement had also been imparted to the man. Now, these are the various false steps which had been taken. A hydrocele is translucent. Next, the vast bulk of hydroceles have a healthy spermatic cord above them. So when you have seen the shape of the tumour and felt the spermatic cord, you proceed to see whether the tumour is translucent. I have seen translucency looked for in all sorts of absurd manners, with wax matches, and I have seen a patient leap because the scrotum has been burnt. I have also seen the investigation made with tallow candles. I wonder the examiner did not ask for a rush-light. But if you resort to such mediæval things as that you are sure to come to grief. My advice is that amongst your possessions you should include some sort of small electric light. You probably have one which is used as a head-lamp, and I strongly advise you to have a Hering's lamp which is used for illuminating the antrum of Highmore. You may go further and have an extra good one, and have a focussing lens upon it, which enables you to illuminate the frontal sinus. When you get a powerful light like that behind a hydrocele in a dark room, what was formerly difficult now becomes an easy matter. I am afraid our friend omitted to examine the case I have referred to with

a proper light, though that is only a surmise of mine. He should have taken the patient into a darkened room, lifted up the scrotum, nipped the skin so as to make the hydrocele tight, and get every opaque structure out of the way—for instance the penis and the thigh—and then examined it properly with his electric light. Had he done that, he would have found out something else, because he tapped it with what I call a cow-doctor's trocar—that is, one of those horrid mediæval things which used to be put into all the instrument-cases. He might have used a trocar of one millimetre, or one and a half millimetre, which is quite big enough to let the water out. The patient would not have felt any more than a mere prick, the fluid would have run out, and he would have felt so little inconvenience from the operation that he might have gone and bought a trocar and tapped himself in future, which is what I have known patients do. This doctor had used a cow-doctor's trocar, and out came blood. He had omitted, when he examined it with the light, to examine it with another definite intent in his mind, and that was to see where the testicle was. This patient had the testicle in front instead of behind. The rule is for the cord to be behind the hernial sac and the testicle to be behind the hydrocele, but you must not trust to such rules as that, because some people have got an inversion of the testicle : it is rotated upon its axis, the tunica vaginalis being at the back instead of in front.

And that seems to have happened to this man. When he came here he was tapped behind with a minute trocar, the fluid was drawn off, and his malignant tumour was gone. That is an awkward position for any human being to place himself in, but all of you will do it unless you take the precautions I have mentioned. Supposing you agree, after examination, that a patient has got a hydrocele, you have observed its oval shape, have seen its translucency, and made up your mind that the testicle is behind; do not be too sure, but examine the neck of the scrotum. If there is any thickening in the neck of the scrotum, be very, very cautious how you proceed. That is not a straightforward hydrocele. Now look at the inguinal canal and at the cord. If that thickening goes up through a spacious external abdominal ring into a dilated canal, be very, very careful. Hydrocele is not often unaccompanied by some form of hernia. One of the forms which accompany it is congenital hernia with epiplocele, the epiplocele being perhaps impacted in the neck of the sac, and the neck of the sac being, in a case of congenital hernia, exceedingly tight, with firm fibrous rings in it. This is not an imaginary danger, because within the last five years I have had a patient in the wards who was diagnosed by his doctor to have a hydrocele. He was tapped under that belief. Very little fluid came out, and when it was over the doctor got suspicious and came to the conclusion that the

patient might possibly have a rupture. That is exactly what he had, a congenital hernia, with the neck full of impacted and adherent omentum, which was removed. He was a source of great anxiety to us, because his hernial sac was in an inflamed and septic condition, and obviously to do the operation for the radical cure of hernia on a person with an inflamed hernial sac was to do it under circumstances of great additional danger. I may possibly have conveyed to your minds so far that I believe hydrocele to be a disease. Of course, it would be just as rational to tell you that I think a cough is a disease. As a matter of fact hydrocele is a sign of disease. As regards the ordinary common passive hydrocele I do not know what it is due to in the least, but I know that ordinary hydrocele is only part of a chronic inflammatory condition of the tunica vaginalis and of the testicle. Supposing you proceed to cure the hydrocele, you cut it open, remove the parietal layer of the tunica vaginalis, and perhaps drain it for a few hours, and in ten days' time it would be quite well. I advise you always to pay the greatest attention to the radical cure of hydrocele, as it is a simple operation and gives most admirable results, the patients are exceedingly pleased, and there is little anxiety. But, like all other operations, it requires to be exceedingly well organized and properly managed, and to be founded upon a perfectly correct diagnosis. You may have done all this, but have omitted before

you began to say to the patient : ' You know this hydrocele of yours is due to chronic inflammation, and it is due to chronic inflammation of your testicle ; therefore after my operation you will find that that organ will remain rather large, rather hard, and perhaps occasionally tender.' If you do not tell him that, I think he would blame you, and legitimately, for having produced those results. There is another thing I should like to mention, and that is that in a person who has had hydrocele for a great number of years the pressure of the fluid apparently spreads out the testicle and flattens it, so that at last it becomes an atrophied organ. But I will assume that the cases have been operated upon within a reasonable time.

Now let us consider some other causes of hydrocele. We will take tubercle next. In some cases of hydrocele the collection of fluid in the tunica vaginalis conceals the fact that the patient has behind it a tuberculous testis. Supposing you overlook a tuberculous testis, and you see it only at the operation, and you have proceeded to remove it without permission, then you have landed yourself into an uncomfortable place. Some hydroceles are associated with syphilitic disease of the testis, and they, too, ought to be considered and carefully diagnosed. And lastly, some hydroceles are associated with malignant disease of the testicle. I think, gentlemen, you should be very cautious how you approach any kind of hydrocele. I might

mention other causes, but they are not active in this country. We seldom see those huge hydroceles which occur abroad and which are due to parasites.

Before I leave the consideration of hydrocele I will mention one other cause of hydrocele, and that is traumatism. I have occasionally seen hydrocele follow accidental injuries and surgical injuries. For instance, I remember operating upon a gentleman for the radical cure of hydrocele who had had his testicle injured by a cricket ball, and when I came to operate there was the remains of a wide split in his testicle, which one could not help associating with the production of his hydrocele. As a further reason for that, I have at least on one occasion after operating for varicocele, observed a certain amount of hydrocele appear. I have been under the impression that it has disappeared. I can remember a case in which it did not disappear. I recollect operating upon an officer for the radical cure of hernia. He was not a good case for radical cure, but he wished to remain in the army, and as a result of the operation a little fluid formed in his left tunica vaginalis. During the course of two years it very slowly increased, and ultimately, to enable him to ride and be more comfortable, the radical cure of the hydrocele was performed, and he remained well.

Lastly, I will draw attention to a class which are called congenital, and which are so important that I, perhaps, ought to have put them first. When

we use the word 'congenital' we simply mean that it is due to developmental defect. Congenital hydrocele in babies should be a source of great anxiety to you as medical men, for all sorts of reasons. There is a particular amount of terror which seems to attach to any swelling in the scrotum. These hydroceles in babies, I think, do not require to be complicated by the addition of words such as 'infantile', and so on. They are associated with an open condition of the upper part of the processus vaginalis. Whether the fluid comes down out of the abdomen or commences round the testicle I do not know, but I think it must come down out of the abdomen. The treatment is very simple in these cases. First of all the infant should have a truss given it, because you must assume that its processus vaginalis has remained patent. Do not believe all that you are told about the inability of infants to wear trusses. I never yet met with a baby which could not wear an ordinary rubber truss, provided it was properly fitted. After the truss has been fitted you may take other steps, and one other step is to tap the hydrocele with a minute trocar, and the next is to apply a slightly irritating fluid, such as chloride ammonium lotion, to the scrotum, and that nearly always results in the cure of the hydrocele. If the child grows up and is not cured, it would be desirable to perform an ordinary operation for the radical cure of hernia, and close the processus vaginalis high up in the

inguinal canal. With regard to congenital hydrocele associated with hernia, I have told you about that, but I want to mention one other complication of congenital hydrocele which may shock you. Our rules are to look at the whole patient first, next look at the whole inflamed structure and compare with the other side of the body if you can, and lastly, see it in a position of rest. Now, if you omit to look at the whole patient you may see a child—as I have—with a hydrocele of considerable size, and you may have omitted, from the lack of looking at the whole patient, to see that it had a large abdomen, and you would have been shocked when you tapped that case, having got an ounce glass ready to receive the hydrocele fluid, to collect a pint; and then it would dawn upon your mind that that child had a collection of fluid in its tunica vaginalis, and that it communicated with its abdomen, and that the child had tuberculous peritonitis, with effusion of fluid. We had such a child in the wards here. I cannot tell you all the surgical details, but I remember part of the treatment, and the case resulted in a series of events such as those which I have mentioned to you. So when you are dealing with children with so-called congenital hydrocele, I advise you to approach them carefully.

Now I come to speak of another common affection of the testicle, and that is tuberculous testis. In my mind there are two kinds of tuberculous testis; one of them is the acute and the other the

chronic. The acute tuberculous testis is usually, as the name implies, painful, rapid in its increase in size, and attended with considerable inflammation. It soon begins to soften in various parts and if left alone it begins to ulcerate. The swelling may be ovoid, but it is sure to be accompanied with considerable thickening and tenderness of the cord in the neck of the scrotum, perhaps passing up into the inguinal canal. When you operate upon such a case as that you will find that the spermatic cord, where you divide it, is œdematous, and perhaps has areas of caseous degeneration in it. The testicle itself is a huge inflammatory mass full of caseous lumps or nodules, and which are in the process of softening, and also threatening the ulceration that I have mentioned. So it becomes a question what you should do with such a testicle, because it is only a part of the general tuberculosis. Patients with that condition have got a high temperature and perhaps advanced tubercle in the lungs or elsewhere, and it is doubtful whether it is worth while to remove the testicle or not. I am in favour of removing such testicles on the ground of humanity. First of all, the removal of a testicle of that description causes singularly little shock or illness to the patients. They take the anæsthetic well if it is properly given. The removal of the testicle is a great mental and physical relief. You can imagine how painful it must be to possess a huge inflamed heavy testicle which is on the point of degeneration

and ulceration. So that, contrary to the practice of some, I feel a tendency to get rid of such an encumbrance, although the operation may not in the slightest degree prolong the patient's life, as he is dying of tubercle in various tissues or organs.

The chronic tuberculous testis is a different matter. Think much about the nodule of chronic tubercle which the patient has got in the head or in the tail of the epididymis—because that is where it usually first appears—that nodule should excite in your minds a very strong suspicion. At that period it may have gone through the various changes which tubercle undergoes. The tubercle bacillus gets into the human body in some manner, through the tonsil, or some wound, or is inspired ; it then passes along the lymphatics and vessels, say into a gland near the tonsil where it perhaps began. The inflammatory products then undergo a curious change ; they become converted into what is usually called caseous material, because, I suppose, it resembles cheese, and perhaps it does a little. It may remain quiet for a long time, or it may even calcify, but the common course for that caseous material to pursue is for it to soften, and the result is a tuberculous abscess. Place this condition of things in the epididymis or in the testicle, and it will go through exactly the same course of events. It will inflame, and at that stage the epididymis is tender. You may say there is a slight attack of epididymitis. If you are a credulous individual, you might credit

it to gout, or neurasthenia, or some rubbish of that sort. If you are not credulous, you will say that there is inflammation of the epididymis and will observe and see what happens. Next, in the course of weeks or months the tenderness will become less, but the nodule will be very hard. You may be suspicious. We will take it at that stage, because that is the one in which they are generally when you meet with them. But tubercle is not a disease of an organ, but a general disease. It will spread quickly, and it spreads up the vas deferens and up the lymphatics. Your mind should carry you from the beginning to the end of the genito-urinary tract. You feel the head of the epididymis, you feel the vas deferens which may have in or about it little nodules of caseous material. After the vas deferens has entered at the so-called internal abdominal ring it passes down the pelvis towards the prostate. There you can begin to feel it again; rectal examination is an essential part of the examination of these cases of nodule in the testicle. If you do not make a rectal examination you will overlook the true cause of the disease. If it is tuberculous you will probably find small hard nodules in the prostate, close to the end of the vas deferens, on one side or the other. As a rule you feel these nodules on the side of the diseased testicle. Next, the finger is passed up the rectum beyond the prostate, and then you feel the ampulla of the vas deferens, and the vesiculæ seminales. They may

have gone through exactly the same stages. They may be inflamed, and in that case you feel them thickened, tender, and hard. If it has gone on to caseation, you will feel a hard lump, with very little sensation of pain in it at all. Next you proceed one step further, and I have been rather astonished at the result of this manœuvre. The patient is supposed to have tubercle, and you have very strong reasons for thinking that he has tubercle of his genito-urinary tract. He is asked to pass his water into a glass, and that is held up to the light, and very likely it will contain blood, and I have seen it contain pus and muco-pus, and all sorts of shreds. In other words, the tuberculous disease may have begun close to the epididymis, passed along the vas deferens to the prostate and the vesiculæ seminales, and out of the ejaculatory ducts and infected the neck of the bladder, where there is tuberculous ulceration, because as the tuberculous material softens it discharges into the bladder. When you look into the bladder with the endoscope you see it is tuberculous. How do you know? You see the caseous material in the base of the ulcer. But you have not finished with your case yet; your mind will still be prompting you to follow up the ureters. Tubercle may spread up the ureters, just as the septis does in the case of ascending pyelonephritis, and you should therefore examine the kidneys, to see if there is any suspicion of tubercle there, because it would be a great disaster to remove

a tuberculous testis and then discover that there is tubercle of the prostate, tubercle of the vesiculæ seminales, and perhaps tubercle of one kidney—errors of diagnosis against which I am sure you must be most anxious to guard yourselves. Your best means of avoiding these mistakes is to pursue a proper method of examination, and that method really means that you are to get into the habit of thinking in anatomical order, just what I myself have been doing during the diagnosis of a case of tubercle. I need hardly say that in any case which presents doubts to your mind you should proceed further in the diagnosis by seeing the tubercle bacillus, which is difficult in chronic cases. I think you should have a proper report from the pathologist, who should be able to tell you that he has produced tubercle in some animal with the fluid which you have sent to him.

I have not time to-day to say much about syphilitic testicles, but there are two kinds of syphilitic testicles which may be a source of trouble to you. The first is that kind of syphilitic testicle which appears during the course of the general eruption of syphilis. I do not think that should be a source of trouble to you, because if the patient comes to you with a heavy, hard, round tumour of the scrotum, with but very slight inflammation or tenderness of the cord above, and perhaps on the other side a similar condition, you will naturally proceed to strip him and examine the whole of his body and

see if he did possess at that time any of the ordinary signs of general syphilitic eruption. You would see that he had roseola or papules, or syphilitic ulceration of his throat, or perhaps syphilitic alopecia. Perhaps you might have suspicions that he had syphilitic anæmia. You should be on your guard about such a case and put him upon anti-syphilitic treatment. If the treatment were efficient and carried out in the right kind of way, you would produce a startling result. Then there is the gummatous testicle, which appears in the later stages, what may be called the fourth stage of syphilis. The gummatous testicle may be very much more difficult to diagnose. I will tell you what happened to me some ten years ago, and I still remember it perfectly. An old labourer was brought into the Great Northern Hospital, and I was told he had malignant disease of his testicle. He had a huge soft tumour, evidently solid, in the right side of his scrotum ; it tapered off up the cord in quite the proper manner, and he had evidently some thickening of the cord ascending into his inguinal canal, and he clearly, for that malignant disease, wanted a serious operation performed. We debated the point as to whether it could be got away ; it seemed to ascend so far in his inguinal canal. I was told he had had some iodide of potassium and mercury, without any result. I remember operating upon him and removing this huge malignant testis, and when I cut into it I still thought it to be malignant. I did not see any

other syphilitic scars or gummata, such as are usual in these cases. But the case did not go on well afterwards; the wound suppurated and the scrotum began to ulcerate. And it did not ulcerate in the usual way, because it was clear that something preceded the ulceration; there was an inflammatory thickening of some sort, and it dawned upon my mind that I had removed a gummatous testicle. But the wound got well quickly when he had had what he should have had before, namely, full doses of mercury and iodide of potassium. Tumours of the testicle are very difficult to recognize and full of pitfalls, and therefore you should always be on the look-out for syphilis of the testicle, of the early or late kind, and if there is any possibility of it being syphilitic, you should give a strenuous course of treatment by iodide of potassium and mercury before you proceed to perform an operation. And under those circumstances these tumours not infrequently disappear in the most extraordinary manner.

But a greater pitfall than any is one which I now have to draw your attention to, and that is malignant disease of the testicle. I will not talk about sarcomas and carcinomas, which are refinements. It seems more rational that you and I should try to make up our minds whether it is malignant or not malignant before we try to label it as a hypothetical sarcoma or carcinoma. Because if you think of it you will admit that carcinoma and sarcoma are

absolutely unknown diseases. Nobody knows anything about sarcoma or carcinoma, whatever they may like to say. Nobody can tell you their cause, or where they came from, nor can they even tell you what course they are certain to pursue. They may guess what course they are likely to pursue. And therefore I think it is irrational to proceed with the mysteries of histology and so forth until our minds are capable of considering a clinical case and forming a fairly reasonable diagnosis as to whether it is malignant or not—a very difficult thing to do. I have brought down the notes of a case which was in our wards recently, and indeed the man is in there now. You cannot learn much from looking at him, I am afraid, but you can study his notes, and I have brought them here to see what is said about his previous history. Some of you are aware that I take a gloomy view of the truthfulness of histories, but I think we may take the broad facts of this case because they are so strange. He came in May, 1904, and is said to have had influenza. I dare say he had—many people have. After that he had a very much inflamed testicle on the left side. It was so acutely inflamed that it was treated with antimony and sulphate of magnesia mixture. I do not think it ever got better. He went on to quinine and iodide of potassium, and it was assumed to be an inflamed testicle which did not get well and was apt to recur. The testicle itself had a peculiarity; it had not descended properly into the

scrotum, it remained near the root of the penis. When I saw him we came to the conclusion that the tumour was ovoid, and the cord and vas deferens and spermatic artery and the strings could not be felt. The cord tapered off gradually, becoming thinner as it went up the inguinal canal. It led to the belief that whatever was in the cord had grown up, and not down. It was obviously tender and inflamed, and therefore one had to admit it might be inflamed. But what a source of fallacy that is! I hesitated the other day to remove a tumour from a man's neck. First of all we concluded the tumour was in the lymphatic glands, next that it was inflamed, next that it was adherent to the sternomastoid, but not much to the carotid sheath. All that was true. It was in the lymphatic glands, it was inflamed, it was adherent to the sternomastoid and slightly adherent to the carotid sheath. It was an inflamed epithelioma of the glands. As regards the tumour of the man with a partially descended testicle. First, it was solid, next it was going up into the abdomen, next it was inflamed, and yet I thought it ought also to be malignant. A portion of it was removed and examined microscopically, and it was found to be malignant, and I proceeded to remove it as high as I could, and I think I got it all away. What a trap to fall into that was! Is it true that these partially descended testes are more prone to malignant disease than other kinds? Once I should have said no. But

within the last year I have removed two. A man had the left testicle retained in the inguinal canal, and the right in the abdomen. The left one became inflamed, and was brought to me as an inflamed testicle. I said, 'Yes, but we had better wait and see.' And I said at the time, 'I am suspicious because it is a retained testicle, and they sometimes become malignant.' The man came back with it a little bigger and more painful, and exhibiting a little more anxiety. He probably derived the impression that it was malignant from one's look—human beings are very clever in this respect. I advised to have it explored and removed. It turned out to be malignant growth, and I removed it. That is the history. I remember a man was brought to me some years ago who was said to be suffering from a gouty inflammation of the testicle, and I examined him and came to the conclusion that he had a fluid swelling at one point. I advised that it should be tapped and some of the fluid withdrawn, so that we might see what it was. It might be tuberculous or an ordinary cyst. And out of it came blood when it was tapped. At the end of the trocar was adherent a piece of tissue, and this was sent to the pathologist, who reported that it was malignant disease. The pathologist was not informed where it came from. The testicle was removed and was found to contain one or two nodules of malignant disease. I have told you that I am suspicious

of all tumours about the testicle; they require a great deal of thought and observation.

I will conclude by telling you of another case of malignant disease. A young gentleman had had a loose cartilage removed from his knee, and that is what first of all brought him within my circle. Two years afterwards he came, looking exceedingly thin and ill, and having a nodule in his epididymis, which I thought was tuberculous. I was very anxious about that, because tubercle of the epididymis, as you will have observed, is very serious. I asked Sir Thomas Smith to see the patient with me, so that we could tell him what should be done. Sir Thomas Smith agreed in the diagnosis of tubercle, and thought it was clear and advised removal. It was done. The testicle contained a malignant growth in the epididymis, consisting of a peculiar kind of malignant adenoma, which I thought to be like a developing Wolffian body. The history was curious. I advised him to leave this country and go to a better climate, where he would have a better chance of fighting the disease. He went to another country, and five or six years afterwards, I am told by a relative, he had his right testicle removed for a similar malignant growth. There is one more strange thing about him. I know his mother died of carcinoma of the liver, his father died of carcinoma of the larynx or œsophagus, and an uncle died of carcinoma of the tongue. And in cases of carcinoma you not infre-

quently come across rather startling histories of malignant disease such as that.

I am quite aware that in my brief remarks upon the tumours of the scrotum, the neck of the scrotum, and the inguinal canal, I have omitted a great deal, especially hæmatocele, and you may observe that I have omitted to deal with, or have only half dealt with, tumours of the neck of the scrotum itself, but that is because I have not had time to go into them. But you have not missed much after all, because these tumours nearly always ascend or descend, and those which originate in the neck of the scrotum are quite rare. My memory tells me of a few cysts or fatty tumours or varicoceles which did not go far down or far up, and a few tuberculous lumps, but tumours which originate in the neck of the scrotum are relatively unusual. If any one does me the compliment of thinking over what I have said, they will come to the conclusion that I have been telling them to pursue methods of diagnosis, and not to try to remember cases. I have told you about a few cases, but they were to exemplify my own errors. And, after all, you cannot remember ordinary cases; you can only remember startling cases, such as those I have related.

XI

EXPLORATORY LAPAROTOMY, ESPECIALLY IN CASES OF MALIGNANT DISEASE

INASMUCH as you will often have to consider the advisability of abdominal exploration I propose to place before you some of the considerations which determine the decision.

I myself perform exploratory laparotomy with reluctance. The term 'exploratory laparotomy' should tell you why. An explorer does not go into regions which are known, but into those which are not; he goes to make discoveries. Now, for an operation to be an assured success it ought to be preceded by a correct diagnosis. Then the operator has a definite objective, nothing is left to chance, and the ultimate result can be foreseen.

But an exploratory operation is done for the purpose of making the diagnosis, and usually in the further hope of that diagnosis allowing of an attempt to cure, or to palliate.

Let us consider for a moment the alternative in which exploratory operation has enabled a diagnosis to be made, and has revealed the presence of malignant growth which cannot be removed. The

abdomen is closed and the patient recovers from the immediate dangers of the operation. Now the moment has come when the result has to be told. It is very painful to have to tell any one that life must inevitably be short. When all the blessings escaped from Pandora's fateful box the gods decreed that Hope should linger behind; and the hope which springs eternal leads many to submit to exploratory laparotomy in the hope that after all a curable disease may be discovered. In a little while I shall tell you of instances in which that hope has been happily fulfilled. But supposing an inevitably fatal form of malignant disease is discovered, then you have to decide what to tell. Fortunately, the patient is usually endowed with courage and fortitude, and requests to be told the truth. At other times it would be manifestly improper to impart bad news. For instance, exploratory laparotomy was performed and a tumour, which had been seen and felt, was found to be in the liver, and had the microscopical characters of a cylindrical-celled carcinoma. Inasmuch as the patient became exceedingly ill, with a rapid pulse, I advised that she should not be told. The evil tidings were withheld for some days and only imparted when it had become clear that the concealment was an additional unhappiness. I am told that when at last she knew, she became tranquil and resigned. This has made a deep impression upon my mind. At first it was right to withhold,

but afterwards that ceased to be the wise and courageous course.

But so strange is the human mind that there are some who do not wish to know. Not long since an exploratory laparotomy revealed a carcinoma of the great omentum. The patient never asked what had been found, or what had been done, and although some ascitic fluid re-formed, and she became much emaciated, yet I am told that she never again alluded to her complaint. Her friends and relations were duly informed. Sometimes it is imperative to tell the patient what is the matter. Business affairs may have to be settled or property disposed of. But the question I have raised is not in reality a very hard one. Although the task is a very painful one to undertake, I am sure that in by far the greater number of instances the patient has a right to know, and ought to be told. Infinite gentleness and tact are needed. At the moment you understand the meaning of the stereotyped phrase: 'the judge was visibly affected when he put on the black cap.'

There is yet another issue to be faced when the exploratory operation has brought to light a malignant growth which cannot be taken away. Very likely the growth has begun to lessen the strength and vitality of the patient, who, not unnaturally, blames the operation when the weakness and prostration increase. Indeed, you need not be surprised to hear that you are blamed for having done an unnecessary operation.

But against this gloomy picture must be set another. I explored the abdomen to ascertain the nature of a tumour beneath the upper part of the right rectus abdominis. For six months the patient had felt ill and lost flesh, but had merely a feeling of slight discomfort in the abdomen. Otherwise he had no symptoms to indicate disease of the stomach, liver, gall-bladder or intestines.

The tumour was in the pylorus and had invaded the liver. Some enlarged lymphatic glands were felt behind the lesser sac of the peritoneum. Six months after this operation the medical man under whose care he was wrote that, although very emaciated, he was not any worse off. He suffered no pain or discomfort and ate and slept well, and in fact, generally, was better than before the operation. Indeed, the patient had begun to doubt the correctness of the diagnosis. However, death took place three months after the letter was written—about nine months after the exploration. Such occurrences as these have made me very cautious how I venture to prognosticate the course which the malignant disease will pursue after an exploratory operation.

In another instance I was requested to perform exploratory laparotomy after unavailing attempts had been made to diagnose the cause of an accumulation of fluid—nearly always an ominous portent. When the fluid had escaped through the median incision a great quantity of malignant growth was

found scattered throughout the whole abdomen. An especially large mass was felt behind the stomach and transverse meso-colon. The place of origin of this growth was never known. But I remember well declining to guess how long the patient would live, and it was perhaps as well that I did so. After the operation the fluid did not re-accumulate for many months, and a comparatively active life was led. In the end, death ensued two years after the exploration. In another case of very extensive irremovable malignant disease beginning in the ovary the patient lived for a year after an exploration. Perhaps both were exceptional in their tenacity of life. Probably not more than from three to six months elapse between the exploration and fatal ending when malignant disease is found in the great omentum or other parts of the peritoneum.

And this leads me to remark that malignant disease of the great omentum is not an unusual cause of ascites. Cancer of the great omentum is one of the deepest mysteries of a mysterious disease. It is remarkable, too, what masses of growth the abdomen may hold without their presence being revealed to the sense of touch. At times the masses are hidden by the fluid, but at others they rest upon the intestines as upon an air-cushion, so that they merely retreat when pressed upon. This was clearly seen during an operation in which great lumps of cancer were found in the omentum, but could not

be felt until they had been supported by the fingers pushed behind them. At other times the mass is hidden by the inflated intestines or stomach.

In a proportion of cases exploration does good rather than harm, although nothing further can be done. The escape of the fluid relieves the heart and diaphragm. Indeed, the breathing and the circulation may be impeded to such an extent that the fluid must be got rid of. I advise that this be done by an incision rather than a puncture, for then a double end is gained—the fluid is evacuated, and the disease diagnosed and possibly cured. For instance, a lady had been thrice tapped for ascites. I guessed that she might after all be suffering from papillomatous growths of the ovaries and advised that next time the fluid accumulated it should be removed by incision, so that this diagnosis might be verified. Each ovary was a mass of papilloma and had a long and thin pedicle. The patient was quite well four years after the operation.

Some might think that in cases of ascites incision is more dangerous than puncture; but this I am inclined to doubt. I have been twice called upon to stop alarming hæmorrhage after puncture, and once I have seen the hæmorrhage speedily fatal. Sometimes the fluid does not reaccumulate after laparotomy, or there is a long pause before its reaccumulation. I can recall several cases in which it has not been necessary to do anything further for the removal of the fluid. Some may have

re-formed, but not enough to embarrass the heart or lungs.

Should exploratory laparotomy bring to light tuberculous peritonitis with fluid distension, the prospects of recovery are hopeful. But when the tuberculous peritonitis is of the dry kind, with a very hard abdomen, an exploratory laparotomy may do harm, and result in a fæcal leak. That, I need hardly say, is a very grave addition to the miseries of the disease. The tuberculous leak is, I believe, incurable and shortens life. However, this kind of tuberculous peritonitis can, as a rule, be diagnosed without an exploration.

It is important that you should clearly realize that abdominal distension is one of the most serious complications of exploratory laparotomy. When the distension is due to fluid the dangers are usually least. The fluid can be got rid of and the heart and lungs relieved, and the intestines enabled to propel their gaseous and fæcal contents. Gaseous distension of the intestines is more serious. The distension must be relieved. Sometimes this may be done by incisions into the bowels, which are sewn up again, at others by making an opening between the small intestine, the cæcum, or the sigmoid flexure and the outside of the body. The last is the best of these expedients, for the contents of ileum and cæcum are so liquid that they cannot be properly controlled, and the patient's life becomes almost unbearable. No one would resort to these

distasteful expedients if by any possibility they could be avoided.

When the abdomen is distended by a solid tumour, and when that solid tumour cannot be removed, a most dangerous condition is brought about. In all probability the heart, lungs, and intestines were much impeded before the operation, and after it is over the cause of their impediment still persists; and now the shock of the operation, the after-effects of the anæsthesia, and the injury to the abdominal wall, have been superadded. Last summer you may remember having seen me explore the abdomen of a young woman. It was distended by malignant tumours of both ovaries. In order to relieve the distension and prevent a speedy ending from intestinal obstruction, I deliberately removed the largest of the tumours. The hæmorrhage was, as I had foreseen, furious. However, it was controlled with clamps and gauze. The patient survived for nine weeks. I can recall another case in which the removal of a large malignant tumour of the ovary prolonged the patient's life for two years. An old lady had a very solid and immovable tumour filling the lower abdomen. When brought into view it looked malignant, and both Dr. Griffiths and I thought at first that it could not be removed. However, I faced the risk and succeeded. Two years later a growth appeared in the ischio-rectal fossa, became infected from the rectum, and led to septicæmia.

Obviously, we must proceed with extreme caution when confronted with solid abdominal tumours of a dubious nature. Remember, that when once the removal of the tumour has been begun it is safer to go on than to turn back. This is pre-eminently a moment when the qualities of the surgeon are revealed.

Let us now turn to the second alternative, that in which exploratory laparotomy is followed by a palliative operation. It would be easy to recall a number of instances of pyloric obstruction in which that most highly satisfactory operation gastro-enterostomy had brought about the most gratifying improvement. Pyloric obstruction of any kind is followed by wasting and loss of flesh, and the patient at last becomes so emaciated that cancer is supposed to be present. But happily, obstruction of the pylorus is not infrequently brought about by other and less mortal diseases than cancer, especially by the contraction and cicatrization which follow inflammation and ulceration. It may be exceedingly difficult to infer the cause of the pyloric obstruction from the clinical data, and doubt may still remain when the pylorus has been seen and felt. Some carcinomata constrict the alimentary tract as if it had been tightly encircled and tied with string. Growths of this kind have to be sought for with the microscope, for they cannot be possibly diagnosed with the unaided senses. They probably pursue an exceedingly slow

course, and cause death by mechanically occluding the alimentary tract. If this result be obviated by gastro-enterostomy, or by intestinal anastomosis, life may be prolonged for a space of time to be measured by years rather than by months. Even when the exploratory laparotomy has revealed an obvious tumour of the pylorus the palliative gastro-enterostomy affords great relief, and not infrequently leads the patient to suppose that he is quite well. Further, as you are aware, the presence of a tumour which can be seen and felt is not proof positive of the presence of a tumour with malignant attributes. When I lectured to you upon the early microscopical diagnosis of tumours,¹ an instance was given in which I had mistaken an inflammatory growth and thought that it was malignant. Provided a piece of the tumour can be excised, an immediate microscopical examination is the safeguard against such an error, and a freezing microtome and a microscope should form a part of the organization of nearly all exploratory operations. The discovery of secondary growths in the liver or lymphatic glands, or the extension of the growth into tissues other than those in which it arose, are also signs of deep significance. But, as you were told when I lectured upon growths in the urinary bladder, the muscular walls seem an obstacle to the extension of the malignant growth. So that, as a rule, those which begin with the stomach,

¹ *British Medical Journal*, July 2, 1904, p. 5.

intestine, gall-bladder, or urinary bladder, are slow in spreading.

This explains why I have seen a man in comparatively good health two years after I had performed gastro-enterostomy for a tumour which obstructed the pylorus, and why some who have had inguinal colotomy performed for carcinoma of the rectum live so long, and in reasonable comfort. Therefore, there is much to be said in favour of exploratory laparotomy when it is followed by a palliative operation such as gastro-enterostomy.

But after the palliative operation has been successfully achieved the future has to be faced. Should the disease be innocent, or should it be very slow in progress, then all is justified. But, obviously, such endings cannot be foretold.

At times an exploration reveals disease whose effects can be palliated for a time, but which must inevitably lead to a not far distant and painful ending. You may remember a young woman in Lucas Ward who had a tumour which could be felt in the right iliac fossa, and which was associated with some amount of intestinal obstruction. Unfortunately, when the tumour was exposed it had the naked-eye characters of colloid cancer of the end of the ileum, ileo-cæcal valve, cæcum, and right colon. In addition, the retro-peritoneal and pelvic lymphatic glands were greatly enlarged. Under these circumstances I anastomosed the hypertrophied ileum to the transverse colon. She made

a speedy recovery and was for a time better, but at the end of the seventh month I was informed that she was very ill, and dying in pain and misery. Distressing cases such as this have led some to question the propriety of performing these palliative operations. But personally I shrink from the responsibility of being judge and jury, and feel bound to do all that can be done to prolong life. Think of the alternative in this case. So far as could be foretold she was doomed to die within but a few weeks of intestinal obstruction—a truly horrible fate. After the operation she had at all events a period of comparative ease and comfort.

You must not infer that an intestinal anastomosis is necessary whenever an irremovable tumour of the intestine is encountered. An estimate has to be formed as to the chances of obstruction, and this requires considerable experience and judgement. The contents of the large intestine cannot be propelled through narrow channels, but those of the small intestine are so liquid that they pass through very small apertures. A lumen the size of a No. 10 catheter will suffice. For this reason I did not anastomose the small intestine of a young woman who had malignant growth upon the ileum and elsewhere, although the intestine looked much narrowed. I inquired afterwards whether intestinal obstruction had supervened, and was told that it had not.

Exploratory laparotomy has oftentimes to be

performed to ascertain the cause of jaundice, and in this class the exploration may be followed up by a further proceeding which is either curative or palliative. It would be very sad to allow any one to die of jaundice, caused by an impacted gall-stone. That can be extracted, and the cases, in spite of the jaundice, do exceedingly well. But when the jaundice is caused by malignant growth in the liver itself, or in the head of the pancreas, the outlook is forbidding in the extreme. But even here some relief can be afforded by establishing a biliary fistula between the gall-bladder and the exterior of the body, or by making a communication between the lumen of the gall-bladder and that of the intestine. I have tried both of these plans and am at present in favour of the formation of a fistula between the gall-bladder and the surface of the body. But, in spite of statistics served up for the credulous, I advise you to approach exploratory operations upon the deeply jaundiced with extreme circumspection, and after every other means of diagnosis has been exhausted. What would you think if one were to try and explain fatalities by such sillinesses as 'failure of the heart', 'delayed shock', and so forth? The children's phrase, 'died from want of breath,' would be quite as felicitous.

Exploratory laparotomy has oftentimes the effect of assuaging the pain of malignant disease. Pain is a very variable feature. It may be wanting or it may be excruciating. It is difficult to account for

its presence or absence. But I believe that it occurs when the growth is spreading into the parietal peritoneum. For instance, a patient had intense pain and tenderness beneath the upper end of the right rectus abdominis. At this spot a malignant tumour in the left lobe of the liver was found adherent to the abdominal wall by recent adhesions ; these were undone and the pain ceased. Or again, a very painful and tender spot to the left of the umbilicus marked the place where a malignant tumour of the transverse colon was adhering to the parietal peritoneum. As is well known, the parietal peritoneum is well supplied with sensory nerves, and is a very sensitive structure. On the other hand, the great omentum may contain quantities of growth and yet be painless. When the pain and tenderness of a malignant growth are accompanied by a nocturnal rise of temperature, and by an acceleration of the pulse, it is easy to assume that the disease is inflammatory, and perhaps that suppuration has occurred. But there is usually something to excite suspicion—unusual pallor, emaciation, languor, mental depression, rapid whitening of the hair, severe and long-continued pain, or a family history of cancer. I have operated upon a patient whose father, mother, and uncle died of cancer. But who can define clinical instinct ? Delicacy of perception and depth of intuition which reveal to gifted men secrets hidden from ordinary mortals ! The act of discrimination

is rendered difficult or impossible because the growth itself may become inflamed. I believe, myself, that inflammation caused the growth of the left lobe of the liver, and that of the colon, to adhere to the abdominal walls. But I am puzzled to tell how the operation relieves the pain. And this brings me to another point. Not infrequently, until the exploratory laparotomy, an inflammatory tumour with or without pus in it is pronounced to be a growth. In the present year a tumour in the right side of the abdomen, and which competent authorities said was a growth, proved to be a collection of pus behind the colon; though another beneath the right costal arch, and which I felt sure was a tumour of the liver, began to fluctuate and was emptied of pus. Under similar circumstances hydatids have been met with. Not long ago I found the region of the gall-bladder occupied by a mass which adhered to the liver, stomach, omentum, and transverse colon. Its removal was impracticable, but nevertheless the pain and vomiting were entirely relieved, and eighteen months afterwards the patient was considered to be very well.

Clearly exploratory laparotomy is not to be undertaken with a light heart. But when it has to be done take every possible precaution that forethought can devise. The preparation of the patient should not be a tax upon the strength. An early hour in the morning is best, and nothing should pass the lips lest vomiting ensue. The

patient's extremities should be warmly clad and the room should be warmed to about 70° F. The skin of the abdomen is disinfected whilst the anæsthetic is being given. The mixture of spirit and biniodide of mercury, which I introduced some years ago permits this to be done with considerable certainty. The operation should be carried through with celerity and with the smallest possible loss of blood. The bleeding is sure to be greater should the anæsthetist give ether. I prefer chloroform, or chloroform and ether, or the A.C.E. mixture. But when chloroform alone is given it is usually wise to ask for the administration of a little ether before the wound is closed lest unsecured blood-vessels be overlooked. When the abdomen contains malignant disease the slightest roughness or unskilful handling will start the blood oozing from numberless little points which cannot be secured, or blood may pour from the lacerated growth. Remember that the blood of soft malignant growths flows in channels devoid of muscular walls and which cannot contract and retract like healthy vessels. The blood, too, is often thin and watery and slow to clot. I shall never forget the jet of blood which sprang into the air when I touched an ovarian cancer with the tip of the finger. Fortunately, it was controlled with temporary gauze packing and did no harm. Therefore, to avoid feeling about within the abdomen a liberal incision is best. When fluid has been let out the slack abdominal walls can be lifted up,

and the inside of the abdomen looked at. The last of the fluid may be removed by gentle sponging with a soft sponge. The growth itself is not to be meddled with unless you decide upon an attempt at removal. In coming to this decision remember that there is no retreat. It is safer to go on than to stop. More than once when much vascular growth has been met with, I have merely ventured to touch it once to learn its consistence. But, if possible, a piece should be removed for microscopical examination. A stained section can be made there and then by an assistant who uses the freezing microtome. It is of the highest importance to see that every trace of bleeding has ceased before the abdomen is closed, which is done quickest by transfixing all the layers with silkworm gut sutures. Under ordinary circumstances the abdominal wound ought to be brought together layer by layer with buried sutures of twisted silk. But that is a slow proceeding, and the expenditure of time is not advisable in malignant disease. And here I would caution you not to hasten the removal of the sutures. The patient may be too ill and emaciated to heal as quickly as an ordinary person. Twice I have seen the wound come apart when the sutures have been cut too soon. Wait a fortnight, and begin by taking out alternate sutures, so that some remain until three weeks or a month have elapsed.

After the operation the danger of hæmorrhage must be borne in mind. Therefore stimulants,

such as brandy or strychnine, should not be officiously administered. I cannot tell you exactly how to proceed should bleeding occur. I prefer to look my enemy in the face, and therefore in such a case I give an anæsthetic and stop the bleeding with additional ligatures or sutures. The greatest care should be taken to avoid sepsis. A case which bled in the hospital became septic and died. I have purposely laid stress upon these pitfalls so that you may learn how to avoid them. As a matter of fact, cases of exploratory laparotomy do exceedingly well. The tendency is to do good rather than harm, even when much malignant disease is met with.

XII

FÆCAL LEAKS AND FISTULÆ

A HUMAN being whose intestinal contents are escaping through an artificial opening upon the surface of the body is a very unhappy person. He will submit to severe and dangerous operations to be rid of his trouble. My observation leads me to think that fæcal leaks and fæcal fistulas are commoner than they used to be. Perhaps that is because so many more incisions are made into the abdomen for the purposes of opening abscesses, and after an abscess within the abdomen—and especially after a neglected abscess within the abdomen—a ‘fæcal leak’ often ensues. I use the expressions ‘fæcal leak’ and ‘fæcal fistula’, but I do not know that they are quite correct. ‘Intestinal leak’ and ‘intestinal fistula’ would, on many occasions, be more correct, because that which comes out, and upon the nature of which the classification is obviously based, is not always fæcal. For instance, the fluid which escapes from the duodenum is certainly not fæcal, and the fluid which escapes from the jejunum is certainly not fæcal; it does not become fæcal until the

leak or fistula is in the ileum or in some part of the large intestine.

Before I proceed with the material which I propose to place before you, it is necessary that some kind of classification should be adopted. One might, as is one's custom, use an anatomical classification, and speak of duodenal leaks and fistulæ, of those in the jejunum, or in the ileum, or in the cæcum, or in the colon, the sigmoid, and so forth; but that, I think you would find, would lead to a great deal of repetition, and it would be impossible to condense what I have to say into the short time at my disposal. I therefore will proceed with the ordinary clinical classification of leaks and fistulæ into those which are congenital and those which are acquired, and under the last heading would be the two subdivisions of traumatic and pathological. The congenital or developmental leaks are not by any means common. Those which I have seen have usually been situated at the umbilicus. I can recall a case which might, by a little straining, be brought under this heading—that of a child who was born with an escape of fluid from an aperture at the umbilicus. This escape of fluid was certainly not, as far as I am aware, ever fæcal. It was a thin fluid, which caused some amount of inflammation of the skin of the abdomen around the umbilical opening. Various efforts were made to ascertain its nature. The fluid was examined by skilled people, and on the whole, I think, the opinions

weighed towards the belief that the fluid was urine and therefore came from the unobliterated urachus. Various attempts were made to obliterate this leak or channel. Anæsthetics were given and the channel was burned. I want to draw your attention very particularly to this point, because, perhaps, you may not have learnt that it is almost impossible to obliterate the whole of the mucous lining of any tract or duct. I believe it to be an impossible task, and therefore if that is true it is not surprising that I and others were absolutely unable to destroy the mucous lining of that from which this child was suffering, so that in the end a set operation had to be performed; and one of the first steps in the operation was to open the peritoneal cavity, and then it was easy to see what one had to deal with—namely, a Meckel's diverticulum passing down towards the ileum, not far from the cæcum. After this had been separated from the abdominal wall it looked exceedingly like an ordinary vermiform appendix, and there was no difficulty in removing it. Two or three vessels, one on either side of this duct, were tied—I suppose they were allantoic vessels—and then a cuff of peritoneum and muscular tissue was turned back as is done in the ordinary removal of an appendix. The child progressed after that operation exactly as any other child who had had its appendix removed during the latent period would have progressed; it made a good recovery. Some of you may remember

another very strange case. About two years ago a young woman was placed under my charge who had a curious history. First of all it was said that since she was an infant she had had a discharge from the umbilicus. She had had a great many operations performed upon this at various hospitals with the view of its relief. The operations cannot have been of a very sweeping character, because she presented herself with an aperture at the umbilicus, along which a probe would pass for some considerable distance, and which discharged a thin and slightly greenish fluid; but surrounding the aperture from which this fluid came was a very small amount of scar-tissue. Well, perhaps growing wiser by experience, I did not try any further attempts at closure of the aperture by burning, but proceeded with a formal operation. The incision at once led into a huge cyst filled with thin fluid full of cholesterin. The cyst extended from the diaphragm down to the depths of the pelvis, and it was a source of great wonder where the rest of the abdominal contents could be situated. A few coils of intestine were seen bulging behind this thin-walled cyst, but the liver, and spleen, and stomach, and so forth could not be discerned. I then attempted to separate this cyst, but speedily had to desist because of its close connexions with the intestines, and after considerable effort I ended by packing this extraordinary cyst full of sterilized gauze. The patient recovered, but was left at the

end of a year with a leak at the umbilicus. I should think the leak was bigger than when I began. There was no question about this leak being one which passed into the intestine, because a piece of rubber drainage-tube which had been placed in it disappeared from sight and was afterwards passed *per anum*; so there was no reasonable doubt in anybody's mind but that this was now an intestinal leak. I had to make another attempt to cure this patient of her loathsome condition. The fluid which came out, it is true, was not fæcal, but it was a fluid which inflamed the abdominal wall, and of course kept the patient in a continual state of great discomfort. On this second occasion I opened the abdomen and again found a cyst, but of a very much smaller extent, and after enormous labour I traced this cyst down to the pelvis, and ended by ligaturing what I believed to be the pedicle of an ovarian cyst growing from the right ovary; but, however this may be, the question of an intestinal leak remained. I believe that in removing the cyst I had again damaged the intestine. I certainly remember closing two apertures in the intestine which I had perhaps made myself; but in addition to that I ended by closing another aperture in the intestine rather higher up, towards the jejunum, and which I believe to have been the original one. This operation lasted three hours and forty minutes, and I think it was the longest and most arduous I ever did; but the young woman seemed to have

the most extraordinary tenacity of life, because she speedily recovered from my efforts, and the note says that she went away with the leak closed. She may have gone away with the leak closed, but I know that I saw her three or four months after that with a very minute leak, but it gave me the impression of being a leak which would get well, and it probably has got well with proper treatment.

I now come to another kind of intestinal leak or fistula, and that is the traumatic. I have already remarked that the traumatic leaks are commoner than formerly. I remember one of the earliest ones with which I had to deal occurred as follows. A patient was admitted into the Great Northern Central Hospital under my care with an enormous appendicular inflammatory tumour, and one which in all probability contained matter. It was clear that the matter would have to be sought for, and I began my incision near the margin of the tumour, which filled at least one third of the right side of the abdomen. The tumour felt exceedingly hard, and I would lay certain emphasis on this point, because I warn you to be very cautious how you begin to make incisions into very hard abdominal tumours. I had not proceeded far with my incision—I had passed through the aponeurotic and muscular layers of the abdominal wall—before I came upon hard inflammatory exudation almost half an inch thick. To find the pus I went on in my search through this, but it was not long before it was clear

that my incision had gone into some part of the intestinal canal—I thought at the time that it went into the cæcum. So I had to desist and close the opening. The next step in the operation was to search a little further from the edge of the swelling and open the general peritoneal cavity, where the intestines could be properly identified. And after this had been done I succeeded in lifting up the intestines from the iliac fossa, and, pushing my finger towards the back of the cæcum, I opened an appendicular abscess. It was drained and treated in the usual manner. As you might expect, an intestinal opening which was sutured whilst it was acutely inflamed, and whilst the intestine was embedded in lymph, was not very likely to hold. So in a few days' time the intestinal contents began to leak through the opening. The case has a certain bearing upon what would be your ordinary course of treatment of such an opening. Obviously haste was not reasonable. Clearly one ought to wait until all the inflammation had subsided, and until all the inflammatory thickening had disappeared, and until the intestinal walls had sufficiently recovered to hold the sutures properly. But after the lapse of about two months, I think—I speak from memory—the abdomen was opened. I am still astonished at what I then saw, because the enormous quantity of lymph and inflammatory effusion and of adhesions which had existed before had entirely disappeared, and it was then quite

easy to separate the cæcum from its slight attachments to the abdominal wall round the artificial opening, and close it in the usual manner with sutures, and not only to close it in the usual manner but finally.

Perhaps you may have seen another fæcal leak, which was traumatic, and which occurred in my wards. A girl was placed under my care who was supposed to have had a tuberculous kidney. In the region of the right kidney there was a mass which felt exactly like an inflamed and probably tuberculous kidney. Perhaps the kidney was tuberculous; at any rate, the urine contained pus, and it contained bacilli which were identified by the Pathological Department as tubercle bacilli. It was clearly necessary to proceed to the removal of the kidney, so I operated through the anterior incision, turned the right colon inwards, and after considerable labour extricated what I believed to be a pyonephrosis. There was a sac full of stinking pus, and surrounded by a mass of inflammatory material. The sac was united with a duct which I thought was the ureter, running down close to the course of the ureter and towards the pelvis. This was supposed to be a tuberculous ureter, and so it was traced down as far as possible and then divided in the usual way. The patient recovered from this operation, and I well remember the dresser coming to me a few days afterwards and asking me if I was aware that I had removed a loop of intestine.

It then flashed through my mind what had happened to this patient. I had removed an empyema of the vermiform appendix, and the tube which I followed down towards the pelvis was the appendix itself. The patient derived considerable but not complete relief from the operation. Some months afterwards she was again under my care with pain and a large inflammatory swelling on the right side of the abdomen, and as a quantity of inflammatory material had been left in the position of the right kidney I could not help assuming from her symptoms that suppuration was still in progress there. So I determined to explore the site of my former operation to see if there was any pus. But I had not gone far with the operation before a swelling which fluctuated, and which I assumed to be the abscess, bulged into the wound. Therefore I opened it and found that I had not opened the abscess, but had made an opening at the back of the right colon. The girl remained under my care with that traumatic fæcal leak for some time, and finally her leak became well and she went home. But whether it has remained well or not I am not sure. I am suspicious because I cannot help thinking that she is the victim of tubercle. And if it is true she has tuberculous inflammation proceeding in the vicinity of the fæcal leak, it is very likely that the colon will become tuberculous and need some further surgical procedure. I wish to refer again to another traumatic leak which also ought to be of very great

interest to you, and which ought to convey some information as regards the prognosis of these traumatic fæcal leaks. A patient was placed under my care for intestinal obstruction. The cause of the intestinal obstruction was an enormous mass of inflammatory material filling the interior of the pelvis. When the finger was passed into the vagina it felt as if the uterus was embedded in plaster-of-Paris. The finger passed into the rectum easily reached this inflammatory mass, but could not be passed further, because it was quite clear that the bowel was constricted or occluded by the inflammatory material outside it. I may say, in order to make a long story short, that a colleague, as I was unable to operate, performed the operation of inguinal colotomy. He made an artificial anus. An artificial anus differs from a fæcal leak or fæcal fistula in this respect, that an artificial anus is an opening from the intestine to the exterior of the body which is so arranged that all the contents of the intestine shall pass out at the opening, and none go beyond. The patient was relieved by this operation and disappeared for a year. She then came back with a curious condition, and one which I have hardly ever seen before. She had an extraordinary extrusion of the mucous lining of the lower part of the intestine out of the artificial anus, a projection four inches long, and with the lumen of the intestine at its apex. To relieve her of this great discomfort I cut that off, and without any bad result, and she

was relieved of that particular trouble. But I took that opportunity of examining the interior of her pelvis, and then found that the inflammation had subsided, and that the lumen of the intestine appeared to be completely restored. So afterwards I closed the artificial anus. There was no difficulty in the operation, and the first attempt was successful. She then disappeared from my ken, apparently very well indeed. But she came back about two years later complaining of great pain in the left side of the abdomen, and it was clear that her intestine was not then emptying itself properly. A further operation had to be performed. I guessed what had happened. At my first operation, in closing the artificial anus I had left the spur, which there is in all properly made cases of artificial anus, and it was this spur which was preventing the contents of the sigmoid flexure passing down into the rectum. I opened the abdomen, put a Murphy's button across the line of this artificial spur, and made a new passage from the top of the sigmoid flexure to the lower part. After that she was relieved and I have seen her no more.

On another occasion I was called upon to operate upon another patient with the same form of pelvic cellulitis and intestinal obstruction. Remembering the first one, I took care not to make an artificial anus, but to make a large fæcal fistula, so that when the patient had got over the intestinal obstruc-

tion, and when the pelvic inflammation had subsided and the rectum had become patent again, then the leak began to close, and when last I saw the patient the leak was only one-eighth of an inch in diameter, so I think I am right in assuming that it has got well.

There is another form of traumatic fistula which you occasionally see, and I show you a specimen of it. I remember helping Mr. Cripps to operate. It is a patient who had been operated upon for uterine fibroids, and who had afterwards had an attack of intestinal obstruction. An opening into the intestine gave complete relief, but as the opening did not close the patient was in a most uncomfortable and distressing condition; so efforts were made to close the leak by the ordinary process of suturing; they failed, and I remember afterwards assisting Mr. Cripps to perform enterectomy. He removed that piece of intestine by what was known as Maunsell's method. The patient soon got well and remained well.

Now, I want you to draw a moral from what I have been saying. I have been telling you, not about all the favourable cases of fistula, but I have told you about such as have come within my cognizance, and such as I have known and can remember, and you may have noticed that they were not difficult cases to deal with. Traumatic fistulæ tend to get well, and if they do not, traumatic cases are favourable ones for operation; but they are

favourable cases for several reasons, first of all as the kind of fistula is known and the kind of traumatism is known. But I will tell you why they are favourable cases to deal with : because you are dealing, or ought to be dealing, with healthy intestinal walls.

I now pass on to another kind of intestinal fistula or leak, and that is perhaps one which you may have seen most of. They are pathological leaks and fistulæ. Now, these are capable of being classified ; and first, I would mention the kind of leaks and fistulæ which are due to acute infective diseases—for instance, those which occur after, say, appendicular abscess. Next, I would refer to those which are due to chronic infective diseases. For instance, there are tuberculous or actinomycotic. Not long since we had one in the hospital in which bilharzia was the cause of the leak ; and, lastly, there are those which are due to carcinoma. This is a clinical lecture, and one of the objects of this clinical lecture is to make you think, and out of one's own experience try to convey to you ideas of what will happen in cases with which you will, perhaps, shortly be brought into contact. Now, I have placed these cases in the order of their curability. The cases which are due to acute infective diseases, get well, most of them recover, and if they do not get well they are more amenable to treatment ; but those which are due to chronic infective diseases, like tubercle, and like actinomycosis, bilharzia, or

cancer, either never get well at all or are most difficult to cure by operation.

Now with regard to the cases which are due to acute infective inflammations, I might, without very much exertion, detail to you a considerable number. Some time since, in April, a boy, who was, I think, in Harley Ward, had had an acute appendicular abscess opened. After the operation the discharge became fæcal, and after a while it ceased again: what I should myself have anticipated under the circumstances. I remember operating upon a Dutch gentleman, who had a very acute abscess with a gangrenous appendix. And I saw on the end of the ileum several dark patches which looked like gangrenous patches, and after the operation one could see this piece of ileum in his wound and a fæcal discharge coming from three or four small apertures in it. I remember his relatives and friends were greatly alarmed as to the ultimate result of this, but I felt sure, from my experience of these leaks due to acute infective disease, that they would get well, and they did. I have seen some enormous leaks into the cæcum and right colon get quite well when they have been caused by an acute infective disease. I can recall one patient who was under my care, and I should think the whole contents of the intestine came through the leak for many months. But she was assured she would get well, and ultimately she did, and has remained perfectly well. Have you observed

that we felt sure she would get well, although she looked exceedingly ill? The reason was that in spite of the most careful search and care, we did not know of any local condition, such as tubercle or actinomycosis or cancer, which would prevent that fistula healing.

As regards the chronic infective diseases, tubercle in particular, they are a dangerous and serious class of intestinal leaks. I can remember the first that I was associated with. I was asked by a medical colleague to explore the abdomen of a man to see if he had tuberculous peritonitis. The diagnosis was in some kind of doubt. I opened the abdomen and found the intestines and omentum densely matted together by tuberculous inflammation. Caseous material was seen, so the diagnosis was not in any doubt. I exercised the greatest caution in searching amidst these adhesions, but the wound afterwards suppurated. When you operate upon those who have tuberculous disease of the abdomen or of other regions, the wound often does suppurate. The ultimate result of the suppuration was the formation of an intestinal leak, from which the patient ultimately died. You might now say, 'What a dangerous thing laparotomy must be in cases with tuberculous peritonitis!' I think you would be right if you qualified your statement by saying, 'in cases of tuberculous peritonitis in which there is no fluid.' An operation on a case of tuberculous peritonitis in which the

abdomen contains a large quantity of fluid is a very successful operation indeed, and usually leads to the recovery of the patient. But an operation on a patient with an adhesive, plastic, hard kind of tuberculous peritonitis had better be avoided.

I will just mention one other case of fistula, or intestinal leak, associated with tubercle, because that again, I believe, conveys a warning. I remember seeing a young woman, who had a fluctuating swelling on the right side below the outer half of Poupart's ligament. Not only did it fluctuate below the ligament, but it was clear that some swelling could be felt in the iliac fossa. This abscess was opened, and at the time I remember warning the relatives that it was likely to be followed by a fæcal leak. My reason for that warning—I may be wrong—was that my experience of abscesses at the outer part of Poupart's ligament which extend towards the back of the cæcum is, that they are usually followed by a fæcal leak. I have seen several now, and I am always exceedingly suspicious of an abdominal abscess which tracks down the outer part of Scarpa's triangle. So this patient continued to leak for many months, and many attempts were made to close that leak. At last it became clear that something more would have to be done, and I made a final attempt by opening the abdomen and closing the opening in the cæcum. The patient afterwards had acute intestinal obstruction and died. Such calamities

teach us wisdom. Here I was quite right about the diagnosis of the disease ; I was right in foretelling the fæcal leak or fistula ; but I was totally ignorant of the kind of tubercle with which I had to deal. On December 3rd, Dr. Hartmann, of Paris, read an essay upon ileo-cæcal tuberculosis, and he showed a picture of a specimen which was the exact counterpart of the one which I unfortunately fell across. The patient in my case had this condition. The leak was at the back of the cæcum, and there was hyperplastic tuberculous inflammation of the cæcum and right colon. That had led to a stricture of the right colon. I was not aware of its presence, because my finger had not felt a stricture. I could not see it from the outside because the cæcum was involved in much inflammatory material, which I thought was part of the peritoneal inflammation which had been associated with the original tubercle. When I closed up the leak at the back of the cæcum I closed up the only aperture by which fæces could escape, and obviously the consequence was that the patient died of acute intestinal obstruction. The only chance she had was for my sutures to give way. I have not time to proceed further with the question of tubercle, except to say that you will find a very good essay on the subject of this form of tubercle by Mr. Stoney, in the *Lancet* of July 29th, 1905, p. 287.

Actinomycosis is another trap for the unwary.

I have known surgeons operate on inflammation of the right iliac fossa, and afterwards, when the inflammation has proceeded and when fæcal leaks have formed, it has dawned on them that they have had to deal with actinomycosis, and that diagnosis has been confirmed by the microscopical discovery of the mycelium or clubs of actinomycosis in those small yellow bodies. The case of bilharzia which was in our hospital, and which was diagnosed afterwards microscopically, came from South Africa. His fæcal leak was not seen at the time of the operation, but afterwards he had for a time, and perhaps has now, a leakage of fæcal contents from the end of the ileum. Thus, when you operate upon a patient who has a chronic disease of the intestinal wall, such as tubercle, actinomycosis, or bilharzia, you are likely afterwards to have to deal with a fæcal leak. And remember you may be blamed, unless you are wary, for the occurrence of that leak.

There is a last kind of fæcal leak which, I think, you will allow me to say a word about, and that is those which are associated with carcinoma. For these, too, in the beginning may be very deceptive. Four or five years ago a patient in Paget Ward had a small abscess of the abdominal wall. It had no very marked characteristics, and was merely a localized abscess pointing through the abdominal wall. It had one curious feature—it was resonant, it contained gases. It did not follow

that the gas had passed through a hole in the intestine, for abdominal abscesses often contain gas-forming bacteria. The abscess was opened, and was clearly associated with malignant disease. It was afterwards followed by a fæcal leak, with which the patient died. How easy it would have been for anybody after opening the abscess to have been blamed for causing the leak. I was asked by Mr. Marsh, some years ago, to see a man in his wards and open for him an abdominal abscess. The abdomen was opened and an abscess was discovered, bounded by adherent intestines and by adherent great omentum. There was nothing peculiar in that, but being of a suspicious disposition I sought for the cause of the abscess, and discovered after a while that there was a considerable mass of very hard growth. That man likewise had carcinoma of the intestine, and afterwards had a fæcal fistula or leak, and ultimately died. Now, obviously, if you are brought into contact with a case such as that, it behoves you to exercise the greatest caution.

Now I want to say a word or two before I cease about the examination of these cases of intestinal fistula, and with which, remember, all of you will have to deal. And, first of all, I would advise you to apply your minds a little to the kind of fluid which escapes. I will tell you why that is rather important. I do not know whether I am right, but I have an impression in my mind that intestinal

leaks which are near the stomach, in the duodenum, and in the upper part of the jejunum, are infinitely more difficult and more intractable than those which are lower down. In fact I am not sure that you might not say, 'the nearer the stomach the more difficult to cure.' I believe you can say, 'the nearer the anus the more easy to cure.' I believe it is almost true; at all events there is an element of truth in it.

I regret to say—and you may have observed it from my first remarks—I have not received very good assistance from the pathologists in the diagnosis of the situation of intestinal leaks. For instance, with regard to the child with a congenital leak at the umbilicus, the dispute was whether the discharge was urine or not? Two opinions led rather towards urine coming out. But recently I have seen a patient who has three intestinal leaks. I will not go into his history. He had had operations performed for them. And from two of the leaks there was still exuding obviously fæcal material, and from the other some fluid which looked to me more like duodenal or jejunal. In the first place it was very thin. Next, it was rather green in colour and it was curiously frothy. I do not know why it should have been so frothy, but it was; perhaps it contained bile. Next, it was alkaline, which led me to suppose it might have been recently mixed with the alkaline fluid from the pancreas. Next, it was strongly digestive, it

digested the abdominal wall. And it had not a very strong odour. So I suspect it was a leak high up, especially as when charcoal was given by the mouth it speedily came through. If that is true, then the leak will be very difficult to deal with. The two leaks which gave passage to fæcal fluid should be easier, for one of them has closed and the other is closing. I do not suppose this leakage higher up will get well without some formidable operation for its cure. So, clearly, it is important to look at the kind of discharge which comes out. Next, it is very important to look at the aperture. We had a little boy the other day in Kenton Ward who had had an appendix operation performed. It was complicated by hæmatoma and abscess, and then he was left with a small sinus which was surrounded by a ring of what might have been either granulations or mucous membrane. This was removed, and it was found on microscopical examination to be mucous membrane. I have already told you what bearing this mucous lining has upon the question of treatment. And so his abdomen was opened again, and it was found that the stump of his appendix had protruded through the centre of the wound and become fastened there. This was treated in the ordinary way and got well. So if you are brought into contact with a fæcal leak or fistula, make sure whether or not it is lined with mucous membrane. If it is, your prospects of curing it will not be very bright without a proper operation.

There is another thing of paramount importance, when you are brought into contact with a fæcal leak, which you should be dead certain about, and that is whether the alimentary tract is quite clear beyond the opening. You may lay it down as an axiom in surgery that no leak of any kind will get better if there is an obstruction in the canal. Whoever saw a perineal fistula get well until the stricture of the urethra was put right, or whoever saw a leak get well when there was a stricture of the intestine beyond the leak? So you have to make your minds clear on that point. I cannot now go into the manner in which you would settle this important point. But if you have a principle in your mind, matters of detail will surely settle themselves.

There is one other warning I give you about fæcal leaks. After what I have said you will take great pains to eliminate the possibility of some chronic infective disease being present. That is obvious. But go a step further and always examine persistent fæcal leaks under an anæsthetic, your object being to clear up the question of the presence of stricture or of chronic infective disease, and also to make sure that a foreign body has not been overlooked. I now have under my care a patient in whom silk had been left behind. I think he has two inches of continuous silk suture hidden away under the scar near the fæcal fistula. It is reasonable to suppose that it will not get well until that silk is got rid of.

I remember also a duodenal leak which was found to have a piece of stick in it, but it was not found until after the death of the patient. The patient had a perforation of the duodenum. And after his death from sepsis a piece of stick was found stuck in the perforation. I remember opening an abscess for a boy who had a fæcal leak, and it was clear that the cause was a bristle which was in the hole in the intestine. It was removed. Had that bristle been allowed to remain, the hole could not have got well. Another patient of mine that got well of a very bad fæcal leak had curious adventures. After a while a piece of thick silk, which someone had put in at a previous operation, made its appearance. That may have had something to do with the persistence of the fæcal leak; but a more extraordinary thing appeared afterwards, and that was a flint half an inch or more in diameter. And it has been a constant source of wonder to me how that flint got into the alimentary canal, and what it had to do with the persistence of the fæcal leak. I have never been able to solve the problem. I cannot now say more about the treatment of fæcal leaks and fistulæ, but there are a few common-sense things which I advise: first of all the patient must be kept still and recumbent. I say that because I have seen people with bad fæcal leaks who, it was thought, could get better when they were moving about in bath-chairs. Sometimes it may be wise to put the patient in a particular position,

especially if you find that the fæcal leak becomes less in a particular position. After that I think you should pay great attention to the patient's diet. There are many things which linger in the intestines and which come out through fæcal leaks and are better avoided, more especially vegetables and pips, and stones, and fibres, and so forth.

Lastly, I am sure it is not good to give aperients. I wonder how many fæcal leaks I have seen kept open by injudicious aperients given by the mouth. Enemata must be given with care, and the best way is to put olive oil in the bowel at night and wash it out carefully in the morning.

With regard to the operative treatment of fistulæ I shall not go further. About a year is a reasonable time to wait for the closure of intestinal leaks and fistulæ before attempting a set operation. Not infrequently the traumatic, and those which are due to acute infective diseases, can be cured by minor measures, such as by separating the intestine and suturing it with Lembert's suture, and then closing the wound in the abdominal wall. One not infrequently succeeds in favourable cases by such ordinary measures, but without doubt some want much more potent remedies. I have already shown you a specimen of a fæcal leak, which was cured by enterectomy, and I do not know whether you saw, during the month of October, a case of a woman who had a huge ventral hernia, with a bad fæcal fistula at its apex, and for whom

I performed a very large operation, removing the ventral hernia, together with the loop of the intestine attached to it, including the leak in it. After this the two ends of the ileum were closed and the proximal end was united by lateral anastomosis to the colon. The patient survived the operation and is now cured.

Clearly there is one other point to consider. It is hopeless to operate upon fæcal leaks which are associated with diseases such as tubercle, actinomycosis, or carcinoma, unless you remove the whole disease, and, of course, this may mean a very formidable surgical proceeding. Attempts to close fæcal leaks are most dangerous operations—dangerous because, whoever the surgeon is, there is one element outside his control, and that is sepsis.

XIII

THE IMMEDIATE MICROSCOPICAL DIAGNOSIS OF TUMOURS DURING THE COURSE OF OPERATIONS ¹

IN June, 1904, I gave a lecture at St. Bartholomew's Hospital entitled, 'The Early Microscopical Examination of Tumours.'² Perhaps this short title did not express my meaning as clearly as could be wished. It does not make clear that the early microscopical diagnosis is made whilst the patient is anæsthetized and ready for the operation to proceed, should that be necessary. Mr. Ernest Shaw is present to-night to demonstrate to you how easily and quickly a perfect section of a tumour can be placed under the microscope, and how certain that makes the diagnosis. A larger experience of the method has shown us how exceedingly valuable it is. Indeed, in my own surgical work it has become part of the organization of all operations in which any question of tumour diagnosis might possibly arise.

Many of the clinical rules laid down for the

¹ An address delivered to the Hampstead Division of the British Medical Association.

² *British Medical Journal*, July 2, 1904, p. 5.

diagnosis of tumours are rules for the diagnosis of tumours which have grown until almost beyond the possibility of scientific surgery. By scientific I mean certain—absolutely certain. At first a cancer of the breast is a small, hard, painless movable nodule, which no one can tell from a little fibroma, an adeno-fibroma, a little cyst, or an inflamed lobule. But left alone that little nodule soon—much sooner than is thought—spreads its cancer cells or juices far and wide into the lymphatic system.¹ An early microscopical examination—and no one can tell what that little tumour is, without one—puts all doubt at rest. If the tumour be harmless the operation is quite trifling; but if it be malignant an efficient one can be done.

The diagnosis of tumours rests upon (a) clinical, (b) naked eye, and (c) microscopical characters. Even when based upon such solid foundations as these it may be profoundly modified by the ultimate fate of the patient. Of late several occurrences have shaken my belief in even the axioms of clinical surgery. What would any one think if he were told that an ossifying enchondroma had been removed from the front of the left thigh bone by sawing and chiselling through the bony pedicle? I venture to opine that few would think that the patient would come back in six years with a large

¹ 'Carcinoma of the Breast and its Spread into the Lymphatics,' by C. B. Lockwood, *British Medical Journal*, January 27, 1906, p. 181. See Lecture VII, p. 122.

growth of the upper third of the femur and enlarged glands in Scarpa's triangle, and for which I had to amputate at the hip-joint. Such a fact as this makes me wonder what will be the fate of a boy who had a similar ossifying enchondroma of the lower end of the thigh bone taken away by chiselling through its bony stalk, and then, after less than a year had elapsed, another from the same spot. Had nothing been known about what happened to these patients after the first operation, I myself, being, perhaps, still imbued with some of the 'idols of the market and of the theatre', should have applied the term 'innocent' to their tumours, and theorized that they would not grow again. Nay, some would have gone further and said that if they did grow again they would not have grown into the lymphatic glands. I suspect that in these cases the capsule is the source of the fresh growth. If this be a correct guess, its bearing upon the method of removal is obvious.

The truth seems to be that the time has come when most of us ought to pass in review and revise our notions about tumours. Should our ancient beliefs prove to be right no harm will be done; faith will be fortified. But should our beliefs prove to be wrong, then it is to be hoped that suffering humanity may ultimately benefit by their recantation.

Now, as regards the clinical diagnosis of tumours. It is difficult for a clinical and pathological sceptic

to treat current beliefs with due reverence. My own faith has been shattered by many errors. A small, hard, movable nodule in the breast of a young woman has been diagnosed *secundum artem* to be a fibro-adenoma, and has proved to be a carcinoma; and I am far from being the only one who has been deceived. A hard, solid mass in the full breast of a florid, middle-aged woman was surely a carcinoma, but was in reality chronically-inflamed breast with a cyst in its centre. A cyst in the mammary gland, when cut into, looked like a thick-walled cyst with walls of fibrous tissue. Its walls were cancer. A large adherent tumour of the breast, with retraction and ulceration of the nipple and enlargement of the axillary glands, was thought by many to be inoperable carcinoma. It was tuberculosis of the breast and easily taken away. An encapsuled tumour in the breast of a middle-aged woman ought to have been innocent, to use the current phrase, but it was cancerous.

A small indurated nodule in the epididymis was diagnosed by surgeons of great experience to be tuberculous. It was a small malignant adenoma. Five years afterwards a tumour grew in the other testicle. A similar nodule met with in the epididymis, and which was associated with hydrocele of the tunica vaginalis, proved forthwith to be inflammatory, not tuberculous. But on another occasion an inflamed testicle was punctured to find out the nature of some fluid. None was found, but a piece

of tissue came away in the cannula. This was part of a malignant growth. To me this discovery was a complete surprise. The testicle was removed and the diagnosis fully confirmed. I can adduce two other instances in which a swelling of the testicle was thought to be inflammatory, and treated as such, and ultimately found to be sarcomatous. Oddly enough, both occurred in cases in which the testicles had not undergone complete transition. In one, the left testicle had stopped just outside the external abdominal ring; and in the other both testicles were retained, the right in the abdomen and the left in the inguinal canal. The latter was the one which I removed, but some months afterwards an abdominal tumour appeared and carried off the patient. It was supposed to have originated in the right retained testicle. I could continue to give instances of erroneous clinical diagnosis of tumours, but surely enough have been mentioned to explain my own mental attitude towards the ordinary clinical diagnosis. It is, however, to be noted that all the examples that have been given are ones of recognized difficulty. Fortunately there are instances in which the clinical diagnosis is unerring; as, for example, when we find a hard nodule in the epididymis, together with similar ones in the prostate and vesiculæ seminales; and perhaps pus and tubercle bacilli in the urine. But I have been speaking of tumours in which corroborative evidence is lacking, and the tumour has to be considered

alone, and upon its own merits or demerits. But how often there is no corroborative evidence !

Now as regards the naked-eye diagnosis of tumours after they have been cut into. Probably it is not wise to cut into tumours. Infective particles or juices may escape from them and bring about a further growth in the severed tissues. At all events the way in which nodules of cancer sometimes appear in the scar and in the stitch-holes after a cancer has been cut into suggests that this is possible. Therefore it is better that whenever possible the suspicious tumour should be removed whole, and then cut into and examined. Inasmuch as tumours are now, I am glad to say, often examined before they have had time to grow big and dangerous, they can oftener be taken away whole with an abundant margin. And now as to the value of the naked-eye examination of the exterior of the tumour and of its cut surface. Some years ago I made an error which sank deeply into my mind. A large and rapidly-growing tumour of the testicle was cut into and then removed because it was thought to be malignant. Soon afterwards fresh growth appeared in the wound, and ulcerated in the manner characteristic of gumma. Iodide of potassium brought about a speedy cure ; very likely it would have cured the original tumour. Again, a surgeon, who was a good pathologist, cut into a tumour growing in the subcutaneous tissue of the arm. To the naked eye it might have been a sarcoma

or an inflammatory swelling. On the whole, it was most like a sarcoma. Excision of the tumour and even amputation of the arm were talked about, but the wound was closed pending a microscopical examination. Ultimately the tumour sloughed, and the slough was exactly like a piece of wet washleather. Obviously it, too, was a gumma.

Many intra-abdominal tumours cannot be diagnosed with the naked eye. I have mentioned elsewhere a growth which implicated the intestine, and was thought to be malignant in spite of a microscopical section which had shown that it was inflammatory. Then I had a lingering confidence in my powers of making a naked-eye diagnosis of tumours. But the microscope was vindicated when the tumour departed and left no trace of its presence, for the patient ultimately died of septicæmia derived from a small abscess along the right ureter. On another occasion a large tumour of the great omentum had the naked-eye appearances of malignant growth, but the microscope revealed nothing but inflammatory deposit. The omentum was removed, otherwise the case might have got included amongst abdominal tumours, supposed to be malignant, but which disappear after laparotomy. The naked-eye diagnosis of such abdominal tumours unsupported by microscopical evidence is mere food for the credulous. Again, tumours which to the naked eye seem to be inflammatory are oftentimes malignant. Gall-stones were removed from a very thick

gall-bladder. The thickening was assumed to be the result of inflammation, but a piece was cut off for microscopical examination. The thickening was, as is not by any means rare, caused by cancerous growth. A second operation was performed and the gall-bladder excised. This brought to light a strange thing. At the first operation the cystic duct had been very thoroughly explored with the finger. Nevertheless, at the second operation it had a large gall-stone in it. However, I have almost ceased to wonder at anything that occurs in this mysterious region, in spite of the infallibility with which its diseases are diagnosed, and the astounding certainty with which they are said to be cured.

Clearly the patient with the thick gall-bladder might have been spared a second operation had I taken, as I do now, the precaution to have an immediate microscopical examination. In another instance this was done, and disclosed that a projecting piece of a curious mass near a slightly distended gall-bladder was inflammatory; although the tumour itself was very hard and did not fluctuate. Encouraged by the result of the microscopical examination, a further search was made, and a little pus was found in its interior. Two years afterwards the patient was well. Recently a gall-bladder which looked malignant was forthwith shown to be quite innocent.

In other regions the naked-eye examination of a tumour may lead one to suppose that it is inflam-

matory when, as a matter of fact, it is not. For instance, a tumour at the inner side of the left nipple was cut into, and some cysts brought into view, and then a little pus. The pathologist, who was in attendance, looking at the cut surface of the tumour, and having also seen the pus, without hesitation said the tumour was inflammatory. It will be agreed that on ordinary clinical grounds he had ample justification. But the immediate microscopical examination brought to light a carcinoma—a so-called duct carcinoma.

Many enlargements of the lymphatic glands cannot be diagnosed by ordinary clinical methods. Fortunately, they are usually situated in accessible places, and can easily be removed for immediate microscopical examination. This method has brought to light things which to me have oftentimes been rather surprising. I know that the philosophical historian has said that we wonder because we are ignorant, but I also know that the recognition of ignorance is the beginning of knowledge.

A dustman had enlargement of the cervical glands which was thought to be tuberculous, but was removed and contained squamous-celled carcinoma. The glands had probably been infected from an ulcer in the pharynx. Another man was supposed to be suffering from lymphoma (I confess that that term conjures up no clear image in my mind), but the microscope showed that the enlargement was myxosarcoma. On the other hand,

enlarged lymphatic glands in a child's neck which were thought sarcomatous proved to be tuberculous, and were removed on both sides with a reasonable prospect of success.

The naked-eye and also the microscopical diagnosis of enlarged glands is sometimes exceedingly difficult, perhaps impossible. A labourer had enlarged glands along both carotid sheaths. There was much difference of opinion when the microscopical sections were looked at as to whether the appearances were those of inflammation or of malignant disease. Acting upon my own opinion I removed the glands, for I thought they were inflamed, which proved to be the case. The cause of the inflammation or the kind of inflammation was never known. But the case shows how important it is that the operator should himself be able to interpret for himself the appearances seen in microscopical sections, for otherwise the operation would, on this and other occasions, have been abandoned.

Another case illustrated very well the difficulties by which the clinical diagnosis of enlarged lymphatic glands is enmeshed. A man had a mass of enlarged and obviously inflamed lymphatic glands beneath the right sterno-cleido-mastoideus. One of the largest of the glands fluctuated and was presumably suppurating. When exposed they looked like inflamed glands, and the fluid which escaped looked like pus; but the microscope brought to light

squamous-celled carcinoma. A clean sweep was made of the growth, but it speedily grew again. Fortunately the efforts of surgery are not always so unavailing.

The immediate microscopical examination is applicable to dubious ulcerations of mucous membranes. An ulcer of the cervix uteri was shown to be carcinomatous, and a growth which appeared in the roof of the vagina after the whole uterus had been removed was also carcinoma, and singularly like the growth which was removed from the cervix in the other case. It had originated in the cervix, and it is to be inferred that, although the whole uterus had been removed, a particle of growth had been left behind and had gone on growing. Some might be contented to say that the growth had 'recurred', whatever that may mean; but to me it seems better to face the fact that when the tumour grows again it has not been effectually removed. Ulcers of the tongue often present difficulties. A small, hard, circular ulcer of one of the foliate papillæ was removed and found to be inflammatory; and yet another with similar characters was epitheliomatous. A papilloma of the penis was epitheliomatous, although no growth could be felt in its base. This microscopical diagnosis enabled amputation to be done at an early stage of the disease.

The immediate microscopical examination of tumours and of tissues has another very important

use. It has fallen to my lot to operate for the removal of some very large connective-tissue tumours. A cartilaginous tumour of the scapula weighed more than twelve pounds after about a pint of fluid had run out of a cavity in its interior ; but within nine months a second operation had to be done for growths in the remains of the scapular muscles and in the axillary glands. Now, these large chondromata of the scapula and pelvis usually contain sarcomatous elements, and we may surmise that these latter had grown along the muscles and fascial planes. Now, the extent of these prolongations cannot be seen with the naked eye. So, in another similar case, besides removing the suspected muscles very widely as I went along, I gave a scrap of every doubtful tissue to Mr. Shaw, who at once reported upon it. Thus one had reasonable certainty at each step of this very extensive operation. In removing numerous and extensive epitheliomatous glands from the neck, too, it was of essential service to know what was being removed, because in addition to the growth there was much inflammatory thickening. What is the meaning of this association of inflammation with carcinoma ? Or, again, the front half of both upper jaws had to be removed for epithelioma. This had grown for an indefinite distance into the upper lip. After the lip had been separated from the tumour, a piece of its deeper part was examined microscopically. It had some carcinoma in it, so a larger piece of lip was cut

away. This case exemplified another point. The cancer had grown along the palate, and it was quite impossible to tell with the naked eye where it ended. Some tissue in the roof of the mouth looked exactly like growth, and most of the palate would undoubtedly have been removed had not Mr. Rose made an immediate microscopical section, and reported that we were looking at palatal glands.

Again, an immediate microscopical examination was useful when amputating at the hip-joint for a tumour of the upper third of the femur. In Scarpa's triangle, by the side of the tumour, some enlarged lymphatic glands were felt. Others could be felt along the crural arch and within the abdomen along the external iliac vessels. The question, therefore, arose as to whether, at the time of the amputation or afterwards, these intra-abdominal glands ought or ought not to be removed. The microscope there and then showed that only the large glands upon the capsule of the tumour contained growth, and that those nearer the crural arch contained none, so that it was deemed unnecessary to open the abdomen.

Mr. Shaw will now demonstrate how quickly sections of growth can be cut with a freezing microtome, and how clear and good the sections are. In my opinion the apparatus he is using is an essential part of the equipment of an operating theatre.

DEMONSTRATION BY ERNEST H. SHAW

Method of Cutting Frozen Sections of Fresh Tissues for Immediate Microscopic Diagnosis during Operations.

The method may be divided into two parts :

1. *The Arranging and Fixing-up of the Apparatus Required.*

The microtome must be fixed on a firm table, and all the instruments arranged in a convenient manner. A mental survey of the cutting, mounting, and staining of a section is then made, in order to make sure that everything is present and in its proper place. This ensures that no time will be wasted when once the process is begun.

2. *Preparation of the Microscopic Section.*

(a) The selected piece of tissue received from the surgeon is placed directly on to the brass disc of an ether-freezing microtome, and is surrounded by gum solution.

(b) The tissue and gum are frozen, and sections made by a razor on a carrier.

(c) The sections are transferred to a dish of cold water, and, after separating them with a glass rod, a suitable section is lifted out.

(d) It is dipped for a moment into pure methylated spirit, and

(e) Then placed in another larger dish of cold

water; the currents set up by the spirit in the water cause the section to spread out flat.

(*f*) A glass slide is dipped in the water under the section, and the latter is lifted out as the slide is slowly drawn up out of the water again.

(*g*) The water is drained off the slide, and a drop or two of stain (Löffler's methylene blue) is allowed to fall directly on to the section.

(*h*) A thin cover-glass is placed on the stain and section after three to five seconds; it is lightly pressed down so as to drive out excess of stain; this is then blotted off, and the specimen is ready for examination under the microscope.

XIV

CLINICAL PATHOLOGY IN ITS RELATION TO DIAGNOSIS AND TREATMENT

AT the present time I see proceeding in my wards and around me many things which are of great interest. Of late years clinical pathology has grown and evolved until it has become almost a dominant factor in the diagnosis of medical and surgical cases. I can remember when I first began to work at that most important branch of clinical pathology, bacteriology, I used infusions of cucumber; then I became a pupil of Dr. Klein, to whom I always feel a deep debt of gratitude, and he taught me potato cultures, broth, and solid culture media, and initiated me into the mysteries of anthrax. Afterwards I can remember that in conjunction with my late colleague, Dr. Vincent D. Harris, we instituted a class for the teaching of bacteriology, and I also remember the very firm resistance with which my proposals were received. Now the evolution has so far proceeded that at my hospital we have a very strong pathological department, with my friend and colleague, Dr. Andrewes, at its head; Dr. Thursfield as Senior Demonstrator; Mr. Rose, a Bedford man, is just giving up the post of Demonstrator, and I

cannot say how much I am indebted to them for the care and trouble which they invariably take in performing investigations which are so often of the most laborious kind. This indispensable work, done for me by the pathological department and by others, places at my disposal a mass of material which is certainly interesting to myself ; and, perhaps, some of it may prove interesting to you. This material I had to classify, and I take a step which is rather unusual, for I am going to classify the material not according to the diseases, but according to the organisms which were separated from the patients.

BACILLUS OF DIPHTHERIA

The bacillus of diphtheria is, of course, very familiar to every one. I suppose there is no one now who, if he is called to a suspicious inflammation of the throat, would not have a culture made and have that culture examined. And if he found that the bacillus of diphtheria was present, he would then proceed to treat that case with therapeutic serum. But my point is not the discovery of diphtheria bacillus in the human throat, but the fact that by the systematic use of culture methods we have found the diphtheria bacillus so often under conditions where we did not expect to see it.

Mr. Burfield, my late house-surgeon, has described a small group of cases in the *St. Bartholomew's Hospital Reports*, mainly from the clinical and bac-

teriological point of view. But they have for me an additional psychological interest, because I remember very well when I first saw a child with inflammation and suppuration of its scalp inquiring what was the matter with it. I was told that it was suffering from cellulitis of the scalp. I am certain that to every one the fact of having applied a name to the condition sufficed. Names are not diseases. I requested the pathologists to give me within a short time the kind of information about that inflammation of the scalp that every one here would require to have concerning an inflamed and suspicious throat. The report came back that the pus had in it a micrococcus which doubtless came from the hair follicles, a streptococcus, and the diphtheria bacillus. The child was treated afterwards with therapeutic serum and recovered, after having had paralysis of various groups of muscles. Shortly after that another child was brought to my notice. When I inquired what was the matter with it, I was told that it had suffered from vulvitis, a name which was supposed to signify a disease. Here again the pathological department reported to us that this child had diphtheria of the vulva. So it was treated with therapeutic serum and ultimately recovered.

A rather interesting thing happened after that case. One of the nursing staff had had an open sore for many months, and had had many kinds of treatment applied to it. Cultures again brought the diphtheria bacillus to light. I can recollect the diphtheria bacillus being found under other circumstances.

A man under my care had an empyema opened, and it was clear that the case instead of progressing, was going from bad to worse. The first step, of course, was to discover what the infection in the empyema cavity was. I remember that we had three or four pathologists to work at that case. At last, I think it was my friend, Dr. Horder, who succeeded in separating the bacillus of diphtheria from the pus. I regret to say that case was not fortunate, because the man died. His original infection was actinomycotic.

Anybody would say, I think, that an ordinary abscess and inflammation of the breast was reasonably straightforward and was not likely to reveal any hidden mysteries. But when the pathological department begins systematically to work at the pus from even such a simple matter as an abscess of the breast, curious things come to light, and I observe amongst my notes that one of the patients who had abscess of the breast had the diphtheria bacillus in the pus. So that if you cease to think that you can diagnose these infective conditions on clinical evidence, and begin systematically to investigate them by modern scientific methods, I am sure you will meet with many surprises.

STREPTOCOCCUS PYOGENES

I will now refer to some of the cases in which the *Streptococcus pyogenes* has been brought to light. A rather lamentable fatality has recently happened. A patient came in who was supposed to have a disease

of the stomach. I suppose that her stomach was thought to be at fault because she was continuously vomiting. In the right side of the abdomen, not far from the region of the pylorus, a movable tumour could be felt, which of course helped, in a slight degree, to corroborate the possibility of a growth having closed the pylorus. She had no marked urinary symptoms. The notes merely say that the urine contained a trace of albumin and a few pus cells. The usual steps were taken to endeavour to make a diagnosis, because, although I have not said so, I am saturated with the belief that the most difficult step in any case is the preliminary diagnosis. When the preliminary diagnosis is made—perhaps it may be a clinical one, or a clinical one plus a scientific one, or the two combined—the treatment is easy. In this instance the stomach contents were examined and skiagrams made, but ultimately an exploratory operation had to be performed, and it was found that the tumour on the right side of the abdomen was an inflamed kidney with a quantity of pus in its pelvis. It was removed, and the patient recovered from that operation, but not completely. However, she was sufficiently well to depart to the country. When she left she had a small sinus in the right iliac fossa, due to hæmatoma along the course of the ureter. Cultures inoculated from the sinus were sterile, and she left us fairly well, but with this sinus in her iliac fossa. After a few weeks she returned very ill indeed. Her temperature was 104° , pulse

112, respirations 28. Both her legs were œdematous, and the left knee was swollen and inflamed. After she had been kept under observation for a few days she seemed to improve, and we thought the fever had subsided. However, she had a relapse, and her temperature and pulse again rose. Then a culture was made from her blood. Of late the pathological department has been infinitely more successful than I can ever remember to have been myself in obtaining growths in cases of acute streptococcus poisoning, influenza poisoning, and pneumococcus poisoning. They reported that she had streptococci in her blood ; she was treated with serums without avail, and she died. Now comes an interesting point in this case. I requested them to ascertain where she got her streptococcus poisoning from, because she had none when she left us. We knew that, because we had taken cultures. It was then ascertained that she had slept in the next room to a patient who had died of puerperal septicæmia and had been attended by the same nurse.

Another very unfortunate circumstance happened about the time that woman died. I had operated upon a man and removed his prostate. He did quite well after the operation, and I should have felt sure that at the end of three weeks he was going to recover ; but one day his temperature suddenly rose to 103.2° F. At the same time he had a rigor ; his temperature remained high ; he had another rigor, and on the third day the pathological department reported that

he had got streptococcus infection; and, although he was treated with serum, I regret to say he died of streptococcus poisoning. When I thought he was convalescent his pulse was 80; when the attack began it went up to 96, next day to 104, and even to 124, and then varied from 120 to 125. He improved for a while after therapeutic serum, and his pulse became 116, but afterwards accelerated to 134, and then he died. One of the most ominous signs in septic poisoning is that acceleration of the pulse.

I will not tell you of any more cases of streptococcus poisoning, except of one which is very interesting because it occurred in the case of a colleague of mine. On the Thursday he performed a post mortem examination on a case of septic peritonitis, and made cultures from it. He pricked his finger at the post mortem examination. On Friday he had a rigor, fainted, and became exceedingly ill. He knew what he had got, because he had been looking at the cultures that morning and had seen *Streptococcus pyogenes* growing in them. He knew he had a virulent form of the poison. He at once got anti-streptococcic serum and was dosed with it, and took an anæsthetic and had the finger attended to. He had a most perilous illness, and I think he would have died if he had not made such a prompt scientific diagnosis of his own case.

PNEUMOCOCCUS

I remember very well being requested to see a child who six weeks before had been seized with a sudden and violent fever. The temperature had run up to an extreme height, as had also the pulse. Then it became clear that pus had formed in the right side of the chest, and a surgeon was called in. He opened an empyema on the right side of the chest, but the patient did not seem to be improved very much by this proceeding, and next it became clear that pus had formed on the left side, and an empyema was opened there. However, the child, at the end of six weeks, was as ill as ever—temperature and pulse were very high, respirations rapid, and he was desperately ill. At that stage I saw him with a view of trying to diagnose the condition. My chain of reasoning—being somewhat of a disciple of Hobbes, I try always to have a chain of reasoning, though I think the great Hobbes calls it a chain of thought—in this particular instance was, first, when this child became ill it had got a severe form of infection. The next step in the reasoning was that this was an infection of the blood. Two considerations pointed to that. How could anything else but an infection of the blood infect first of all the right pleura and then the left? A further reason was that the child had an endocardial murmur. Further, it seemed likely that it was suffering from the infection of the blood with which it began, and I guessed that the organism was either a pneumo-

coccus or a streptococcus. The next step was to ask a pathologist to make a culture from the blood. Afterwards Dr. Horder informed me that he had separated the pneumococcus from the blood. That child had been ill for six weeks. If in any case of septic poisoning serum is not given until after the bacterium is lodged in every crevice of the body, it is of no avail. Such remedies ought to be given at the earliest possible moment.

STAPHYLOCOCCUS PYOGENES AUREUS

Staphylococcus pyogenes aureus is met with frequently. In my wards, with sixty or seventy patients under my care, I am sure to see three or four women with abscess of the mammary gland. I get weary of seeing these unfortunate people week after week in the hospital, and continually having fresh abscesses opened. You can readily believe that I am heartily glad to seize upon any possible chance of cutting short their stay in the hospital. It must obviously be bad to have in the hospital people who are pouring out *Staphylococcus aureus* capable of infecting everything within their radius. Here is an ordinary case of a young woman of twenty-two. It is a gruesome history. On February 7th, after the birth of a child, her nipples were cracked. Immediately afterwards there was an abscess which burst in three places. A month later another abscess was opened in the surgery. Three weeks later the abscess was

reopened, and two tubes put in. On April 6th, a week later, the abscess opened again. A fortnight later another incision was made, and she was injected with a vaccine of *Staphylococcus aureus*. That is the point I am coming to. I have been much struck with the fact that when an abscess of the breast is bacteriologically diagnosed, and found to contain *Staphylococcus aureus*—a severe form of infection—the disease is certainly shortened by vaccination. I am not an enthusiast for vaccination, but I have been struck with the fact that the duration of these cases is materially shortened by vaccination with *Staphylococcus aureus*. The method is simple: probably you are more familiar with it than I, but when the abscess is to be opened, a culture, as a matter of routine, is taken, and the infection is diagnosed. As a rule it is *Staphylococcus aureus*. That is grown in broth for twenty-four hours. The broth culture is then sterilized by heat, and we inject into the arm or some cellular tissue, two-thirds of a cubic centimetre of that culture. That quantity is chosen because it is found by experience to be a safe and proper dose. It is said by Mr. Blakeway, to whom I am infinitely obliged, to contain 600 millions of cocci. Others have observed that the vaccinated cases get better quicker than those not vaccinated. Side by side with one of my patients was another who had not been vaccinated, and the difference in the result was so obvious that they are now going to vaccinate the other patient.

I could give other cases of *Staphylococcus aureus* infection of the breast, but I trust I have said enough to make clear what I think about that method of treatment. I also have a case under observation of a man who had an abscess of the thigh. It was opened and cultures were made. It was proved to be due to *Staphylococcus aureus*. The abscess was very slow in getting well, and the man is at the present moment in the hospital with a sinus, but we are convinced that he did not make satisfactory progress towards recovery until he was vaccinated with the cultures of *Staphylococcus pyogenes aureus*. The immediate results of these vaccinations are very striking. After the vaccination the temperature may undergo very considerable rises, and likewise the pulse; in this man's particular case it produced a marked leucocytosis. Mr. Ball has been working with great zest at this man's opsonic index, which is raised in a remarkable manner after the vaccination.

There are many who think that the diagnosis of an abscess of the breast is an easy matter, but not if you are going to diagnose the kind of infection. Here are notes of a case of acutely inflamed breast sent up from the country. The inflamed breast fluctuated and might have been taken for an ordinary mammary abscess. But when the pus was let out, a portion of the indurated tissue was excised and found to be tuberculous. We have been steadily working at the treatment of tubercle by Sir A. E. Wright's method of vaccination. Tubercle generally

comes to the surgeon after it has reached a certain stage of its progress. The inflammation of the tuberculous gland of the neck, for example, has proceeded as far as caseation, and perhaps as far as suppuration. It is incomprehensible to me that any method of vaccination could remove caseous glands from the human neck. I can believe that it may prevent the spread of the tubercle from one gland to another, or prevent it from spreading in the synovial membrane of the knee-joint. On the whole, the treatment of tubercle by vaccination has been, from my point of view, disappointing, probably for the reason I have given.

COLON BACILLUS

Now I will proceed to another bacterium which our pathological department is continually discovering, the colon bacillus. I have learnt to look upon it as one of the most dreadful scourges of the human being. Cases of colon bacillus cystitis are detected when they are looked for, but it is a curious and deceptive disease, and is, I am sure, often overlooked. For instance, a gentleman had a small operation performed, and came afterwards with a very acute orchitis on one side. On inquiry it was quite clear that he had been exceedingly ill, he had had a rigor, high temperature, accelerated pulse, and was passing water frequently day and night. When the urine was looked at it contained flakes of lymph and pus. The pathologist reported that it was teeming with

colon bacillus. That was supposed to be an attack of influenza, the bladder symptoms being caused by influenza or by the inflammation of the testicle. How easy it is, when you come to think about it, in such a case as that to confuse the cause and effect. Not long ago a man had increased frequency of micturition. I observed that his bladder was tender on pressure. On rectal examination the prostate and the neck of the bladder were exceedingly tender. When asked to pass urine into a glass I was almost deceived, for the urine was perfectly clear and betrayed no signs of inflammation of the genito-urinary tract. But it was sent for bacteriological examination, and the report came back that it was teeming with colon bacillus.

Acute colon bacillus cystitis is a dangerous disease. I have seen a man with a pulse of 110, delirious, with violent pain in the lower part of the abdomen and the neck of the bladder, and passing water with great frequency. It is an infection that may spread up the ureters and cause ascending pyelonephritis. Colon bacillus cystitis, unless properly treated by means of urinary antiseptics and perhaps by vaccination, may go on indefinitely. I have notes of a patient who was under my care in the year 1893. She was treated for a very long time—two years off and on—for cystitis. Five years afterwards the right kidney was explored to see if it contained a stone. She again came under my care at the end of last year, and we found colon bacillus in the urine. We managed, I think, to make

that patient a great deal better. It is very difficult to draw any conclusions from the clinical evidence. It does not follow that because a particular kind of treatment makes a patient better, therefore the patient got better because of the treatment. Still I cannot help thinking that that patient made an improvement, such as she never made before, after she had been vaccinated with colon bacillus.

THE EARLY DIAGNOSIS OF CANCER BY IMMEDIATE HISTOLOGICAL SECTIONS

I will not trouble you any more with bacteriology and the curious groups of bacteria we have met with, but I would like to mention one other direction in which the pathologists have been of most essential service to surgeons. It is now my habit whenever I remove tumours to have a pathologist present with a freezing microtome, and have sections made there and then as I proceed. The most extraordinary things have been brought to light. I am quite sceptical that anybody can diagnose early carcinoma of the breast from a small adenoma or little cyst. These tumours can at once be diagnosed, if you remove the tumour and have it there and then examined microscopically. I saw a patient a little while ago who had had carcinoma of the left breast removed. Two years afterwards a small lump appeared on the inner side of the right breast. She naturally thought her old enemy had returned. The tumour was felt, and if I had been asked to guess what it was I should have

said it was a cyst, but I knew that she had a carcinoma removed on the opposite side, and I was cautious. The tumour was cut out and it was a cyst, but a piece of it put under the microscope showed that the wall of the cyst contained carcinoma. After removal the mammary gland was examined, and other patches of carcinoma were found in it two inches beyond the original growth. We cannot diagnose tumours with the naked eye. Here is an illustration of that essential fact. A lady had an operation performed for the removal of a tumour of the left breast. It was supposed to be a chronic mammary tumour. The major portion of the mammary gland was removed, and possibly some of the axillary glands. Then the tumour was examined and found to be a carcinoma. But if you come to think, a breast which has one piece of carcinoma may have others; and if one lymphatic gland has cancer in it, there be may a great deal more. Six months after, a tumour appeared in the right breast, and the same surgeon who had operated for the supposed chronic mammary tumour performed a sweeping operation. It seems so natural to assume that if a person has had carcinoma in the left breast, the tumour which appeared in the other side was of the same description. After the sweeping operation, it was found that the tumour was a chronic mammary tumour. Had these tumours been microscopically examined there and then such errors would not have been fallen into.

XV

SALIVARY CALCULI

I LECTURE to-day upon salivary calculi. I have chosen that subject because it may not form part of the systematic lectures in surgery, and also because it is very inadequately written upon in the books on surgery at my disposal, and therefore, probably, in your own textbooks. Perhaps the reason is that salivary calculus is not a very common disease. I have not seen many cases in my lifetime. Perhaps, also, it is because salivary calculus is not dangerous, or perhaps because it is not very painful. However, it is a disorder which causes the patient extreme discomfort, and salivary calculi are very easily overlooked, and it will not redound much to your credit if you overlook a salivary calculus which is afterwards found by somebody else. I am afraid salivary calculus is not a subject which can be of extreme interest to the egoist; it is not sufficiently abstruse or incomprehensible. I have here some specimens of salivary calculi for you to look at. There is not much to be said about their physical characters. They are white, and rough, so that they are easily felt if a probe can be introduced into the duct or gland which contains them. Next, they

are very hard, so that they can easily be felt in the ducts if you know how to feel for them. Curious statements are made concerning their size. There is a calculus in one of those bottles which looks to me one and a half inch long, and quite one and a half inch in circumference, and must have considerable weight. It is stated in some book which I have read, that a calculus one and a half inch long was removed from the sub-maxillary duct, and that the calculus was as broad as the little finger. Unfortunately, that does not convey very much to my mind, because I do not know how thick that gentleman's little finger was. Another calculus was said to have been as large as a pigeon's egg. Well, it is so long since I kept pigeons that that does not convey much to my mind either. I mention this point to warn you, gentlemen, that we ought to be more scientific, and when we speak of the size of things we ought not to compare them to little fingers or pigeon's eggs, but express them in terms of the foot rule or the metre. As regards the chemical composition of these calculi, it is said that they consist of phosphate and carbonate of lime, with magnesia, and perhaps with a certain amount of organic material. I merely mention that because the calculi which are found in another very large salivary gland other than the sub-maxillary or parotid have a similar composition. I refer to the calculi which are found in the pancreas. The origin of salivary calculi seems to be quite unknown. A

great deal of loose language has been used in speaking of their origin. Even gout has been invoked. But I hardly think that gouty people are more likely to have salivary calculi than others, and, as a matter of fact, one of the patients of whom I shall presently speak to you was a little girl ten years old. I did not think she had acquired gout. Further, some other patients were strong and healthy young gentlemen, in whom there was no suspicion of gout. I suspect, myself, that salivary calculi may have some sort of bacterial origin. I do not know the composition of the tartar upon the teeth, but that undoubtedly has a bacterial origin and I suspect that the salivary calculi arise in much the same way. I mention this point because some one may desire to find some subject to write a thesis upon. Saliva is easy to obtain, and the ingenious might possibly precipitate some calculous material from it. There is one thing which I am inclined to think has something to do with the formation of salivary calculi, and that is the presence of a nucleus. There is in one of these bottles a calculus in section, and in the centre of this calculus there is a minute brown nucleus. And you will observe that there are concentric rings of this white phosphate and carbonate of lime round the little nucleus. From the history of that specimen which is given in the museum catalogue I gather that the calculus took a good many years to form in the substance of the sub-maxillary gland. These salivary calculi are

met with in the ducts of the sub-maxillary and of the parotid gland, and, it is said, in the duct of the sub-lingual gland, but I never myself met with calculi in the duct of the sub-lingual gland. It is also said that they are more frequent on the left side of the body. Three males, of whom I am about to speak, had their calculi in the left sub-maxillary duct. One female had her calculus in the right sub-maxillary duct, and others in the right sub-maxillary gland, whilst one female, a little girl, had her calculus in the left parotid duct. It is strange, but that lends some support to the idea which seems to prevail, that they are more common on the left side of the body than on the right.

Now I propose to proceed to discuss the clinical effects which are brought about by the presence of such calculi as those we are looking at, first those in the sub-maxillary duct and afterwards those in the sub-maxillary gland. The patient will not come complaining that he has a calculus, but that he has discomfort in his neck beneath the jaw. And further, he is almost sure to say that he sometimes has a swelling there. Perhaps he may have a swelling at the moment you see him, but more often he will come complaining of the pain and discomfort and an occasional swelling. I have had what I think is a unique opportunity of seeing the early stages of the swelling of which the patient complains, because I remember one day a gentleman telling me that he thought he would have to

come to see me because he had some discomfort in his neck, and that a swelling appeared there when he was eating. I happened to sit opposite to him at dinner shortly after that, and I actually saw that after he commenced his dinner a considerable swelling appeared beneath the left side of the body of his jaw, in the region of the sub-maxillary gland. The floor of his mouth was examined, and then a small salivary calculus was felt slipping about in the left sub-maxillary duct. Calculus in the sub-maxillary duct is very easily overlooked and may not be felt at all unless you know how to feel. There is not a very good anatomical specimen here of the sub-maxillary gland and duct, but I think this one will suffice to show you the point to which I am about to refer. If you begin at the sub-maxillary gland itself, you will see it lies in the sub-maxillary fossa, beneath the body of the lower jaw, and beneath the mylo-hyoid muscle. From its under and posterior edge its duct arises. That duct is of some size ; I should guess that it is well over one-eighth of an inch in diameter. Its walls are thin and distensible. I think you will agree with me that the duct is decidedly bigger just at its origin at the sub-maxillary gland, where it looks as if it had a dilatation, very much like the pelvis of the ureter. After thus beginning, the sub-maxillary duct makes a sharp bend round the edge of the mylo-hyoid muscle to run along the floor of the mouth to end at the sub-lingual papilla. Salivary calculi are

often met with just at the bend where the duct turns round the posterior edge of the mylo-hyoid muscle, and if you feel for these calculi by putting the finger into the floor of the mouth and pressing upon the duct, they slip down into that dilatation of the sub-maxillary duct, and you fail to feel them. Or they may actually slip down into the sub-maxillary gland itself, and so elude your sense of touch. Therefore it is clear from what I have said that you have to feel with proper precautions. I do not know whether there is anybody here who remembers my explaining how to feel for carcinomatous enlargement of the glands in the floor of the mouth in a case of cancer of the tongue. I advised you, if you were going to feel the floor of the mouth, to place the patient in a chair facing you. Next insert a gag, if one be necessary, to prevent the patient from biting your fingers; and finally, put one finger outside the floor of the mouth and another inside. You may not feel a calculus at once, but by pressing the fingers together you will find the calculus slipping about.

So much for the symptoms of which the patient complains, and so much for that which you may be able to feel. But if the patient has had a salivary calculus for any length of time in the sub-maxillary duct it will cause inflammatory changes to take place, so that when you look at the sub-lingual papilla through which the mouth of the duct opens, you will see it swollen, perhaps red. Next, the

sub-maxillary duct runs in the floor of the mouth beneath a ridge of mucous membrane, and you may see that mucous membrane a little swollen and perhaps red and œdematous. Sometimes the inflammation in the floor of the mouth is so considerable as to have spread to the side of the tongue. All these things would make you very suspicious, and lead you to investigate properly with your fingers. I have been describing a case in which the little calculus was slipping about in the sub-maxillary duct, close to the posterior edge of the mylo-hyoid. Now I am going to tell you of another in which a calculus three-quarters of an inch long and half an inch in diameter, was stuck in the anterior end of the sub-maxillary duct, just beneath the sub-lingual papilla. Now observe how a case of that sort may deceive. It was sent to me because the patient was supposed to have an epitheliomatous ulcer growing in the floor of the mouth, and surely it was very like one. There was the ulcer, there was the surrounding inflammation, and there, underneath the jaw, was a lump which would very reasonably correspond to the carcinomatous glands which would be enlarged in association with an epitheliomatous ulcer in the floor of the mouth. There were one or two points about the case which led me to suspect that it was not really epithelioma. There was a history of a very long period of discomfort in the floor of the mouth. Next, the lump was vivid red, and looked to my eyes like an inflammatory lump.

And the ulcer was small and seemed accurately located to the opening of the sub-maxillary duct. When a probe was passed into the floor of the ulcer a calculus was felt. This made the case perfectly clear. The glands under the jaw were large and slightly tender, and there was inflammation of the sub-maxillary gland itself.

Now I wish to tell you of a false step which I took in this particular instance. I will tell you it now, before I forget to. The patient was exceedingly anxious not to have a general anæsthetic, and that I should endeavour to remove the calculus under cocaine anæsthesia, and, with the clear understanding that I might fail, the attempt was made. As soon as I cut into this inflamed mass to try to extract the calculus, the mouth became full of blood, and as soon as he was aware of this bleeding he became alarmed and shut his mouth. So that attempt was a fiasco. In the end a general anæsthetic was given and the calculus was extracted.

Here is a case which illustrates another point. It should warn you not to be too positive as to the curative effects of operations. A gentleman came with the usual history of salivary calculus, and one was found slipping about in the left sub-maxillary duct. It was removed, and one would have thought that would have been the end of the case. However, two years afterwards he came back with two calculi slipping about in the same duct and I endeavoured to extract them. After he had been

under the anæsthetic from half to three-quarters of an hour, and after I had, with infinite trouble, tried to open the sub-maxillary duct and get these calculi to slip out, I was conscious that they had disappeared, and I could not find them any more. I do not know where they are to this day. Obviously one operation may not be curative, and if calculi are there, even a person who has taken every possible precaution and does not minimize the matter, may fail to extract the calculus and have it in his possession to show the patient. I have often wondered what happened to those calculi. And I can tell you something else which is of interest, because the calculi may arise not in the duct itself, but in the gland itself, and it is within the range of possibility that calculi which slip out of your ken have actually disappeared into the gland whence they came. For example, here is a specimen of a calculus situated inside the duct just at its origin from the gland. Some time ago I saw a young woman who had in the neck a great enlargement of the sub-maxillary gland. The usual question, 'Is this a cancer or a gland?' at once arose. It certainly felt exceedingly hard, and it also seemed exceedingly suspicious. However, on examination, a salivary calculus was felt in her sub-maxillary duct, in the usual place, close to the posterior edge of the mylo-hyoid muscle. The gland felt so intensely hard, and it was so unlikely that it would ever return to its proper size, that I made up my

mind that the best policy to pursue was the removal of the sub-maxillary gland. Therefore I removed that gland and the calculus. When we came to cut into the sub-maxillary gland itself we found that it contained a great many small calculi ; it was full of grit. So I have very good reason for saying that in some of the cases salivary calculi arise in the substance of the gland. Acting upon this knowledge I advised the gentleman whose two calculi I failed to produce that if he suffered from pain or inconvenience from his calculi he should have the sub-maxillary gland removed. Of course, that would finally and completely cure him.

I am now going to speak of the method of removing these sub-maxillary calculi. I am sure to have disabused your minds of the idea that it is an easy operation. The patient must have a general anæsthetic and ought to be very carefully prepared. As regards the position of the patient during anæsthesia, I have found that it is easiest to proceed with the patient sitting up in a chair, and the operator has one finger outside the mouth and another inside, so as to have better command and control. Next, it is exceedingly important to have an efficient gag on the opposite side, and an assistant to pull out the tongue. I generally put a temporary silk suture through the tongue so that it can be well pulled out during the operation. If you have the gag thoroughly well placed and the tongue well under command, you proceed

to cut down on the calculus in the floor of the mouth and get it to slip out of the duct. It is most difficult to fix the calculus. It is no use pushing down—the calculus slips away into the dilated part of the duct. I have found that by getting the assistant to push with his thumb underneath the angle of the jaw, so as to push the sub-maxillary gland and duct up and fix the floor of the mouth, I have been able to steady the calculus itself against the body of the lower jaw, and then a cut has been made in the duct and the calculus has slipped out. I have already told you of a case in which the attempt failed. After the operation a little gauze packing is placed in the cut in the floor of the mouth. The patient suffers very little inconvenience and is soon well. Should it be desirable to remove the sub-maxillary gland, that could hardly be classed among the difficult operations of surgery. If you will glance at this anatomical specimen you will agree with me that a semilunar incision through the skin, platysma, and deep fascia of the neck, division of the facial vein and facial artery and sub-maxillary duct, will speedily enable you to extract that gland from the human body.

Before I cease speaking of the calculi in the sub-maxillary duct, I want to say a word about the clinical effects which are produced by calculus in the duct or in the gland. Those produced by calculus in the duct or in the gland are produced

in exactly the same manner as by the presence of a foreign body there. I remember a farmer coming up from Gloucestershire with an enlargement of his right sub-maxillary gland. The gland was very large, and it felt very hard and was tender. The usual question arose: had he a cancer growing in his sub-maxillary gland? On looking inside the floor of the mouth one observed that the sub-lingual papilla on the same side was very red and swollen, and also the ridge of mucous membrane along the sub-maxillary duct, and it was also stated that the swelling was not always the same size—it seemed to get larger when he ate. He said he was going about his farm with some oats in his mouth, and he asked me if it was possible that an oat could have got into his sub-maxillary duct. I made the usual remark, that it was possible but highly improbable. However, I received a letter about six months afterwards in which it was stated that an oat had actually come out of his sub-maxillary duct. You can take these various statements for what you think they are worth; I can only tell you what I saw, and do not think a man, such as he was, could have had any reason for pretending that he had an oat in his sub-maxillary duct.

Before concluding I propose to tell you about a case of calculus in the duct of the parotid gland. Disabuse your minds of the impression that the diagnosis of these calculi is at all easy. You will have to exercise considerable care and

some skill if you are going to find them at all. A little girl, *æt.* 10 years, came to the out-patient department with a swelling of her left parotid region. There was no question that she had a swelling there, because it could be seen, but none could be felt. Various curious things were said about that swelling. It was said that it varied in size ; that it became a little bigger when she stooped down ; and that pressure made it disappear. Altogether, it was very odd and curious. The child was under observation for many months. She was shown at consultations, and the most diverse opinions were given upon the nature of the swelling. On the whole, most thought it was a cystic hygroma. Certainly the clinical symptoms made it more likely to be that than anything else. Various operations were suggested, but I was averse to making a cut upon the face of a little girl ten years of age to cure a cyst, the existence of which I was not absolutely certain about. I did not think it fair to do a speculative or exploratory operation under such conditions, and it was perfectly clear that this swelling, whatever it might be, was not growing rapidly and was not endangering the child's life. After this girl had been to consultations she went back to the ward, and then the dresser made a discovery. He wrote down that he felt a calculus slipping about in the parotid duct. That was the truth. She had been examined over and over again by surgeons and assistant-surgeons, and by the

house-surgeon, but none of them could feel this calculus, and then at last the dresser discovered it. I suppose the explanation of that was very much the same as the explanation of the difficulty of feeling these calculi in the sub-maxillary duct : it slipped back into some dilatation in Stenson's duct, or even into the parotid gland itself, where it could not be felt. The removal of this small parotid calculus had then to be undertaken. It never occurred to me that I had before me an easy task. The child was anæsthetized, and the gag adjusted, and then one began to feel for the calculus in Stenson's duct. For a long time it could not be felt at all. At last, by squeezing the parotid gland rather hard, it was felt to slip forward. Then it disappeared again, and at last, after a great deal of manœuvring and trouble, it was fixed in the parotid duct. Acting upon the little piece of knowledge which I had acquired by my attempts to remove calculi from the sub-maxillary duct, I tried to fix it against something, and I found that it could be fixed against the coronoid process of the lower jaw. When it was fixed there I managed, with a curved knife, to cut down upon it, and it slid, almost by chance, into the mouth. After the removal of the calculus the swelling got well.

XVI

THE WIND AFTER ABDOMINAL OPERATIONS

THIS unconventional title has been chosen for this scientific subject because no other term gives such a vivid notion of the condition I am about to describe. Shock, reaction, traumatic fever, are the sequel of all major operations. In addition to these, abdominal are usually followed by vomiting, and almost invariably by wind. The signs of the onset of an attack of the wind are perfectly clear and straightforward. (1) The non-passage of wind by the natural channel. (2) The accumulation of wind in the intestines. (3) The onset of vomiting due to obstruction.

I begin by trying to make clear the source of the intestinal gases, and afterwards the reasons for their accumulation.

Owing to the kindness of Mr. Ball I am able to show these culture tubes. The first is one of broth which has been inoculated with the colon bacillus. As you are aware, the colon bacillus is a constant inhabitant of the alimentary tract. It may be found from the beginning to the end, but the largest numbers are present in the ileum and colon. A small glass capsule has been placed in the broth, and

although the inoculation of the broth with colon bacillus was only made twelve hours ago, a large bubble of gas has already collected at the top of the glass capsule.

Here is a second tube of colon bacillus growing for twelve hours in gelatine. Innumerable small colonies have already appeared throughout the gelatine, and each one is crowned with a little bubble of gas. These bubbles are so numerous that the gelatine looks as if it were in a state of effervescence.

Finally, here is a third tube of colon bacillus grown for twenty-four hours in gelatine. In it the bubbles of gas are much larger, and one near the surface, where the pressure is least, looks about one-third of an inch in circumference. So we may infer that under suitable conditions of food, warmth, and moisture, the colon bacillus produces gas in prodigious quantities, and with great rapidity.

Having seen a small number of colon bacilli acting upon a small quantity of nutritious material and producing therefrom large quantities of gas, now imagine the effect of countless billions of colon bacilli at work upon the foods within the alimentary tract.

Obviously the production of gas is inevitable. It is, however, to some extent within our control. There are other gas-forming intestinal bacteria.

An anærobic group, of which the bacillus *ærogenes capsulatus* is one of the best known, is an abundant gas producer. When I made inquiries on this point

in the Pathological Department, I was told that it was especially active in milk ; a remark of deeper significance than perhaps the utterer knew.

The production of gas within the intestines is mainly dependent upon two main factors—the quantity and quality of the intestinal bacteria and the quantity and quality of the intestinal contents. Both of these must obviously depend to a considerable extent upon that which is introduced into the alimentary tract.

It is unnecessary to point out that bacteria of various kinds abound in some articles of diet, and are absent from others, likewise that the quantity and quality of the intestinal contents must depend in a large measure upon the diet.

As you are aware, and this is a point of extreme importance, any stagnation of the intestinal contents, or any obstruction to their onward passage, fulfils the conditions which are necessary for the abundant production of gas.

One who has a partial intestinal obstruction usually undergoes untold miseries from what he calls 'the wind'.

On the whole a fluid diet affords the most favourable nutrient material for the intestinal bacteria to work upon and produces abundant gas.

The application of the above to the preparation of patients for abdominal operations, or indeed for all operations, hardly needs to be pointed out.

For several days before, the diet should be simple

and well cooked, and not too abundant—experience shows that some things are more apt to remain in the intestines than others, especially ill-cooked vegetables, stalk of cabbage or of cauliflower, Brussels sprouts, uncooked fruit, tough and fibrous meat, and such like.

Next, it is clear that the quantity of the intestinal bacteria and the amount of the intestinal contents can be diminished by the judicious use of aperients. There is no aperient so useful for this purpose as castor oil. It is probably the only drug which efficiently clears the large intestine. It is a great misfortune that this safe and admirable aperient should be so nauseous. I have often asked Sir Lauder Brunton for an aperient which could be introduced hypodermically and act like castor oil.

When patients have been under observation in bed for days or weeks I have observed that they usually bear abdominal operations exceedingly well, and afterwards suffer very little from the wind. Of course there are many reasons for this, but perhaps one of the chief is that for some time their diet has been carefully regulated, and there has been time to thoroughly clear out the alimentary tract. One who dines at his club in the evening and undergoes an operation in the morning is in grave peril.

My own observations have led me to believe that the intestinal gases are produced in the greatest abundance in the ileum. They are also produced in the large intestine, but to a less extent.

Whilst performing gastro-enterostomy it is unusual to find much gas in the upper part of the jejunum. The stomach sometimes has a little in it, but not in large quantities, except when its interior is septic. Gas which has collected in the ileum easily passes upwards into the jejunum, but I doubt whether, except under very unusual conditions, it passes into the stomach—doubtless it does so in the later stages of intestinal obstruction.

The gas produced in the ileum is continuously passing through the ileo-cæcal valve.

In appendicitis, when the ileo-cæcal junction is inflamed, the passage of wind through the ileo-cæcal valve is obstructed and is attended with pain. The usual remark made by the patient as it passes on is that the wind was felt moving, and hurt as it 'turned the corner'—you will learn to look upon the wind 'turning the corner' as one of the favourable signs after appendicectomy.

In the absence of a stricture or other mechanical obstruction, gas can usually be got out of the large intestine by means of enemata. Handling, bruising, or inflammation of the large intestine are not followed by very formidable obstruction to the onward passage of gas. After nephrotomy, nephrectomy, and nephrorraphy, all of them operations involving disturbance—and perhaps bruising of the right or left colon—we do not dread an attack of the wind.

The intestinal gases are mainly driven onwards by the muscular contractions of the small intestine,

and to a less degree by the contractions of the large. Obviously therefore it is of great importance to watch the peristaltic movements. When the abdominal walls are thin the movements of the small intestine can be seen. Also they may be felt both by the patient and by the surgeon. As they move the gas within them can be heard squeaking and gurgling so that the peristaltic movements can be heard. The presence of peristaltic movement is a clinical sign of intense significance.

When the peritoneal covering of the intestine is inflamed, as in cases of general septic peritonitis, the intestines cease to move. The abdomen becomes silent.

Presently I shall explain how, when wind has been allowed to accumulate, the crowded intestinal coils stop one another's movements and the abdomen is silent. In intestinal obstruction due to mechanical causes, the muscular walls of the intestines become exhausted and cease to move, bringing about a similar silence.

The stethoscope is most helpful in abdominal cases, and oftentime gives valuable information.

Matters are becoming desperate when the intestinal movements cannot be heard.

The conditions which interfere with the propulsion of the intestinal contents, and which allow, therefore, the accumulation of intestinal gases, are to be looked for in the intestines, in the abdominal wall, and in the vascular and nervous system. First of all it must be

clearly understood that the intestines cannot move unless they have sufficient space to move in. Thus the other day one of our patients had a violent and almost fatal attack of the wind, and which was foreseen at the time of the operation, because her abdomen was of small capacity with exceedingly thick strong muscular abdominal walls. Obviously under such circumstances as these there would be no room whatever for the intestines to expand. Distended coils would of necessity speedily compress one another and their peristaltic movements come to an end. I intend to refer to this case again.

Next the presence of a large abdominal tumour may bring about the same result. When laparotomy is performed and a large abdominal tumour brought to light, it is justifiable to incur grave risks in order to remove the tumour rather than leave it behind. Should it remain to diminish the capacity of the abdomen, an alarming and perhaps fatal attack of wind is almost certain to ensue.

Thus it is that after the removal of large abdominal tumours the wind is not a serious anxiety.

Towards the end of last year I performed hysterectomy upon a woman between 50 and 60 years of age, under most unfavourable circumstances. She had a very large thyroid tumour which fortunately did not impede her respiration. The pulse was rapid and very irregular, and she had a partially fixed abdominal tumour which weighed $10\frac{1}{4}$ lb. after removal. She suffered very much from shock after-

wards, but her abdominal condition never caused us any anxiety.

The conditions which most interfere with the propulsion of intestinal gases are those which affect the lower part of the ileum, especially in the neighbourhood of the ileo-cæcal valve.

Much handling or bruising of the intestinal walls interfere with its muscular contractions; they also excite an attack of inflammation which brings about the same result, and a speedy accumulation of wind throughout the whole of the small intestine. Thus some of the worst attacks of wind follow appendectomy, in which, owing to the presence of adhesions, much handling has been required. Also salpingectomy, when the Fallopian tubes have been much inflamed and become firmly fixed. This was one of the factors in the case of the young woman to whom I have already referred, and in whom the abdominal space was small, with thick rigid surroundings.

Operations such as these bring about a state of things which stops the movements of the lower end of the ileum, so that the gases which are being constantly produced in the small intestine of necessity accumulate. In addition to the mechanical effects of the operation, such as bruising, the peritoneal covering of the ileum may become inflamed. Inflamed structures cease to function properly and so the onward passage of the wind ceases.

The end of the ileum may come to form part of the wall of an abscess. When the abscess is

chronic and bounded by thick adhesions the passage through the intestine is less likely to be obstructed than when the abscess is acute. In the latter case the inflammation is both more intense and more extensive.

In cases of appendicectomy, the ileum, cæcum, and the ileo-cæcal valve are often swollen and inflamed, an additional obstruction to the onward passage of gas.

If we contrast these operations in the lower part of the abdomen with those in the upper, we begin to understand why, in operations upon the gall bladder and stomach, the onset of an attack of wind is much less to be feared.

A very severe and dangerous attack of wind occurred after an operation upon a patient who was supposed to be suffering from appendicitis. At the operation the appendix was inflamed, but not enough to account for the acuteness of the symptoms. An exploration of the pelvis brought to light an inflamed Fallopian tube full of pus. The operation was one of emergency and there had not been time for a thorough clearing of the intestines, so that an attack of wind was likely to ensue. The nurse in charge of the case was therefore instructed to give enemata and make certain of the passage of the wind. When the patient was visited the following morning she had omitted to carry out her instructions, because she said the patient had been so comfortable during the night. All the time the intestinal

gases had been accumulating—the abdomen becoming tightly distended, and intestinal obstruction beginning. Relief was a matter of great urgency, and was only brought about with difficulty. A devoted and skilful nurse is of untold worth at such a crisis.

Anything which prevents the intestines from being properly cleared before an abdominal operation renders an attack of wind much more likely.

A man in Kenton ward had a malignant growth of the cæcum, ileo-cæcal valve, and half of the right colon. I removed the large growth and joined the ileum to the transverse colon by a lateral anastomosis. Great abdominal distension set in and only slight relief was obtained by enemata. The attack was ultimately overcome by calomel and other aperients. The onset of this attack of wind had been thought of during the operation, and the anastomosis had been made, as is my custom, with a triple line of sutures behind and two in front. In another similar case this suturing likewise withstood considerable quantities of aperients.

I have already referred to the danger of wind after exploratory laparotomy, when a large tumour has not been removed. On more than one occasion I have faced the risks of an immediate fatality from shock or hæmorrhage, rather than leave the patient to die of distension and obstruction.

The state of the abdominal walls has an obvious influence upon the intestinal gases. Abdominal walls which are thin and flaccid are not necessarily

a source of danger. It is true that they are not well fitted by their contractions to aid the diaphragm in the expulsion of the wind, but at the same time they yield readily and permit the necessary movements of the intestines. But abdominal walls which have been divided by long incisions, bruised, lacerated, or pulled about, are for a time incapacitated. The need for gently handling and judicious incisions is obvious. It is sometimes better to lengthen a small incision than to violently pull it apart. The degrees of distension are best seen in those with thin abdominal walls. At the beginning the abdomen bulges in front, then at the sides, and finally rises above the costal margins.

The condition of the vascular system of the intestines may also be an element in the accumulation of gases within them. I know little about this branch of the subject, which nevertheless is very important.

In two cases of complete thrombosis of the mesenteric veins, upon which I operated, the intestinal distension was enormous. And in a case of partial thrombosis of the inferior mesenteric vein, about a foot of the end of the ileum was engorged with blood and thickened, whilst the part above was much distended. Furthermore, during the course of operations I have noted very marked vascular changes in cases operated upon during abdominal distension, and believe that the relief of this vascular engorgement helps very much in bringing about intestinal movements.

Obviously the splanchnic and somatic nervous systems are able profoundly to affect the muscular tone and movements of the intestines. It is well known that the intestinal movements are influenced by the emotions, which may bring about other results, such as vomiting or diarrhoea. Also, the suddenness with which an attack of wind sometimes occurs suggests the controlling influence of the nervous system.

Marked abdominal distension and intestinal paralysis were met with in a female who five years before had had an operation performed for a carcinoma of the breast. Afterwards she became paraplegic, probably owing to a secondary growth implicating the spinal cord. An attack of flatulent distension came on and ended in obstruction. The abdominal walls were very thin and became so tight that their skin was shiny. A tap brought forth a high-pitched note. Every remedy was tried and laparotomy was on the point of being done, when, fortunately, an enemata administered during anaesthesia gave relief.

Thus the production of wind is inevitable after abdominal operations, and its accumulation most likely unless proper precautions be taken.

As I have already pointed out, the production of the gas can be controlled by judicious dieting and treatment before the operation, and by care and gentleness during its performance.

The essential clinical sign preceding the accumu-

lation of wind is the non-passage of flatus. The next sign, abdominal distension and discomfort, is not reliable. There are some who think that the patient is doing well and is safe so long as there is no distension. But I have already pointed out that those who have strong and muscular abdominal walls never do become distended; and in very stout people, distension may be hidden by a layer of fat three or four inches thick.

When the usual signs of distension are wanting, the extreme tension of the abdomen may be felt even through a thick layer of fat. When that has been pressed through with the finger, the hard tense muscular walls are met with. Percussion, too, is an aid even in those who are stout. The ear should be trained to recognize the high note of abdominal distension.

Abdominal auscultation also affords most valuable information. During the early stages the movements in the intestines can be heard—a most favourable sign—but when they have become tightly distended their movements cease, and the abdomen may be as silent as it is in general septic peritonitis, and the silence is as ominous.

When the accumulation of wind has caused intestinal obstruction, then vomiting ensues. Some suffer for a very long time from anæsthetic vomiting, but it is easy to tell this kind of vomiting, as that which is brought up smells of the anæsthetic. Vomiting ceases when the odour of the anæsthetic is lost. If

the vomiting continues after this, suspect that there is something wrong.

As a rule no flatus is passed per rectum for the first twelve hours after an abdominal operation. Our ordinary routine treatment is to insert a rectal tube at intervals of four hours. The sphincters of the anus may prevent the escape of flatus. A rectal tube overcomes this difficulty. This tube is sometimes called 'the flatus tube' or 'wind tube'—both good names, telling the nurse why it is used. If at the end of twelve hours no flatus has escaped, then enemata are given. These usually consist of $\frac{1}{2}$ oz. castor oil, $\frac{1}{2}$ oz. turpentine, and as much soap and water as the bowel will comfortably hold—a pint, pint and a half, or two pints. This is given with a tube and funnel with the patient lying on the side. The nurse must be warned not to hold the funnel too high, as of course enormous pressure can thus be got up in the rectum. About a foot above the level of the bowel is safe. I forbid the use of the syringe known by the name of Higginson. I have reason to believe that it is a dangerous instrument; not only is it dangerous, but a fallacy attends its use. With it air may be pumped into the bowel and, escaping, lead to error.

Supposing the first enema brings about no result, then a dose of strychnine is given, say $\frac{1}{50}$ th of a grain, and the enemata repeated. We have not got such good effects with esserine as with strychnine.

Good results sometimes follow the addition of $\frac{1}{2}$ oz. of tincture of assafoetida to this enema—also

sulphate of magnesia and glycerine. But much depends upon the skill with which any of these enemata is given. There is as a rule no difficulty in placing them well up the large intestine by means of a long tube, but the passage of the wind from the small into the large intestine is essential to success. Perhaps this explains why the wind is sometimes got rid of after tight bandages have been loosened. The small intestines being relieved from pressure, have room to move in. The answers given by nurses may mislead, further inquiry eliciting that the so-called passage of wind had been from the mouth.

Should these measures not succeed, further enemata should be considered, but as a rule we begin at the end of twenty-four hours to administer aperients by the mouth. But before beginning you should know whether the patient is in the habit of taking aperients. Those who are always taking aperients may require enormous quantities after operations. Three grains of calomel are usually given for the first dose—then, after waiting for two hours, the bowel is tested with an enema of a pint of hot water with a tablespoonful of salt in it, to see whether the calomel is beginning to act. If there be no result, a grain of calomel is given each hour, and further test enemata are tried. After four or five doses of calomel have been given, they are followed by a saline aperient such as a seidlitz powder. It is, however, difficult to give aperients when the patient

is vomiting, but even then a double seidlitz powder sometimes answers.

A patient had a double pyo-salpinx removed and that on the left side of the pelvis communicated with the rectum. It was found impracticable to close the opening because sutures would not hold in the inflamed and rigid tissues. This dangerous operation was followed by great abdominal distension and vomiting. Enemata could not be given because of the rectal opening, but a double seidlitz powder brought about an enormous expulsion of gas and the patient speedily recovered.

Now and then, when other aperients have failed, a large dose of castor oil has proved successful. When the wind has escaped, the patient feels relieved, falls asleep, and the pulse and temperature begin to fall. After the enucleation of a large intramural fibroid of the uterus after the escape of much wind the pulse fell quickly from 115 to 107, and then to 100. But safety is not ensured until wind is passed naturally.

If in spite of every effort the attack of wind is not relieved, matters are becoming exceedingly critical. Do not assume that peritonitis has supervened. There is sure to be some peritoneal inflammation about the seat of operation, but a general septic peritonitis is extremely rare. In bad cases of the wind there is usually a moderate rise of temperature and acceleration of the pulse.

I have observed that some, when they have diagnosed a general peritonitis, become fatalistic in

their attitude, and disinclined to persevere in those strenuous measures which give relief.

In searching for the cause of the attack of wind the steps of the operation should be reviewed. Every one is keenly alive to the dangers of uncounted swabs or instruments. Each operation has its own possibilities, but a thorough rectal and perhaps vaginal examination should never be omitted.

After radical cure of hernia in which some of the omentum has been taken away, the ligature upon the stump may have slipped or have become septic ; after operations on the gall bladder, bile may have escaped into the peritoneal cavity ; after sigmoidostomy a loop of small intestines may have got caught between the sigmoid flexure and the lower abdominal wall ; after posterior gastro-enterostomy, the small intestine may have herniated through the transverse meso-colon ; and so forth.

Percussion may help to locate an effusion of blood, a collection of pus, or twisted intestine. Localized tenderness and rigidity may also point to any of these.

Any peculiarities met with at the time of operation should be taken into consideration. Deaths after appendicectomy are extraordinarily rare, but some years ago a patient died from intestinal obstruction. She had a floating cæcum.

One of the worst attacks of wind that I have ever seen occurred in a similar case. A woman was supposed to have had a severe attack of appendicitis.

Three or four years afterwards she suffered from symptoms which pointed to chronic inflammation of the appendix. Then she became pregnant, and at the third month was seized with very acute pain under the right semilunar line low down. The possibilities of a tubal gestation were considered, and likewise the possibilities of appendicitis. When the abdomen was opened a pregnant uterus was found in the pelvis, but nothing in the Fallopian tubes. The right iliac fossa and right flank contained small intestines, but no cæcum or colon. Some years ago I had investigated congenital abnormalities of the cæcum and colon, and knew therefore that, under these circumstances, the cæcum would be found in the upper left segment of the abdomen. The hand, therefore, was introduced towards the spleen, and the cæcum pulled down with a small chronically inflamed appendix attached to it. As I already said, this operation was followed by a most alarming and critical attack of wind which was overcome by means of enemata, which undoubtedly saved the patient's life. From a state of extreme peril she was in a very short time practically convalescent.

Pathological possibilities should also be taken into consideration, such as inflammatory bands or adhesions, especially omental adhesions.

After the removal of a pyo-salpinx, the pedicle of inflamed and thickened tissues had a cut surface the size of a halfpenny, which is 1 in. across. An attempt

was made to suture the peritoneum over this, but the sewing would not hold. For the first twenty-four hours after the operation the patient was very well, but little wind was passed. Then she felt sick and vomited a little. The abdomen became distended and the vomiting more frequent—no escape of wind could be obtained. Guessing that the intestine had become adherent to the stump an anæsthetic was given, the finger passed into the abdomen and a loop of adherent intestine torn from the stump. At the same time a good deal of blood-stained serum escaped from the abdomen. A drainage tube was inserted, and after further enemata relief ensued and the patient got well.

I have already mentioned the case of a young woman who had a violent attack of the wind after the removal of inflamed adherent Fallopian tubes. Those in charge of the case, misled perhaps by the absence of distension, had allowed the wind to get 'ahead of them'. Perhaps they thought general peritonitis had set in. Aperients were vomited up and enemata gave no result. Forty-six hours after the operation, under anæsthesia by gas and oxygen, the wound was opened with the finger and a long rubber drainage tube thrust into the bottom of Douglas' pouch. A little blood-stained serum escaped when the tube was put in, but afterwards ran out in considerable quantities and soaked the dressing. Very little was seen of the intestines, but their peritoneum was red and their veins engorged with

blood. The fluid that escaped was sterile. In a little while relief was obtained with enemata and the patient's condition ceased to cause anxiety.

I have come to the conclusion that in such grave cases of wind, the insertion of a drainage tube in this manner is a most valuable aid. Should it fail to give relief in a few hours, the question of enterostomy has to be considered. This desperate and objectionable expedient should not be left too long. It sometimes gives relief, but may necessitate a further operation for the closure of the fæcal leak.

XVII

FRACTURES OF THE PATELLA AND THEIR SURGICAL TREATMENT

INTRODUCTION

IT is fortunate that the knee-cap is seldom broken.¹ All fractures into joints are especially serious both at the time and afterwards. The joint is sure to inflame and cause pain and suffering, and it may in the end be stiff. But a broken knee-cap has a further evil. It is more likely than other fractures to end in bad union, and a weak and unreliable limb. In the past this has always been a grave misfortune to the wage-earner, but in the future he will find himself in a sadder plight. Under the Workmen's Compensation Act of 1906, employers are liable to compensate any person under a contract of service—workmen or domestic servants—for personal injury caused by accident arising out of and in course of the employment.² It is obvious that

¹ 1·4 per cent of all bone fractures according to Reichel, quoting v. Bruns and Rossi, *A System of Practical Surgery*, E. von Bergmann, vol. iii, p. 573, trans. by Bull and Tolley.

² See that useful little book, *Workmen's Compensation Act, 1906*, 7th edition, by T. Hy. Baylis, 1907, p. 73.

a crippled leg must make its possessor more liable to undergo fresh accidents; to fall again, or get run over. But in addition, after the knee-cap has once been broken it is more liable than other broken bones to break again. Out of the thirty-eight patients upon whom I have operated four broke the same knee-cap twice—two broke fibrous union obtained without an operation, and two pulled the fragments apart after they had been fastened together by an operation. Both of these will be described hereafter. In each there was an avoidable fault in technique. But not only is a broken knee-cap likely to break again, but when one has been broken the other is not unlikely to follow suit. Four out of the thirty-eight patients upon whom I have operated had broken first one patella and then the other. They were broken at intervals of one and a half, three, eleven, and thirteen years. I question whether my own experience of refracture or of double fractures of the patella is at all unusual.

So employers of labour have good reasons to look askance at those who have broken the knee-cap. The chances of refracture or of double fracture are very considerable, moreover the strength and security of the limb has to be reckoned with. A weak or crippled limb increases the liability to all kinds of accidents.

Clearly it is of paramount importance that the treatment of fractured patella should give a perfect result. Also that it should give a perfect result in

the quickest time. In the past I have treated fractures of the patella either with or without an operation. With the willing help of Mr. A. L. Candler, to whom my warmest thanks are due, a good deal of information has been collected about these cases. I propose to give the results and contrast the two methods of treatment. In addition Mr. Candler has collected some of the other cases treated in my wards, and these too will be used.

In contrasting methods of treatment it is necessary to know clearly what ought to be contrasted. Risks come first, and risk to life first of all. Then the risk to the limb, and last of all the risks to the utility of the limb.

Now, so far as I can learn, no one ever died of a simple fracture of the patella. A patient in my wards died with a fractured patella, but he had grave injuries to his thorax. A compound fracture is another thing altogether. Nor has any one, so far as I know, ever lost a limb because of a simple fracture of the patella.

So that as far as treatment without operation is concerned both of these dangers may be put aside, but there remains still that important question, namely, the ultimate value of the limb.

The operative treatment has, on the other hand, dangers which the non-operative does not possess. Although not always recorded, deaths after operation are not unknown, nor can risks to the limb be overlooked. Twice I have seen such severe suppuration

after attempts to unite fragments which were far apart that amputation had to be considered. Should life and limb be saved then, needless to say, the suppuration ends in a stiff and crippled limb.

Although the risks of the operation to life and limb cannot be ignored, yet my own experience—which will be given hereafter—leads me to think that they must be very remote. But however remote they may be, yet they ought not to be incurred if the alternative safe method of treatment without an operation can be relied upon to give sufficiently good results.

THE TREATMENT WITHOUT AN OPERATION

At St. Bartholomew's Hospital the treatment without an operation has usually been as follows—For three or four weeks the patient has been kept in bed with the limb extended upon a splint, or inclined plane. Sometimes the blood and inflammatory effusion has been removed by aspiration. Some surgeons, including my old master Mr. Willett, tried to pull down the upper fragment with strapping and elastic traction. About a month after the accident the extended limb is encased in plaster of Paris bandages or in a poroplastic splint. These are continued for about a year during which the patient walks about with a stiff leg. In the end bony union did not take place. The most that could be hoped for was strong fibrous union. We now

know that in nearly every instance of fractured patella some of the dorsal aponeurosis is torn off and pulled between the broken ends. This intrusion prevents bony union and retards or prevents fibrous, however close the fractured surfaces can be got together.

These remarks do not apply to those extremely rare instances of fracture without any separation.

SOME CASES ILLUSTRATING THE RESULTS OF TREATMENT WITHOUT AN OPERATION

The following are some instances—taken at random—of cases treated by long continued extension of the limb, and are probably fair examples of its results. Some of them had broken both patellæ and had had one treated by operation and the other without. These are especially useful for comparison. For example :—

In 1909 a carman broke his right patella and had it operated upon at Guy's Hospital. The skiagram shows that this patella has united by bone, and that the wire has been broken and pieces of it scattered amidst the tissues over the outer tuberosity of the tibia. The next year he broke his left patella. The fracture was transversely through the middle of the bone with half an inch of separation. No operation was performed because he had a stricture of the urethra and his urine contained much pus. He was treated in the usual way. The limb was

extended on a splint for about three weeks and then put into a plaster of Paris case for six months. The skiagram, taken two years and two months afterwards, shows that this patella has united by fibrous tissue with about a quarter of an inch of separation.

The patient himself is of opinion that there is nothing to choose between the two results and that both are equally good. He has perfect movement in both knees and is able to earn his living as a carman. He says that eight months elapsed before the right leg, which had been operated on, got well. The left, which was not operated upon, took about the same time.

Clearly good results can be got by both methods. But I should have expected the leg which was operated upon to have got well quicker. Clearly the patient took the risks of an operation without reaping one of its great advantages—a speedy recovery.

At the age of 50 years a man broke his right patella and had it treated in an infirmary by means of a plaster splint. Eleven years afterwards the fragments are three inches apart and the femur can be felt between them, covered with thin tissues. The knee-joint can only be flexed 15° and is almost powerless for extension.

When 61 years old he broke the left patella. I treated this by operation. Nine months afterwards he said the leg felt strong. Its muscles were firm and well developed. The knee-joint flexed to a

right angle and extension was perfect. The patella was apparently united by bone with the wire in position. Unfortunately we could not get this man to have it photographed.

The superiority of the operation seems incontestable. Perhaps we ought to know more about the way in which the splint treatment was carried out upon the defective side.

At the age of 37 a man broke his left knee-cap. The treatment without an operation resulted—as shown by the photograph—in at least three inches of separation; but in spite of this the limb seems to have been a useful one, because three years afterwards he broke the right patella, and after it had been treated, and after this double fracture, he said he could run and catch a tram. His right knee-cap was broken by slipping on a greasy road (February 4, 1904). The fracture was transverse with the fragments close together. The leg was extended upon a back splint until February 17, when there was so much swelling and cramp that the splint had to be taken away. On February 24 massage was begun. On March 26 a poroplastic splint was fitted and he began to walk with crutches. The splint was worn for eight months. This man's right patella was not wired because he was a glass-fascia writer and it was considered he would get a sufficiently good result without the risks of operation. Clearly this was so, for in spite of three inches of separation on the left side, he was able, as I have said, to run after

a tram-car. But on October 6, 1909, he again slipped whilst crossing a road, and tore through the fibrous tissue by which his right patella had united. On this occasion an operation was performed; the lower fragment was so small that it had to be united to the upper by a horizontal circular loop as well as by the usual vertical wire. He now has a useful but far from perfect limb. The skiagram shows that the wiring has torn out of the upper fragment. Possibly torn a piece of it off, because there is an inch of separation with a shadow between which may indicate the presence of bone. There is no wasting on this side and the limb can be flexed to a right angle. His progression is very good but he cannot run.

Here we have a case in which massage was employed at an early date and in which, although the results seemed to be good, the patella broke again and then after wiring pulled apart again, indicating, one would suppose, some fault in the patella itself. It is noteworthy that the patient says he could run after breaking both patellæ, and with three inches of separation on one side.

Here is an example treated without an operation. A man broke his right patella in May 1907. The fracture was transverse with an inch of separation. He was kept for a month in bed with limb extended on a splint. Afterwards a leather splint was worn for two years. Three and a half years after the injury there was much wasting of

the muscles of the right thigh and leg. The knee-joint can be fully extended but only flexed 20° . The man walks with a limp and says the leg occasionally gives way under him. The fragments of the patella, as the skiagram shows, are barely half an inch apart. They are united by strong fibrous tissue. In spite of this excellent repair of the patella the result is most unsatisfactory. Because although the long continued splinting had united the patella so well, yet it had wasted the muscles. The man was a printer's cutter and could earn his living although his leg was weak and insecure. Many other occupations would have been closed against him.

In 1897 a woman broke her right patella; it was treated by Sir Thomas Smith who kept her in bed for a month with strapping on the knee. Afterwards it was in plaster for six months and when that was taken off a back splint was worn for many months. She says she could not kneel upon it for two years. Thirteen years afterwards the right patella was firmly united by fibrous tissue with the fragments half an inch apart. The muscles of the thigh are strong and well developed—she says she can go quickly upstairs but cannot run.

In November 1910, the left patella was broken transversely at the junction of the upper and middle thirds. There was about one inch of separation. This fracture was united by operation. The skiagram shows that bony union is almost completed.

The calcification is not yet dense throughout. Clearly the right knee took a long time although in the end the result was very good. It is rather too soon to say what the left is going to be like, but it promises to give an equally good result in a sixth of the time.

A porter at a mineral-water works broke his right patella in 1904, and it was treated without an operation. He was in the hospital for three weeks and wore a splint for eight months. After taking off the splint he began to bend the leg himself, and four months elapsed before he got movement in his knee-joint. In the end the fragments of the patella, which feel of equal size, are slightly movable upon one another; the gap between them is about a quarter of an inch. There is no muscular wasting, and a complete range of movement in the knee-joint. He considers that the broken knee is as strong as the other. It never gives way although he has to carry weights of nearly 2 cwt.

We have here examples of the results of treatment without an operation. (1) Good fibrous union with a good and useful limb. (2) Good fibrous union with wasting of muscles and defective limb. (3) Wide separation and a useful limb. (4) Wide separation and a wasted defective limb.

None of these cases illustrates (5) the formation of intra-articular adhesions during the expectant treatment. But I have seen such an one operated upon.

A burly policeman broke his patella in the month of March and again seven months afterwards, in October. In December the knee was quite stiff and the fragments of the patella two inches apart.

An operation failed to bring them together in spite of much cutting of extensors and retinacula. When the joint was opened, very firm and extensive fibrous adhesions were seen inside it. This is one of the cases which had such severe suppuration after the operation. One would suppose that after the first break the limb became of use again, and that the adhesions formed as the result of inflammation following the second. It is not likely that the man would have broken his knee-cap a second time had the joint ankylosed after the first breakage.

These instances of the results of the treatment of fracture of the patella without an operation are unsatisfactory in two respects. In a large proportion the limb was weakened or permanently lame, and in all the treatment took a long time ; perhaps a year on the average.

Some would say that these cases give a far too gloomy impression of the results of the treatment without operation. Unfortunately we have found it difficult to interview many of those who have been treated without an operation. An increasing number of fractured patellæ are now operated upon. Last year (1910) thirty-seven cases of fractured patella were admitted into St. Bartholomew's Hospital and twenty-five were treated by opera-

tion, so that few remain for purposes of comparison. But my own general impression is that the cases I have given are about average samples of the results of treatment without an operation, and by long continued fixation of the knee. I regret never to have seen any of the results of the treatment of Kraske and Zum Busch,¹ who 'give up the idea of bony union from the onset, allow the patients to go about on the second day, walk up stairs after the eighth day, and report good results'. Heroic surgery practised upon heroes.

The question of time is all important. Wage earners cannot afford the long course of treatment usually thought necessary. The following gives a vivid idea of what time means to the poor. A man writes: 'I feel very grateful for the operation which enabled me to start work again after fifteen weeks and this (*sic*) saved my wife and family from that place we all dread to go—the House).' When I place the disadvantages of operative and non-operative treatment before my hospital patients, the saving of time always makes them choose the operative—in spite of a clear statement of the risks.

A messenger, aged 52, broke the left patella. It was operated upon in the German Hospital. After a month in that hospital and a further fortnight's rest he says that he was able to work again. Eighteen months afterwards he had bony union with two wires in position—a complete range of movement and a perfect

¹ Loc. cit., p. 578.

limb. Then he slipped in the street, but did not fall. He said he heard something crack in the right knee, but went on wheeling a barrow, and when his work was done walked to the hospital. The right patella was broken across and he elected to have it operated upon. The expectant treatment was declined by this man on the score of time, and also because he thought that he could not mount stairs whilst the knee was being kept stiff.

TREATMENT BY OPERATION

I now come to the alternative treatment by operation. I have always done the open operation which since 1878 we owe to the genius of Lord Lister. Although according to Reichel¹ 'three hundred years previously Severino, and later Dieffenbach and Rhea Barton, had performed the same operation'.

About some of the details of this operation there is still some divergence of opinion, and, therefore, I propose to say what I have done myself and try to point out the methods which have given the best results. In the first place there can be no question as to the propriety of always performing an open operation. We now know that some of the dorsal aponeurosis is nearly always torn off and intruded between the broken ends. This effectually prevents bony union although good fibrous union can take place in spite of its presence there. It is, I believe,

¹ Loc. cit. p. 578.

impossible to ensure an accurate coaptation of the fragments except by an open operation.

Clinically it is not possible to form a precise opinion of the kind of fracture or of the position of the fragments. To the touch they may feel as if the broken surfaces were facing one another, whilst as a matter of fact the lower one may be so much tilted that its cartilage faces the broken surface of the upper one. X-ray photographs too give valuable information, but they cannot be relied upon to reveal the additional breaks which are so often met with in the lower fragment. An open operation also enables one to remove all the clotted blood from the inside of the joint. This could not be done through a puncture or small wound. And, in addition, rents in the capsule can be repaired.

Most of my operations have been done about the fourth or fifth day, but nothing is gained by delay, and now they are done as soon as the operation is properly organized.

Thirty-nine operations were done upon thirty-eight patients—thirty males and eight females.¹ The youngest was a woman aged 22, the oldest a man over 63 years of age. The right patella was wired sixteen times and the left twenty-one. Twice the side is not stated in the notes. Various skin incisions are in use. The patient who had been operated upon so successfully at the German Hospital had a transverse

¹ I have since operated upon a youth aged 14 years and upon a man. Both were quite successful.

scar showing that the operator made use of Lord Lister's original incision. But many surgeons now prefer to cover the line of union of the fracture with a flap, rather than leave it directly beneath the wound.

Another patient who had been operated upon in Guy's Hospital, and who also had an excellent result, had a vertical scar situated over the long axis of the patella.

I have always of late adopted a semi-lunar incision carried at the distance of an inch round the inner side of both fragments; beginning on a level with the highest point of the upper one; and ending at the outer side of the ligamentum patellæ. When this flap has been turned up it gives of course full access to the fracture, to the interior of the joint, and to the rents in the capsule. As soon as the joint is exposed, all the blood clot is removed from within it, and, not infrequently, some of the badly bruised tissues are cut away with scissors. The blood is nearly always situated between the fractures and in the front part of the knee-joint, a circumstance which makes it much easier to extract it with some sort of a scoop or spoon. At times the blood is under considerable pressure and spurts out when the joint is opened. One would have thought that under such circumstances the patient would suffer additional pain, but a man upon whom I operated a few days ago seemed to suffer little inconvenience although the blood spurted out of his joint in quite a considerable jet. This was the man who pushed a barrow for

half an hour after he had broken his knee-cap, and who walked to the hospital when his work was ended.

When any difficulty is experienced in dislodging the blood, abundant lotion is poured into the joint, usually a solution of biniodide of mercury, 1 in 4,000, sometimes saline solution, but I question whether the latter is quite to be relied upon in the dust-laden air of the city of London.

And next all the dorsal aponeurosis is cut away from between the ends of the bone. This structure is nearly always intumed, and covers part of the broken surfaces of both upper and lower fragments. As a rule the upper is the largest flap. When the operation has been delayed the broken surfaces are often covered with firm reparative material. This is removed with a knife, scissors or a Volckmann's spoon. This removal is very thorough and occasionally I have sawn off a thin layer of bone. At this stage of the operation the torn capsule is trimmed up with scissors.

At an early stage of the operation a drainage tube about one third of an inch in diameter is placed at the outer side of the joint. A pair of curved pressure forceps is pushed down the inside of the joint over the external condyle and made to project in front of the external lateral ligament—here it is cut down upon until it can be pushed through, so that when it is withdrawn it carries with it the drainage tube. It is advantageous to introduce a drainage tube at this early stage of the operation, for it serves to carry

off any of the blood which escapes whilst the fragments are being trimmed, or the lotion used to wash out the blood.

In some of my earlier cases the drainage tube was lodged through some part of the original skin incision, but that did not seem to afford such satisfactory drainage. The question of drainage will be referred to again.

I have already remarked upon the difficulty of diagnosing the kind of fracture except by looking at it through an open wound. The following were met with in thirty-eight cases :—In twenty-nine the fracture was transverse, the fragments being about equal in size ; but in six of them the lower fragments were smallest, sometimes too small to hold a wire, and in two the upper fragment was smallest—but I have never seen the upper fragment as small as I have sometimes seen the lower.

In two the fracture was T-shaped, the main fracture crossing the middle of the patella transversely with a vertical fracture through the centre of the lower fragment. In three the fracture was transversely through about the centre of the patella, with a small triangular piece broken off the outer angle of the lower fragment. Once the fracture was transverse with a small triangular piece broken off the inner angle of the lower fragment.

Two were Y-shaped, the fractured end of the upper fragment forming an angle which fitted by its apex into a vertical fracture through the centre of the

lower fragment. Once the fracture was comminuted, with a transverse fracture through the middle of the patella with the lower fragment broken into three by two vertical fractures, so that there was a large central fragment and a small outer and inner one.

One of the Y-shaped fractures was diagnosed clinically as being a star-shaped fracture. There is a tradition that these fractures unite by bone and therefore do not require an operation.¹

A woman with what was diagnosed to be a star fracture was treated in the usual way and went to Swanley with a plaster splint on. On her return she broke her knee-cap as she was ascending the surgery steps. On this occasion an operation was performed and, as I have said, a Y-shaped fracture was found.

I am afraid that I cannot agree with my friend Mr. Burghard that X-ray photographs 'readily differentiate' the kind of fracture. But it is found that the fissures in the lower fragments are hidden by the shadow of the femur.

We have found it hard to learn from the patients how they broke their knee-caps. As a rule those with a transverse break did not hit the knee against the ground, although they may have fallen down. An in-

¹ In a recent work on Surgery it is said, 'the stellate or longitudinal fractures which are readily differentiated by the X-rays and which are not accompanied by any marked separation of the fragments, are excluded from this category; in them excellent bony union usually results without operation.' *A System of Operative Surgery*, by F. F. Burghard, vol. i, p. 611.

telligent man said he was wheeling his barrow in the street when he felt a crack in the knee. He never fell, nor touched the ground with his knee, but went on wheeling his barrow, and afterwards walked to the hospital. So his right patella was broken across by muscular action. This case also shows that muscular action may do more than break the bone clean across, because he had a small piece, which was not to be seen in the X-ray photograph, broken off the outer angle of the lower fragment; leading one to think that the T- and Y-shaped fractures may also be caused by muscular action; and also those in which the fracture is transverse with a piece broken off the outer or inner angle of the lower one.

Another fairly well educated and intelligent man with the patella broken into four fragments was positive that he fell so that the bone came in violent contact with the ground.

Mr. Blakeway has most kindly obtained for me knee-joints divided vertically. These have been photographed at various degrees of flexion. They confirm what we already know about the movements of the patella. When the knee is bent to half a right-angle a very small area at middle of the patella is in contact with the convex condyle and the bone is most advantageously placed for being broken across the condyle; like—as the books say—a stick across the knee. When the knee-joint is bent at a right angle only the upper third of the patella is supported on the condyles, the lower two-thirds

being unsupported, and, as can also be felt, projecting in front. Thus the lower end is most favourably placed for being broken off or comminuted by direct violence—or for being pulled off by the leverage of the tibia acting through the ligamentum patellæ. The lower end of the patella is thin and pointed and, we may assume, unable to offer as much resistance as the rest. We have here some reasons to explain why the lower fragment is usually the smallest. I have not yet met with a vertical fracture in the upper fragment. It has invariably been single. As Mr. Blakeway suggests, the upper fragment is not likely to be broken again by the quadriceps extensor because it is easily pulled up and can offer no further resistance.

But vertical fractures are quite common in the lower fragment and are usually central.

The outer angle of the lower fragment was broken off in three instances, and the inner in one—I should expect the thin outer angle to be more easily broken off than the thick inner angle which is supported during flexion upon its crescentic facet.

I suspect, moreover, that during violent muscular action the thin outer edge of the patella may be pulled against the external condyle. I have seen a case of genu valgum in which both patellæ were pulled outwards over the external condyle of the femur by the contractions of the extensors of the thigh.

THE OPERATION

I have always used annealed silver wire to bring fractures of the patella together and consider it by far the best and most reliable material. One of my patients had previously had his patella united with fishing gut, but came apart again six months afterwards. When I operated the second time, bony union had not taken place. The fishing gut had come undone at the knot. Every salmon fisherman knows what treacherous material gut is to knot. On these grounds alone it is to be condemned.

Kangaroo tendon was used once last year at St. Bartholomew's Hospital.¹

Once my colleague, Mr. Rawling, tried chromic gut, but found it difficult to pull upon it enough to bring the fragments firmly together.

My colleague, Mr. Bruce Clarke, uses catgut to unite the dorsal aponeurosis and rents in the capsule and puts no other retaining sutures in. He is very contented with the results. It is, he considers, harmful to drill the patella.

Although silver has been used for subcutaneous sutures for many years, we do not know much about its behaviour in the tissues. In the year 1900 I used silver wire ten times for the radical cure of hernia after the method of Bloodgood. At the beginning of this year I had to remove some of the

¹ Surgical Registrar's Report.

wire which had been buried in the tissues for about ten years. It had turned black and all the tissues about it were deeply pigmented.

The photograph of the knee of a patient who had had his right patella united by wire at Guy's Hospital in 1899, shows the front half of the wire, including the twisted part, in position. But the hinder part of the loop has been shattered into three pieces. Two of these, each about one-third of an inch long, are close to the outer side of the tubercle of the tibia, whilst one bent piece three-quarters of an inch long lies over the outer tuberosity of the tibia. This state of things was not suspected until the photograph was taken. Another man whose patella was wired on January 13, 1906, and who was photographed in January, 1911, had bony union and a perfect result, but the skiagram shows that the wire has come undone at the twisted part, and that its loose end gradually tapers off, as if it had undergone corrosion.

For simple transverse fracture of the patella I have used a single stout wire. This is applied with the intention of keeping the fragments in accurate contact until they have undergone bony union. Nothing short of bony union is able to withstand the enormous strain of the great extensor muscles. I have never seen it stated, but infer that some surgeons do not attach the same importance to bony union, but expect sutures alone to withstand the action of the extensor muscles. I know an instance in which the patient was allowed to work within six weeks of the operation. The

surgeon could not have expected bony union in that time and must have relied upon the wire. The photographs lead me to think that about three months is required for good bony union. One of my cases illustrates the dangers of using a double strand of wire. On February 25, 1910, I wired the transverse fracture of the left knee-cap for a man aged 27. The wire supplied for the operation was so thin that it had to be used double, both strands being threaded through the same drill-holes in the patella and then twisted together in front of it. On August 29, 1910, the man whilst carrying a weight broke the bone again. At the second operation I found that the wires had come undone at the knot. There had been no attempt at bony union. I suspect that the wire came apart long before the accident, allowing the fragments to separate with failure of bony union. On the second occasion the wires themselves were not taken out, they seemed in perfect position, and by merely twisting them together separately the surfaces came into accurate apposition. In January 1911 the photograph shows both knots intact and that bony union has taken place. He was seen at the end of February 1911. He is a fish porter and says he can carry two cwt. of fish—that his left leg feels as strong as the other, and that he can run upstairs. He has complete flexion and extension, but slight wasting of the muscles of the left leg. This man's case illustrates another point. I usually drill the frag-

ments obliquely, so that the drill-hole emerges through the broken surface exactly in front of the articular cartilage. When the wire is tightened up none of it is exposed within the joint. But in this case, as in some of the others, the cartilage was drilled, and the skiagram shows at least half an inch of the loop within the joint. So far no harm has resulted and I question whether it matters at all. Indeed I have not hesitated whenever the lower fragment has been particularly small to encircle it, cartilage and all, by piercing the ligamentum patellæ.

The fractures were found to be so variable that they had to be wired in a number of different ways—such as—

A. A single vertical loop at the middle of transverse fractures.

B. Two slanting sutures for Y-shaped fractures. The upper fragments drilled once.

C. Four slanting sutures. The fractures being obliquely across the patella with the lower fragments broken into two unequal parts.

D. A single horizontal loop to enclose a piece broken off the inner or outer angles of the lower fragment. Transverse drill-holes. The same used for T fracture with small lower fragments. Mattress sutures not practicable.

E. A horizontal circular suture the same as D, through the upper and both lower fragments of a Y-shaped fracture, combined with two slanting

sutures the same as B but intertwined with the horizontal loop.

F. A horizontal circular suture through the upper fragments and surrounding a very small lower fragment by passing through the ligamentum patellæ. Reinforced by a vertical loop through the upper and lower fragments above and below the circular loop.

Sixteen months afterwards the fragments were nearly an inch apart, the wires having torn out of the upper fragment. The man had a useful limb because of the presence of fibrous union and his skiagram shows some bone in it.

This man had broken the left patella once and the right twice, the right being the one operated upon.

G. An attempt was made to fix another small lower fragment by using the wire horizontally through the ligamentum patellæ, but it tore out sideways. A horizontal loop was carried through the centre of the lower fragment and through the ligamentum patellæ near its tibial attachment. This served to reinforce the ligamentum patellæ and support a vertical median suture which held the upper and lower fragments together; and two thin lateral sutures which closed large rents in the capsule and helped to hold the fragments together.

The ultimate result was good—four months afterwards the patient had good union and could kick hard with the leg. Ten years afterwards he writes to say that he is cured and working in a warehouse.

Whilst inserting the wire I have taken care to

keep the twisted ends as high as possible so as to be out of the way in kneeling. But when kneeling is painful, as it was in two instances, the pain is not always due to the wire.

I am indebted to Mr. Cyril Thatcher for a very clear photograph of a wired patella. At the operation the lower fragment was very small and consequently the twisted end of the wire was rather low down. Six years afterwards the result is all that could be desired, except that kneeling is painful. Mr. Thatcher's photograph shows the wire in position, and the patella united by bone. But just below the patella there is a small bony nodule in the ligamentum patellæ. There is no reason for thinking that this was part of the lower fragment—which although small was single—it seems to be a new formation.

A butcher whose left patella was wired in 1908 said in 1911 that the leg was as strong as the other and that he 'could do anything but kneel, because something seemed to pierce the skin'. Here again the lower fragment was very small. The twist in the wire is high up, but the lower end of the loop projects in front at the attachment of the ligamentum patellæ.

The wire had to be taken out of two of the thirty-nine cases.

A woman, aged 28, had a transverse fracture of the left patella wired February 10, 1906, five days after the accident. Large rents in the capsule were closed with silkworm gut. A week later a subacute

abscess formed beneath the flap—drains were placed beneath the flap, and one at the outer side of the joint. At this time an offensive vaginal discharge was discovered. The infection was mild and ran a favourable course. Two months after the operation the patella was movable, and nine months later I saw the patient, and she also had a good and movable knee. The wire was removed from a sinus three months after it had been put in. Also one of the pieces of fishing gut ultimately came out.

This was before the days of X-rays, so that I do not know for certain whether bony union took place, but I think it did. It is the only case out of thirty-nine in which there was suppuration. The operation would not have been done had I known about the infection.

The second case was also that of a woman, who was admitted intoxicated with a compound fracture of the right patella. The fracture was supposed to be of the star kind, and should therefore, according to tradition, have united by bone. After a projecting piece of bone had been removed there was slight suppuration caused by staphylococcus albus infection. The wound healed and the woman went to our convalescent home wearing a plaster splint. On her return she slipped on the Hospital steps and broke her right patella again. When this was operated upon the main fracture ran across the middle of the patella obliquely from above downwards and outwards. The middle of the lower

fragment was broken in two vertically. The upper fragment had adhered to the femur but the fibrous tissue was easily broken through. The fractured surfaces were covered with fibrous tissue and had to be refreshed with a saw. Four slanting sutures were inserted. The wound healed by first intention, but later a hæmatoma was found under the flap, and the wire was seen through a minute ulcer in the skin. Gas was given and one of the four wires removed. Healing quickly followed, but three months after the operation the movements of the knee were limited—although she was walking about quite well. We have been unable to see this woman again.

I do not propose to dwell upon the remaining stages of the operation. The wound is closed and the limb adjusted upon a comfortable splint such as Neville's or Mackintyre's. The drainage tube inserted at an early stage at the outside of the joint is left in position and taken out in forty-eight hours or more. The dressings are put on with a many-tailed bandage so that it is easy to get at the tube without any movement whatever.

The drain always allows much blood and synovial fluid to escape. I have tried to do without the drain, but the patients usually suffered from far more pain than those who were drained. When the tube was removed on the third day, from the knee of a man who is said to have had no pain, a gush of synovial fluid ensued. Once the drain was kept in for several days because some synovial fluid con-

tinued to run from it. In another instance bright arterial hæmorrhage followed the removal of the tube on the second day. This was at once stopped with a strip of gauze. One man had much reactionary hæmorrhage from the tube the night of the operation, and the sheets were soaked with blood. It ceased spontaneously, but the tube was left in for five days. His recovery afterwards was uneventful. Another man had a small hæmatoma under the flap, but it caused no delay in the repair.

The only other complication occurred in an aged working man who before the operation looked ill and anæmic. He gave his age as 63. The operation was undertaken with reluctance, although he was very anxious to have it done. A week afterwards his leg swelled, and it was clear that he had thrombosis of the popliteal and superficial femoral veins. Three and a half years afterwards he had a very good and useful limb; but said the knee ached a good deal at night. He has chronic rheumatic arthritis in both knees. The skiagram shows good bony union.

I declined to operate upon a man with stricture of the urethra and septic urine, and upon another with albuminuria.

To learn the ultimate result, we have been able to obtain skiagrams of twelve out of the thirty-seven patients. In eleven there is good bony union with a thoroughly good limb. The case in which bony union failed has already been mentioned. It

is that of the man whose left patella had been broken, and, as a result of treatment by splint, the fragments were three inches apart, with wasting of the thigh. He afterwards broke the right patella, which was also treated with splints, but the bone broke again and was then wired. The lower fragment was so small that it could only be surrounded by horizontal and vertical wires. The skiagram shows that these have come out of the upper fragment leaving a gap of nearly one inch, but with what appears to be a bony nodule in it. The man is well able to do his work, but says that he cannot run or go quickly upstairs. Although he has not got bony union, the leg which was operated upon is certainly the best.

Another patient whose patella was wired for me by Mr. Jameson also has good bony union, and the limb is as good as the other.

A small proportion of the patients had some slight diminution in the degree of flexion, but as this never prevented kneeling it was scarcely noticed.

In spite of excessive wasting of the left leg, owing to bygone infantile paralysis, a man, aged 43, recovered with a leg as good as it was before. Two and a half years after the operation he has good bony union, but cannot flex the knee quite so far as he could before the operation.

Considerable differences of opinion prevail as to the best treatment after the wound is healed. Some begin to bend the joint during the third week—

massage it—and by the sixth week allow it to be freely used. But, as I have said, bony union cannot be relied on so soon ; and therefore, although making use of gentle movements and of massage, I apply a restraining apparatus until the end of the third month—lest the repair be torn through by a sudden bending of the joint. A plaster of Paris or poroplastic splint is usually employed, but those who can afford it have a jointed steel apparatus—made with a ratchet—to allow of gradually increasing flexion.

I only find one note to say that nitrous oxide had to be given six weeks after the wiring to break down ‘ many firm adhesions ’. The formation of adhesions seems to be exceedingly rare.

It has been found desirable to begin massage and movements early when there has been much bruising of the tissues. This is commonest when the fracture is comminuted.

In conclusion, I venture to say that the facts which I have laid before you are most strongly in favour of treating all fractures of the patella by an open operation. That gives a far better result in a far shorter time. The advantages gained far outweigh the risks.

XVIII

AMPUTATIONS AT THE HIP AND SHOULDER

(INCLUDING INTERSCAPULO-THORACIC), AND ADVOCATING THE PRELIMINARY LIGATURE OF MAIN VESSELS IN OTHER AMPUTATIONS

I PROPOSE to put before you some amputations at the hip and shoulder. This series of major amputations has impressed me with the extreme importance of avoiding hæmorrhage. Hæmorrhage and shock are almost synonymous terms. The chief step in preventing hæmorrhage during amputation at the hip and shoulder has been the preliminary ligature of the great vessels. I shall conclude by advocating the extension of this step to amputations elsewhere, more especially to those of the arm and thigh.

I am emboldened to speak about amputations because I have observed how little attention the subject has of late received. Nevertheless, it seems reasonable to suppose that the last word has not yet been said upon this important branch of surgery. I wonder whether all surgeons feel quite so keenly a disaster after a major amputation as they would one from septic peritonitis after laparotomy. But amputations have often to be performed upon

patients who are gravely and critically ill, and I am convinced that more of these would recover if every drop of blood were saved during the operation.

In referring to the cases I do not propose to enter into minute details. The first step has always been to ligature and divide the main artery; the second to elevate the limb to economize its blood¹ and then to secure the great vein; and the third to amputate the limb in a methodical and leisurely manner. The flaps are fashioned of skin and muscle, using a small knife for the purpose. The shape of the flaps and the position of the incision are determined by the conditions at the time of amputation. Muscles infected with sarcoma have been, as far as possible, divided at or near their attachments. If necessary, a separate vertical incision has been made to ligature the main vessels. It is a matter of little moment whether this be done or whether the amputation incision be used for that purpose. Three cases of interscapulo-thoracic amputation will be given first.²

INTERSCAPULO-THORACIC AMPUTATION

The following case, which culminated in interscapulo-thoracic amputation, is full of instruction. It shows the hopelessness of inadequate operations

¹ See also A. Webb Jones, *The Lancet*, April 24, 1909, p. 1208.

² I am greatly indebted to my house surgeon, Mr. P. T. Spencer-Phillips, for the trouble he has taken in supplying me with the notes of the hospital cases.

for malignant disease, and also how cautious we should be in making a clinical diagnosis.

CASE 1.—The patient was a thin woman, aged 48. Two years before I first saw her an operation had been done for the removal of the right breast for carcinoma. The mammary gland had been taken away and some of the pectoral and lower axillary glands. The same surgeon did two more operations for removal of axillary glands, but without removing the pectoral muscles. As might have been expected after such inadequate attempts, the cancer continued to grow in the axillary glands. These required removal, but in the meantime another complication had arisen. A tumour had grown in the left breast. Taking its clinical characters and the history into consideration, most would have assumed that this was a carcinoma. On July 19, 1904, it was incised. Competent observers pronounced it to be a carcinoma, but the immediate microscopical sections were those of excised chronic mastitis with cysts. So that after this had been removed the growth in the right axilla was taken in hand. After the pectoral muscles had been removed a number of carcinomatous glands were found reaching nearly to the clavicle. To get these away the axillary vein had to be tied and a portion of it excised. Great difficulty was experienced in removing some growth along the subscapular artery and its branches, although a part of the latissimus dorsi was taken away in the attempt. But in the end

I could not help feeling doubtful whether I had got away a prolongation which ran backwards above and below the long head of the triceps. I made up my mind that should recurrence take place nothing short of interscapulo-thoracic amputation would afford any reasonable chance of safety. Rather a heavy price to pay for the first inadequate operation. Recovery was uneventful with the exception of some temporary loss of power and swelling of the right arm.

Four months later (November 8, 1904) the patient returned, and it was thought that there was some enlargement of the glands in the neck, so I exposed the posterior triangle by the usual incision; but, in spite of a prolonged search, I could find no enlarged lymphatic glands. From my experience of other cases I believe this delusion was caused by the posterior belly of the omohyoid muscle, which contracts when it is touched, and in thin people feels like a small lump. For another year (until November 21, 1905) everything seems to have gone well. Then the arm became very painful, œdematous, and nearly useless. There was a quantity of hard scar in the axilla. On November 21, 1905, some of this was removed, and consisted of fibrous tissue with scattered groups of cancer cells. Recalling my former operation, at which the backward growth of cancer was found, I proceeded to perform interscapulo-thoracic amputation. The pectoral muscles had already been removed and no axillary vein was met with. After the clavicle had been

divided the subclavian artery was tied and the arm slowly and quietly removed, every bleeding-point being immediately secured. The division of the brachial plexus caused no shock. The operation took about one hour and a half, very little blood having been lost. It was followed by a moderate amount of shock and the patient rapidly recovered, the wound having healed by first intention.

The interscapulo-thoracic amputation was justified by what was afterwards found. Carcinoma had grown along the axillary border of the scapula and into the subscapular and infrascapular fossæ. Cancer cells were also seen in the cords of the brachial plexus. This probably accounted for the severe pain.

But this was not the end of this unfortunate patient's sufferings. On May 8, 1906, nearly two years after my original operation, I performed a seventh operation, and removed under gas a small nodule of spheroidal-celled carcinoma from the left side of the chest wall. It may be remembered that previously a part of the left breast had been excised for chronic mastitis with cysts. There is no record of this woman's death at Somerset House, so that she may yet live.

The second case of interscapulo-thoracic amputation illustrates the extreme malignancy of chondrosarcomata; also, how the wound may be infected by the air—usually a danger during such a long operation and with such a big open wound.

CASE 2.—On July 20, 1909, an attempt was made to remove an ossifying chondro-sarcoma springing from the neck of the scapula at the base of the coracoid process. The patient was a man, aged 64 years. Although the growth had a stalk, yet it infiltrated the neck of the scapula near the base of the coracoid process, and it was doubtful whether some of it had not been left behind. Interscapulo-thoracic amputation afforded the only possibility of removal. That was done on July 31. The incision which had been made in the attempt to remove the tumour was enlarged—much blood had been effused. After a quantity of clot had been turned out the clavicle was sawn through and the subclavian artery and vein tied and divided. The blood effusion made this step difficult. Clearly it would have been better that the growth had not been touched. The amputation was completed in the usual way. Bleeding from the small vessels was stopped at once and there was very little loss of blood. Again there was no obvious shock when the brachial plexus was divided. A large drain was left in the lower end of the wound. The shock afterwards does not seem to have been severe; the temperature fell from 98.8° F. to 98° F., the pulse being 84 and respirations 20, nor was reaction marked, for the temperature merely rose to 99° F. and the pulse and respirations remained the same.

The wound became infected and a severe illness ensued, followed by rather slow recovery. The

infection was *bacillus ærogenes encapsulatus*. When such a calamity takes place we always have a searching inquiry made, and I am indebted to Dr. M. H. Gordon for the trouble he took in investigating the source of infection. A film taken from the wound as soon as infection was suspected was crowded with a large Grampositive bacillus with a capsule. A guinea-pig injected with some of the exudation died in twenty-four hours from emphysematous gangrene, and its fluids were full of the same bacillus, which grew anærobically on agar and in milk. It corresponded to the *bacillus ærogenes capsulatus* of Welch, and also gave the characteristic change in milk known as Klein's enteritidis change, so that the kind of infection is not in doubt. Dr. Gordon ascertained whence it came. He says that having previously recovered this micro-organism from the open air, from the dust of rooms, from dirt deposited by boots, and from fæcal matter, it was thought probable that the wound had been infected from air at the time of operation, which took at least one and a half hours. So the next time I operated in that theatre Dr. Gordon exposed plates for two hours all round the theatre and obtained the same bacillus from a plate between the operating table and the door. Smoke experiments showed that a current of air passed from the door over the table to a fan, and that this current of air drew with it dust deposited by people's boots on the outside landing. The uses of adversity are sweet. Steps have been

taken so that in future this source of contamination may be avoided.

Thanks to Mr. John Adams and Dr. Hubert Chambers, I have learnt the after-history of this patient. Dr. Chambers informs me that he died in December, about five months after the original operation, from pleuritic effusion on the right side, the original tumour having been on the left. Dr. Chambers had no doubt but that this was due to a secondary growth, for he aspirated twice and removed blood-stained serum. He could not detect any recurrence in the scar or glands.

CASE 3.—Interscapulo-thoracic amputation was also performed on April 12 upon a man, aged 25 years, who had a large sarcoma of the scapula, filling the supra- and infraspinous fossæ, and complicated by growth in the glands of the axilla and the posterior triangle of the neck. It is said that a swelling had been noticed for less than six weeks; stiffness and pain seem to have been the leading symptoms. As it was obvious that the man would speedily die if left alone, I thought it right, after consultation with my colleagues, to attempt to prolong his existence. The anterior incision was begun almost at the top of the posterior triangle of the neck, and after the external jugular vein had been tied and the clavicle sawn through, the subclavian vessels were tied and the limb removed, together with the enlarged lymphatic glands. Some of these reached inwards so far that part of the sterno-mastoid muscle had

to be divided before they could be removed. This formidable operation lasted one hour fifty-five minutes; the shock was more than usual, but was easily and quickly overcome by injections of strychnine and the administration of a pint of saline solution with brandy in it. The patient made rapid recovery; his evening temperature was 99° F. for several days, and the quickest pulse-rate 100 on the third day. It is to be doubted whether reaction would have been so speedy and the improvement so lasting had his blood not been economized. Within a fortnight all the stitches had been removed, the wound having healed except a very small piece in the centre, which speedily closed. This man went to our convalescent home, but I regret to say the growth soon returned, and he died about two months afterwards. We had to deal with one of the most malignant types of sarcoma, for the pathological report says that the growth was soft and consisted of large round cells.

Glancing over these three cases of interscapulothoracic amputation I cannot help being struck by the moderate amount of shock—on no occasion did it give rise to serious anxiety—and next by the quick recovery. Even the case of serious sepsis was able during the height of his illness to sit up in bed, and although we felt apprehensive we always expected him to get well. To my mind the small amount of shock and the quick recovery were almost entirely due to Berger's admirable foresight. He

knew that to attain success hæmorrhage must be prevented by the preliminary ligature of the great vessels. It was not necessary to carry out Kocher's suggestion and try to tie the small transverse cervical and suprascapular arteries.

Next in importance comes the systematic division of muscles one by one, each vessel being secured, if possible, before division, but certainly the moment it begins to bleed. Not only does the prevention of hæmorrhage in these ways tend to minimize shock, but it also leaves the patient in infinitely better condition to make a speedy convalescence and to combat any complication which may ensue.

Although I especially inquired of my friend and colleague Mr. Richard Gill, who gave the anæsthetics, he was of opinion that there was no shock when the cords of the brachial plexus were divided.

In the next place, we should note the rapidity with which the sarcomata grew and carried off the patients. In the instance of round-celled sarcoma of the scapula, which was of the most malignant nature, there was no occasion for surprise, but in the other of chondro-sarcoma of the scapula there was every reason to hope that the operation had extirpated the disease. But this and other instances have convinced me that a chondro-sarcoma is a far more malignant disease than is usually thought; that it is likely to recur at the place whence it had been removed, and also to invade other parts of the body. We know the swiftness with which cancer invades

the lymphatic system. My experience tells me that a rapidly growing sarcoma invades the lymphatic system almost as quickly, and the vascular system much more quickly—very strong reasons why every tumour should be at once exposed, microscopically examined, and surgically dealt with.

AMPUTATION AT THE HIP-JOINT

I now propose to refer briefly to the cases of amputation at the hip-joint.

In recent works on operative surgery I cannot see that the principles which are so admirably fulfilled in Berger's operation have been systematically applied to amputation at the hip, otherwise we should not have to read accounts of ingenious, and in my opinion unnecessary, devices for the prevention of hæmorrhage. Amongst these various devices are steel pins for transfixion, steel rods, clamps, hæmostatic forceps, Esmarch's tourniquet, rubber tourniquet with transfixion steel pins to prevent it from slipping, a rubber tube pushed through the limb, and so forth. Such a multiplicity of devices is a sure indication that all is not well. In the following eight cases of amputation of the hip-joint the principles which govern Berger's operation have been carried out.

The operation has been begun by ligaturing the main vessels after emptying the limb of blood by holding it up. Then the flaps have been fashioned

and the muscles cut through one by one, and each vessel secured either before division or the moment afterwards. In spite of the grave condition of some of the patients at the time of operation, and in spite of the formidable growths for which some of the operations were performed, all recovered from the operation, and in some the recovery was rapid. In a few instances not only was amputation done at the hip, but malignant glands were taken away from the iliac fossa or pelvic brim.

Even an enchondroma is not so innocent as is sometimes thought.

CASE 4.—A man, aged 47 years, had a hard oval swelling four and a half inches by three inches fixed to the left femur below and in front of the great trochanter. It was thought that this had been growing for a year. It was an ossifying enchondroma, and was removed by chiselling through its bony pedicle. The body of the tumour consisted of cartilage undergoing mucoid degeneration. Six years later (February 13, 1906) he was readmitted. A tumour had slowly grown at the site of the old operation. It was sufficiently large to prevent full flexion of the left hip-joint. The inguinal glands were large on both sides, but the left were by far the largest.

On February 24 I performed an amputation at the left hip-joint. The incision was begun over the common femoral artery, which was tied and divided, also the common femoral vein, then all the lym-

phatic glands were removed from Scarpa's triangle and immediately examined. Those upon the top of the tumour had growth in them, but not those near the femoral canal. Amputation at the hip-joint was then completed in the usual way, the muscles being cut one by one and every small vessel secured immediately. My note says that there was very little bleeding. The shock following the removal of the lower limb at the hip-joint of a muscular man aged 47 years was by no means excessive. The temperature fell to 96° F., and the pulse rose to 112 and the respirations 28. A speedy convalescence ensued. There was slight necrosis of the edge of the flaps, but this caused but little delay in the healing.

The pathologist reports that the tumour was a chondroma and with no evidence of sarcoma in it. But however this may be, it is to be noted that the tumour had grown again after a bony stalk had been cut through, and that it had invaded the neighbouring lymphatic glands. This patient is known to have been well (November 28, 1908) one year and nine months afterwards.

CASE 5.—Amputation at the hip-joint was performed upon a woman aged 39 years. She had a huge growth in the upper part of the right thigh, and enlarged lymphatic glands in Scarpa's triangle and in the iliac fossæ. Thus the operation consisted of two parts: first, the removal of the lymphatic glands; and secondly, of the limb. The growth is

said to have been of six years' duration, and probably began in the soft parts.

On November 23, 1906, an incision was made over the common femoral vessels, which were ligatured after the lymphatic glands had been removed. The iliac fossa was then exposed and several enlarged lymphatic glands removed from it. Although these glands were so much enlarged they were said by the pathologist not to contain any growth. In spite of this I suspect that they would have continued to grow. The amputation was completed in the usual way and with hardly any hæmorrhage. The shock was comparatively slight, the temperature fell to 97.4° F., the pulse remained at 80 and the respirations at 28. My note says that the shock was about the same as might have been expected after the complete amputation of the breast with removal of the pectoral muscles. The wound healed by first intention. The amputation was performed on November 23, 1906. The patient died from a growth in the thorax on June 8, 1907. Six years is a long interval between the discovery of a tumour and its removal.

CASE 6.—Amputation through the hip-joint was performed upon a girl, aged 18 years, for a recurrent sarcoma of the femur. Six months before Mr. Butlin had amputated through the middle of the thigh for a tumour which the pathologist reported to be 'a periosteal cystic sarcoma—mixed-celled, with giant-cell systems'. On April 13, 1897, amputation

through the hip-joint was begun by an incision over the common femoral vessels, but a high division was found, so that the superficial femoral artery and vein were first tied and afterwards the deep femoral artery and vein. This anatomical peculiarity was easily recognized and gave rise to no difficulty. The remainder of the limb was then removed by lateral flaps and the wound was drained. Although there was but little hæmorrhage the shock during the course of the operation was severe. From beginning to end of anæsthesia it took one hour. There was great shock on division of the great sciatic nerve, and the patient had two injections of strychnine whilst on the table and afterwards saline injections. During the shock her temperature fell to 97° F., and the pulse rose to 108 and the respirations 24. However, a few hours afterwards a satisfactory reaction had taken place. For the shock which sometimes ensues when the sciatic nerve is divided may be severe at the moment but is not of long duration. The shock caused by hæmorrhage does not soon disappear, because the vascular system has to be replenished. Also we may note that the previous removal of the lower limb from the middle of the thigh made no difference in the shock. This wound became infected with colon bacillus, although I had taken care to keep the incision as far away as possible from the anus and pudenda. However, it ultimately healed well. About ten months later, on February 24, 1898, Mr. Butlin removed some

sarcomatous glands from the neighbourhood of Poupart's ligament, and eleven months later the patient died from a recurrence in the lung, having survived less than two years.

CASE 7.—Amputation at the hip-joint was performed under similar circumstances upon a boy, aged 11 years, who had had a tumour at the lower end of the femur. On April 2, 1907, the knee was explored by my colleague Mr. G. E. Gask, who performed amputation through the middle of the thigh. The tumour was a periosteal sarcoma of the lower end of the femur. The patient made a rapid recovery, but shortly afterwards some enlargement was observed in the lymphatic glands in the groin, and the end of the stump seemed suspicious.

On May 14, 1907, I explored the end of the stump and found mixed-celled sarcoma. Next, the common femoral vessels were tied and the enlarged inguinal glands removed—they are supposed only to have shown inflammatory changes, but I am very doubtful about the microscopical diagnosis of sarcomatous glands. I believe that they are often infected, although the pathologist cannot determine the fact. Amputation at the hip-joint was then completed with very little shock and consequent small loss of blood. The sciatic nerve had an unusually large bulb at its cut end. During the shock the temperature was 97° F. and the pulse 120; during the reaction the temperature rose to 99·4° and the pulse to 120. He left the hospital five weeks afterwards

for a convalescent home. Whilst he was at the convalescent home he is said to have had severe pleurisy, and he returned to his home, where he shortly afterwards died.

This is another instance of the malignancy of subperiosteal sarcoma. The swelling at the lower end of the femur had been noticed six weeks before Mr. Gask's operation—a slight limp drew attention to it, so there was no unreasonable delay in this case.

CASE 8.—Amputation of the hip was performed upon a female aged 32 years. Some of the clinical features were unusual. There was a fluctuating swelling at the upper and outer part of the right thigh, which was thought to be a hæmatoma, but in addition the lymphatic glands in Scarpa's triangle and in the iliac fossa were much enlarged. The tumour had been growing for three years. The tumour swayed a little, and the superficial femoral vessels lay behind its inner side. The tumour was exposed and after the escape of a quantity of dark blood, probably liquefied clot, a portion of the surrounding growth was removed and found to be sarcomatous. The anterior crural nerve ran through it and the superficial femoral artery and vein could not be separated from its inner and posterior surface, therefore a large anterior flap was made; all the lymphatic glands were removed from Scarpa's triangle and from the iliac fossa, after which the femoral vessels were ligatured and the limb removed, the muscles implicated in the growth being detached

from the pelvis. Very little blood was lost, and shock was moderate and satisfactory reaction speedily ensued. So that it is possible to add a pelvic operation to amputation of the hip.

I was guilty of an error of judgement in this case. The wound looked so dry and altogether satisfactory that instead of putting in a proper drainage tube a small gauze drain was used. This proved quite inadequate, for a hæmatoma formed and became septic.

The operation was performed on June 27, 1905; by September 27 of that year a growth could be felt fixed to the right side of the pelvis, and on October 31, 1905, the patient died from sarcoma of the pelvis. It is to be noted that the growth had been allowed to grow for three years.

The next two cases of amputation at the hip make an excellent contrast—they were both performed upon patients who were desperately ill and emaciated, after prolonged suppuration in and about the hip-joint. In the first the hæmorrhage was slight and consequently the shock was moderate. In the second, owing doubtless to the prolonged inflammation around the hip, there was more bleeding than usual, although in no way an excessive amount.

CASE 9.—A man, aged 46 years, had suppuration in the left hip for three years. There was a large abscess on the upper and outer side of the thigh. A photograph showed erosion of the head and neck of the femur and partial dislocation. A vertical

incision was begun well to the outer side and the anterior flap turned upwards and inwards. The common femoral vessels were then secured and divided and the amputation completed in the usual way. The note says that the shock was slight, the temperature fell to 96.4° F., the pulse became 116, and the respirations 26. As some of the acetabulum was bare at the time of amputation a sinus persisted and convalescence was slow, but the ultimate improvement in the patient's condition was remarkable.

CASE 10.—Ten years ago amputation at the hip was performed upon a boy by Furneaux-Jordan's method for suppurative arthritis of the hip. I began by tying the common femoral artery and vein through a separate vertical incision. There was some bleeding from a branch which sprang from the back of the vein and which was transfixed by the aneurysm needle. However, the bleeding was easily controlled by pressure forceps. The amputation was then completed in the usual manner. The hæmorrhage was smart when the circular cut was made and also during the removal of the upper end of the femur. The subsequent shock was severe.

This does not contrast favourably with the operations done by slow and methodical division of the muscles and immediate restraint of hæmorrhage. This patient ultimately recovered and did well.

CASE 11.—Amputation at the hip-joint was also performed for a youth aged 16 years. He, too, had suppuration in and around the right hip. He was

very anæmic and emaciated, with a persistently elevated pulse and temperature. On December 19, 1908, an abscess was opened and some thick pus escaped and staphylococcus pyogenes aureus was cultivated from it. But slight improvement ensued, and on January 25 the wound was reopened and a quantity of inflammatory tissue with cancellous bone in it was removed from about the back of the neck of the femur. After this he improved for a while, but the swelling remained, and then his temperature became continuously high, so on April 13, 1909, the sinus was again reopened and drained, but with no good result.

On June 22 amputation was performed at the hip. As the case was septic the common femoral vessels were tied with chromic gut, a separate vertical cut being made for the purpose. Some enlarged glands were with difficulty removed. My note says that the external circumflex artery and its branches bled freely. Owing to the brawny condition of the tissues it was difficult to restrain this as quickly as usual. Perhaps I could have done so if I had provided myself with toothed pressure forceps. Great shock ensued; his temperature fell to 96° F., the pulse became 116, and the respirations 28. As might have been expected, the wound supplicated, and on December 2, 1909, the boy left for a convalescent home, markedly improved in every respect, but there was still a sinus.

This case had, to my mind, a singular feature

about it. The tissue which was removed, and which was called inflammatory tissue with cancellous bone in it, looked like an ossifying sarcoma, and in spite of the suppuration I have never felt sure that this was not the case. A skiagram taken before the boy left the hospital showed a large shadow filling up the acetabular region, and the stump was enlarged and very hard.

Eight cases of amputations at the hip-joint have now been given, and fortunately all have recovered from this formidable operation. I have not met with that hæmorrhage from the obturator, gluteal, or sciatic vessels which some speak of. Vessels of any size can be seen and secured before they bleed.

AMPUTATION AT THE SHOULDER-JOINT

Impressed by the lessening of shock and speedy reaction in most of these amputations at the scapula and at the hip, and which, in my opinion, was due to the care taken in preventing bleeding, one naturally extended the same principle to other amputations.

CASE 12.—On January 21, 1908, amputation at the shoulder-joint was performed upon a man, aged 56 years. In addition to a periosteal sarcoma in the lower half of the left humerus he had osteitis deformans with characteristic changes in his skull and long bones. It is probable that the tumour had been growing for fifteen months and had attained a considerable size. A fresh section confirmed the

clinical diagnosis. The amputation was performed at the shoulder-joint by a method which is practically the same as that of Spence, but as a preliminary an incision six inches long was begun over the tip of the coracoid process and carried through the tendon of the pectoralis major. This brought in view the axillary vessels, which were tied above and below and divided. Then the brachial plexus was cut though without additional shock, and the amputation was completed in a leisurely fashion, the vessels being secured either before division or the moment they were met with. There was very little bleeding and consequently slight shock. After the operation the temperature merely fell to 97° F., the pulse being 76. The wound healed by first intention and a rapid convalescence ensued.

I am informed that within a few months this patient died from a recurrence of the growth in the spine. It is much to be regretted that the tumour had not been explored months before. A frozen section would have cleared up every doubt.

CASE 13.—Amputation at the shoulder-joint was performed in the same way upon a woman, aged 23 years, a domestic servant, for a subperiosteal sarcoma of the upper end of the left humerus. The operation was performed on April 17, 1900. The incision was begun at the tip of the coracoid process and carried down the arm. After division of some of the fibres of the deltoid the axillary artery was tied. Next some sarcomatous glands were dissected

out of the axilla. Then the incision was carried outwards and backwards through the deltoid because the tumour had grown into its lower part. Owing to the size of the tumour a part of the acromion process had to be sawn through to enable me to open the shoulder-joint and disarticulate. The size of the tumour was such that it had grown up against the acromion process. My note says that in the course of this operation the patient might have lost two ounces of blood. During shock the temperature was from 97° to 98° F. and the pulse was 88. During the reaction the temperature rose to 99.6° and the pulse to 114—that was the highest temperature. The pulse rapidly fell. The wound healed by first intention and the patient was discharged before the end of the third week.

This patient died on August 9, 1903, about three years and five months after the operation. Eighteen months before her death it is said that an operation was performed at Cardiff for an internal tumour, but afterwards she had, I am told, tumours all over her body.

CASE 14.—Amputation at the shoulder-joint was also performed on a woman, aged 65 years, on July 9, 1909, for gangrene which had begun on June 13. When the amputation was done the gangrene had almost reached the middle of the arm. When the artery was exposed it was thrombosed. There was very little shock—the temperature fell to 97° F., the pulse rose to 120, and the respirations to 32. The wound healed by first intention.

AMPUTATION THROUGH THE NECK OF THE
HUMERUS

The advantage of ligaturing the main artery was shown in the following case of amputation through the neck of the humerus.

CASE 15.—A man, aged 62 years, on October 27, 1908, had amputation performed through the surgical neck of the humerus. He had an epithelioma on the back of the wrist which had been growing for seven years, but more quickly during the last two. It had been ineffectually treated by X-rays and by zinc ionization. There was considerable enlargement of the supracondyloid gland, also suspicious fullness along the brachial vessels. The axillary glands were a little enlarged.

The usual incision was made beginning at the tip of the coracoid process and carried downwards for about six inches. The axillary glands were taken away, but contained no growth. Next, the cords of the brachial plexus were cut short and the brachial artery and vein were tied and divided. It seemed wise to remove as much as possible of the lymphatics, many of which are so close to the nerve trunks. The arm was then removed at the surgical neck of the humerus. There was very little loss of blood and consequently little shock, the temperature merely falling to 97° F., the pulse being 76. During reaction the temperature rose to 100° and the pulse to 100. Slight necrosis of the skin edge of the flap,

which was associated with the colon bacillus, delayed union. But he went to our convalescent home in less than a month (on November 24, 1908). This man died on January 21, 1910, the primary cause of his death having been certified to be bronchitis and the secondary morbus cordis.

In other amputations lower down the arm the main vessel is so easily controlled, even by the operator himself, that the previous ligation of the brachial vessels is not of great advantage. Nevertheless, I have of late begun by exposing and tying them. In the case of a child they were exposed, clamped, and tied, the ligation being put on afterwards.

AMPUTATIONS THROUGH THE THIGH

To anyone in the habit of ligating the main vessels before amputating at the hip and shoulder it is but natural to take a further step. I have never felt satisfied with what has hitherto been done to control hæmorrhage during amputations through the thigh. These have oftentimes to be done under desperate circumstances and when it is of the greatest importance to avoid loss of blood. Even a moderate loss of blood may extinguish the chances of one who is dying from diabetic gangrene. Those who are emaciated and weakened by long-continued tuberculous or septic arthritis of the knee are ill fitted to bear much loss of blood. If the immediate shock is overcome the reaction is tardy, and the exsan-

guined patient is not likely to make a speedy recovery or combat complications which may ensue.

The steps usually taken to control hæmorrhage during amputation through the thigh are not, to my mind, at all satisfactory. Am I singular in feeling distrustful of digital compression? Of digital compression of the subclavian artery Mr. F. F. Burghard says it 'is an uncertain method at the best'.¹ Once I failed to compress the external iliac artery and felt deeply humiliated by the failure. Compression was difficult because of some enlarged glands, and the fingers slipped when the limb was jerked up. More than once I have seen digital compression fail. Considerable loss of blood has invariably ensued. Others, too, seem distrustful of digital compression, otherwise we should not have had such a multiplicity of tourniquets. Those made of steel seem to have disappeared since the introduction of Esmarch's elastic band. But that has always seemed to me rather a crude, unsurgeonlike device. The wheal which it may make is unpleasant, and such pressure cannot always be good for the underlying artery and vein. May not thrombosis follow its use? Why should the skin flaps at times mysteriously die?

CASE 16.—A man, aged 66 years, had gangrene of the left foot. The anterior tibial artery could be felt pulsating, but a skiagram showed that it was calcified, and also the posterior tibial; the arteries

¹ Loc cit., p. 146.

generally were hard and tortuous. On March 9 a surgeon amputated through the upper third of the leg. An elastic tourniquet was put on just above the knee. By March 25 the flaps were gangrenous, with severe pain and septic absorption.

On March 26 I again amputated through the lower third of the thigh, the artery being exposed and tied forthwith. There was comparatively little shock. Owing to the infection of the cellular tissues of the thigh at the time of the operation the stump suppurated, but ultimately healed after further removals of femur. The patient left the hospital at the end of July with the wound healed.

The risks of amputating through the upper third of the leg when the vessels are calcareous are well known, but elastic pressure upon such vessels must be attended with danger, and it seems to me reasonable to suppose that in the instance just given that may have had something to do with the gangrene of the flaps. If the surgeon elects to amputate in the upper third of the leg when the arteries are known to be calcareous, it would, in my opinion, be wiser to plan the amputation so as to tie the tibial and peroneal vessels before completing the flaps, thus avoiding compression of the main artery and vein.

The troublesome and persistent arterial bleeding which takes place after the removal of the elastic band is one of its most objectionable features. This is mentioned in most recent works on surgery, but

they do not suggest any practicable alternative.¹ The long-continued elastic pressure seems to paralyse all the small arteries, especially the muscular ones. So that of late, taught by my experience of amputation at the hip and shoulder, I have begun amputation through the thigh with an incision over the superficial femoral vessels, so as to be able to tie them at the level of the sawn end of the bone. Afterwards the flaps have been slowly made with a small knife, each small vessel being instantly clamped. The following are examples of advantages of this simple proceeding.

CASE 17.—On October 10, 1909, amputation was performed through the left thigh of a man, aged 56 years, for advanced tuberculous arthritis of the knee. The disease had been in progress for two years; the pain was very severe. The man was emaciated, and in addition to tubercle of the knee had tubercle of the right lung. An incision was made at the lower end of Hunter's canal and the femoral vessels were ligatured and divided as they entered the popliteal space. The amputation was then completed in the usual way, any bleeding-point being at once secured. There was very little loss of blood and consequently little shock. The wound healed by first intention and the patient left the hospital twenty-four days after the operation.

The improvement in the patient's condition was

¹ e. g. Burghard in *A System of Operative Surgery*, 1909, at p. 248, refers to the well-known persistent paralytic oozing after its removal.

very marked, and we all agreed that he did very much better than such cases usually do, and which I attribute to the avoidance of hæmorrhage during the operation.

Every one, I am sure, dreads having to amputate the thigh in cases of diabetic gangrene, for that operation is attended by a very high rate of mortality. I doubt whether the following cases of amputation through the thigh for diabetic gangrene would have recovered had it not been for the immediate ligation of the main vessels.

CASE 18.—The patient was 64 years of age and had 4 per cent. of sugar in the urine. For many weeks some of his toes had been gangrenous. Latterly the heel had become affected and the leg was threatened. Septic absorption was taking place, the temperature was usually well over 100° F., and the pulse at least 120. His lips and face were rather livid and at times he was drowsy—altogether a forbidding state of affairs.

The operation was begun with the patient lying comfortably upon his back. The vessels were exposed at their exit from Hunter's canal. The artery and vein adhered so closely that, rather than spend time in separating them, they were both included in a single silk ligation. Without moving the patient the amputation was completed in a leisurely manner, the muscles cut through with a small knife, and each bleeding vessel secured at once. Very little blood was lost, and my friend Mr. Gill

tells me that he noticed no shock. Owing to the diabetes and the condition of sepsis the patient remained gravely ill—the pulse sometimes ran up to 130 and was intermittent. Strychnine and strophanthus certainly improved the heart, but the greatest benefit was obtained from the continuous inhalation of oxygen. This was continued day and night for many days. The stump healed by first intention and the patient was able to return to his home about three weeks after his operation. The relief was, I fear, but temporary.¹

I cannot help contrasting this case with another of almost exactly the same kind which was operated upon by the methods which I have now discarded.

CASE 19.—The patient, a lady of mature years, suffered from diabetes and had 5 per cent. of sugar in the urine. One day she applied some strong acid to a corn, and gangrene of the foot ensued. An assistant undertook to control the artery with his fingers. The amputation was performed through the thigh. When the vessels were divided there was for a moment smart hæmorrhage which was speedily controlled. In other circumstances the loss of blood might not have mattered, but the patient is said never to have recovered from the shock, and died within twenty-four hours. How much better it would have been for the surgeon to have had the hæmorrhage entirely within his own control.

Although my experience of ligaturing the main

¹ This patient survived for close upon sixteen months,

vessels of the thigh before performing amputation is so small, yet the results have been so admirable that I shall now continue to do so, and I trust that others will follow my example.

Since the above I have twice amputated at the junction of the upper with the middle third of the leg ; once for gangrene of the foot and once for supuration and necrosis at the ankle-joint. An incision was made over the anterior tibial vessels, which were tied. Then an outer flap was made and the fibula sawn through and the peroneal vessels tied ; next, an inner flap was made, the tibia sawn through, and the posterior tibial vessels tied. The immediate and ultimate results were exceedingly good.

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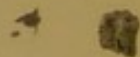
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