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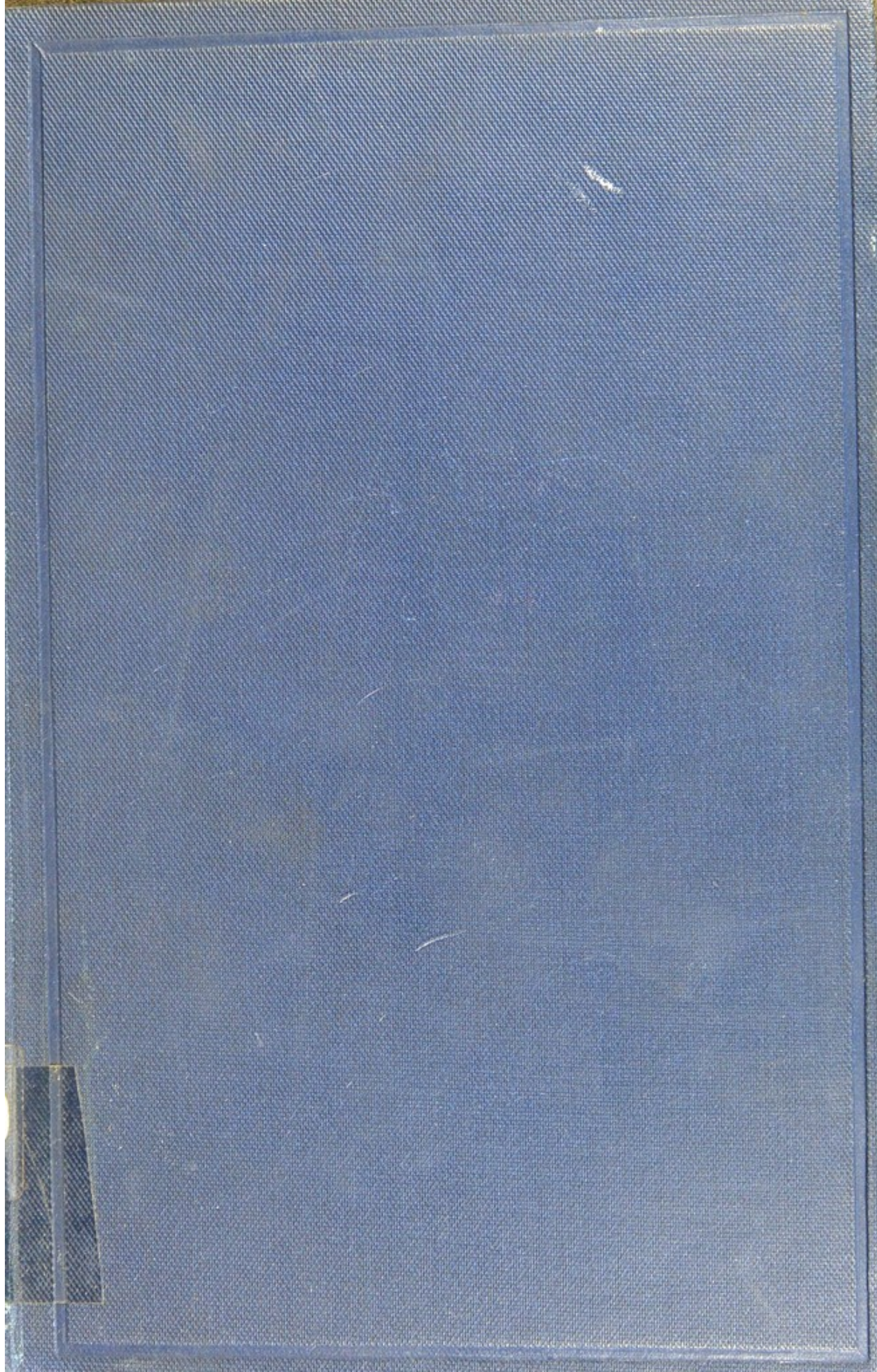
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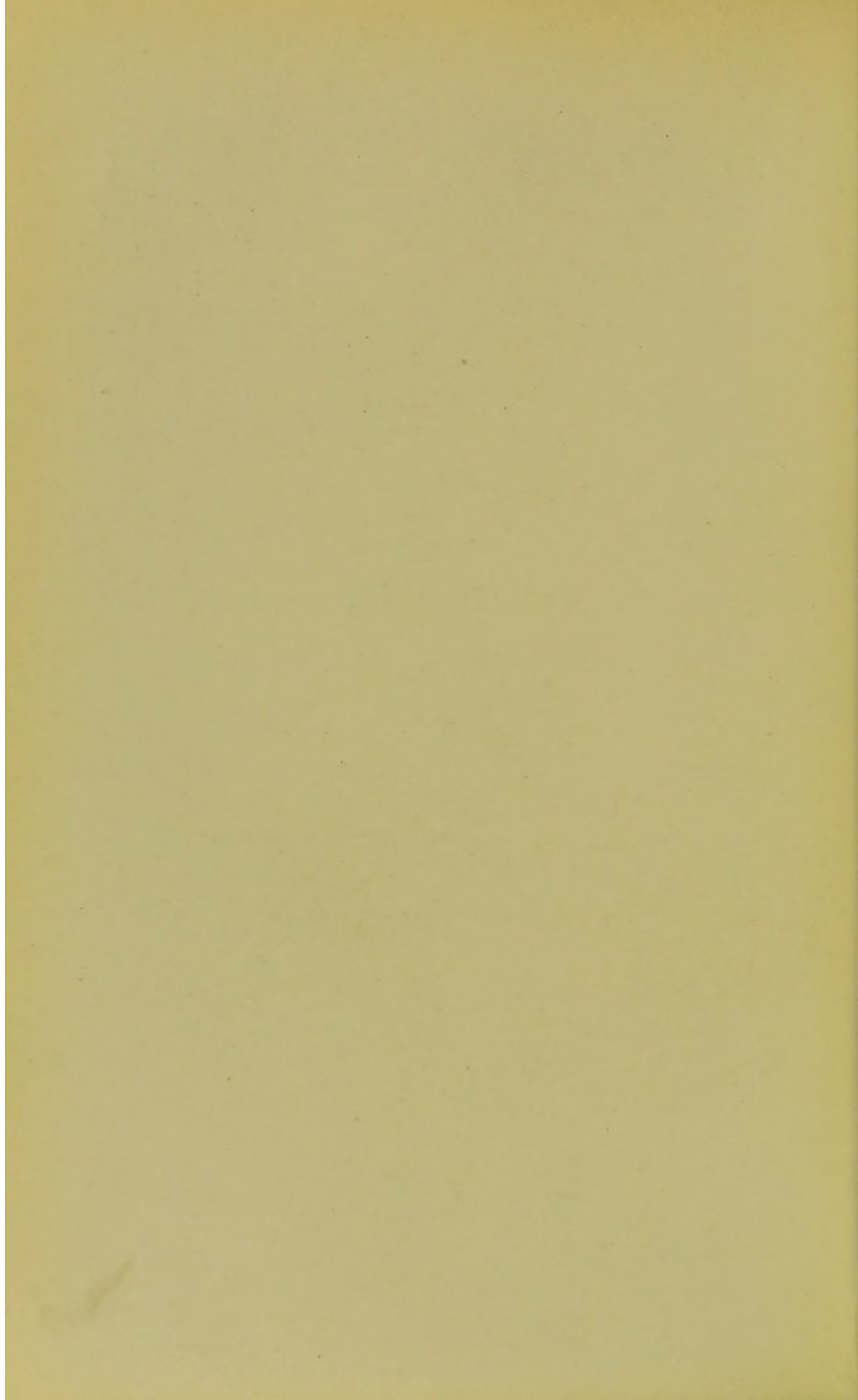
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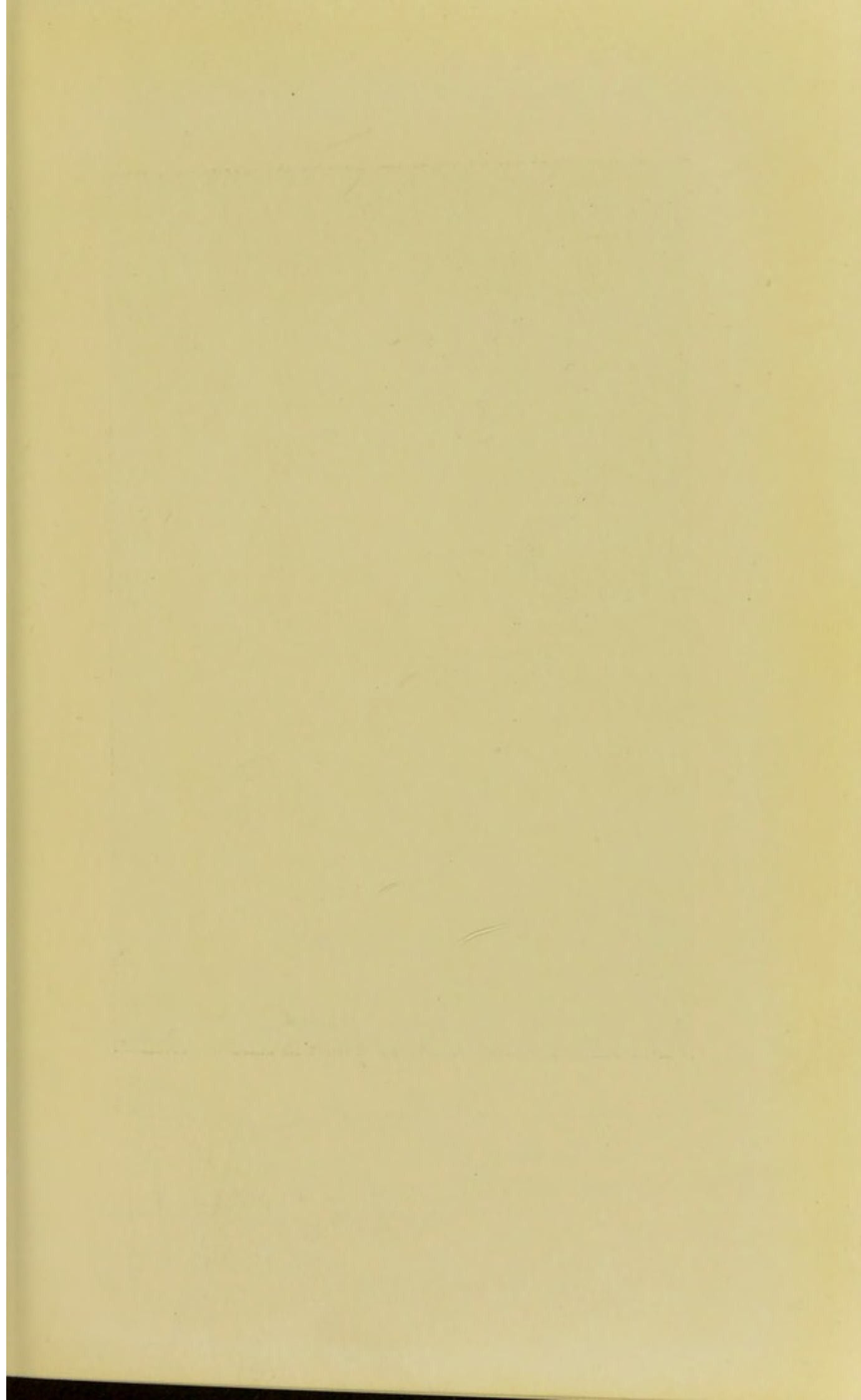
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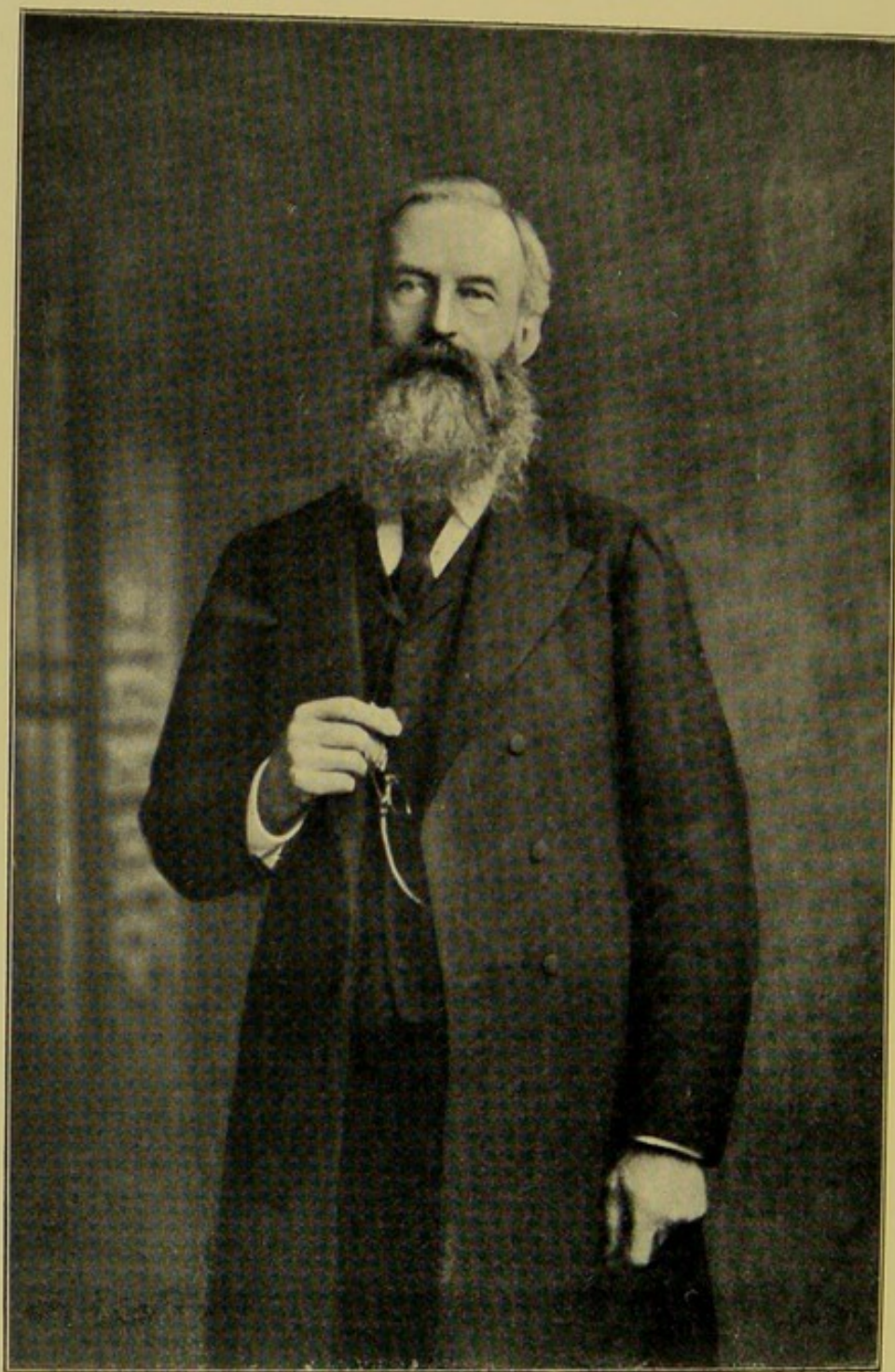
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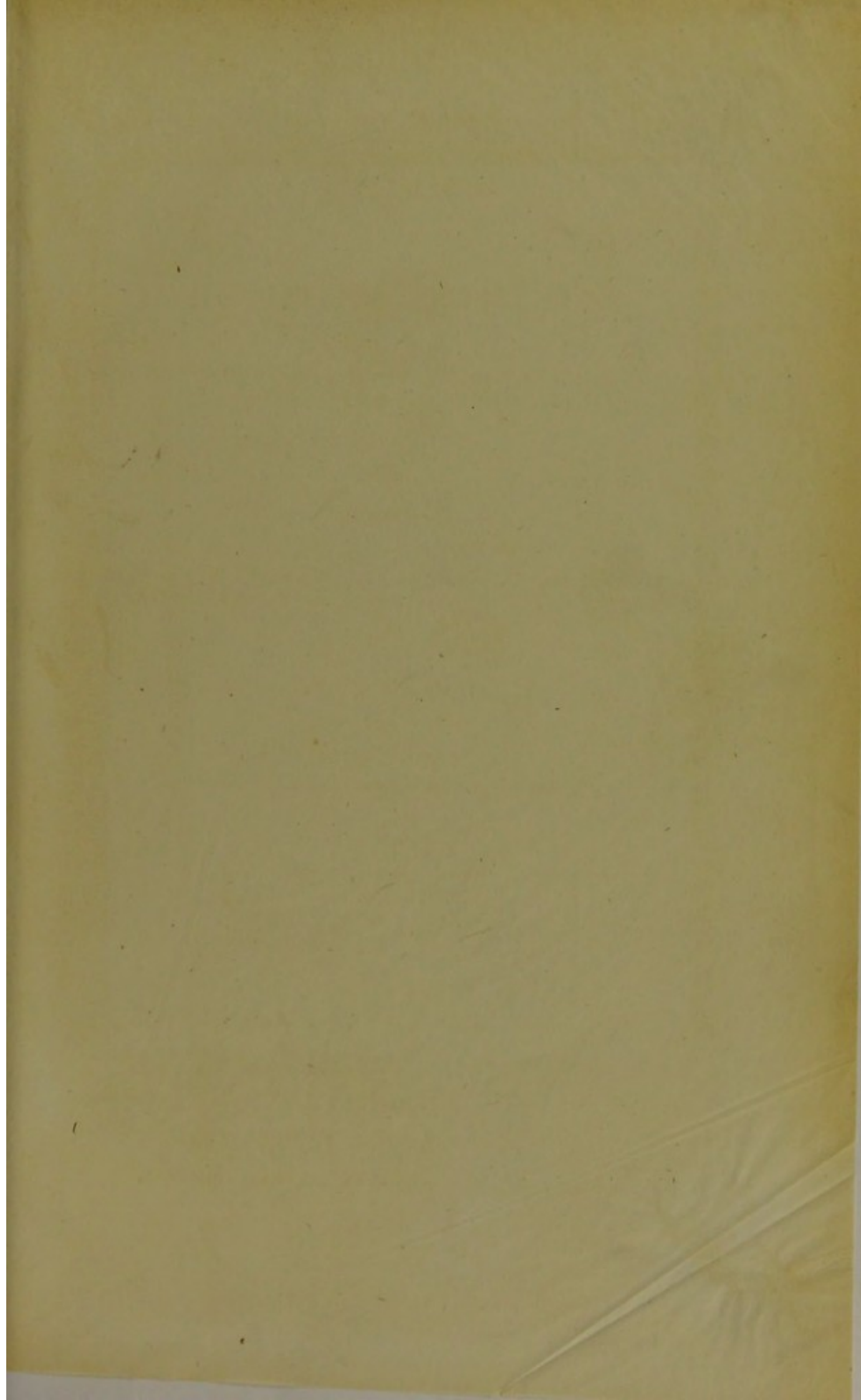
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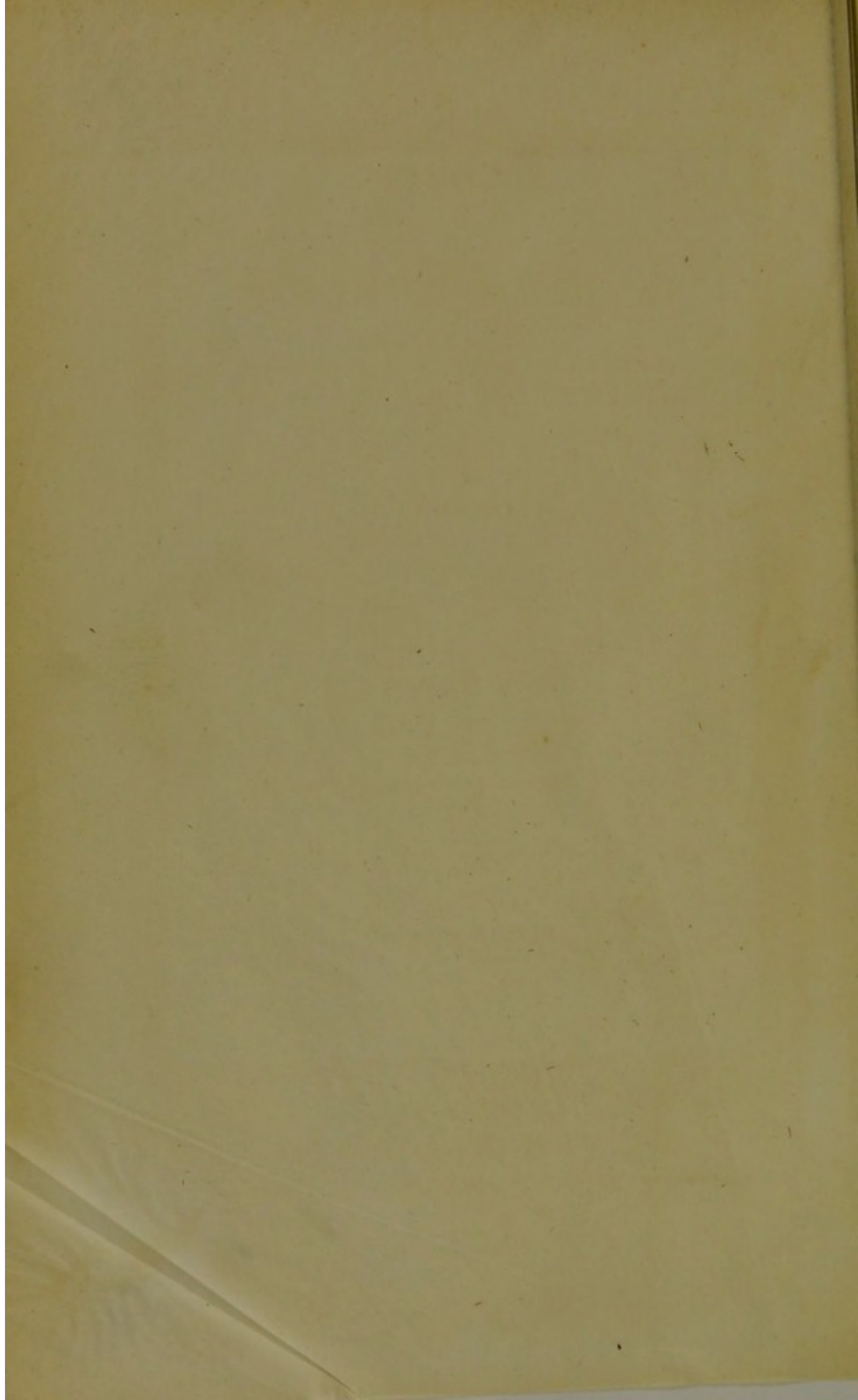






John Bayley
John Duncan





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Angioma, and Other Papers

By JOHN DUNCAN
M.A., LL.D.

*Late Fellow of the Royal College of Surgeons, Edinburgh, and Consulting Surgeon to
the Edinburgh Royal Infirmary*

EDITED BY

JAMES HODSDON

*Fellow of Royal College of Surgeons, Edinburgh; Assistant Surgeon to the
Edinburgh Royal Infirmary; and Lecturer on Surgery, Edinburgh*

WITH MEMOIR

BY

JOHN CHIENE

Professor of Surgery in the University of Edinburgh

WITH PORTRAIT

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P R E F A C E

WHEN Mr Duncan ceased to be a member of the Acting Staff of the Royal Infirmary, he began the preparation of a book on Clinical Surgery, in which he would have placed on record his experience, extending over nearly thirty years, as a Hospital Surgeon and Teacher.

This volume contains MSS. of the proposed book, which was placed in my hands by Mr James Duncan after his father's death, with permission to deal with it as I considered advisable. Certain portions of the MSS. are not completed, but are, it is believed, of sufficient interest to warrant their publication. I have included in the volume a selection of papers previously published, together with a biographical sketch of their late author by Professor Chiene.

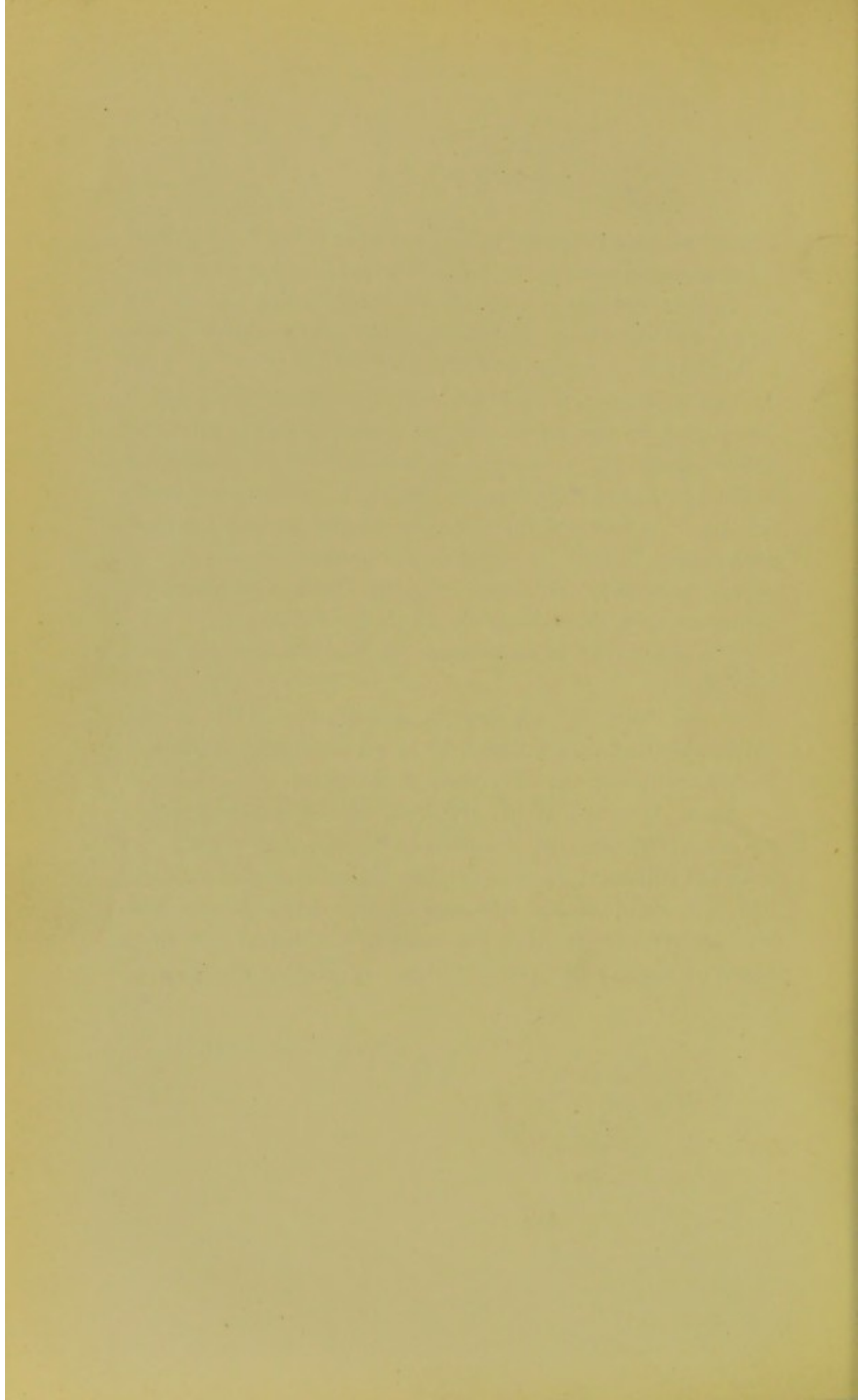
The publication is undertaken by the son in memory of his father.

It has been my privilege to arrange the MSS. of my old teacher and valued friend, to select previously published papers, and to see the work through the press.

To Mr Duncan's pupils the book will, it is hoped, prove an instructive and useful memento of their old master; to others it will serve to indicate the logical and philosophical line of thought of one who was an able surgeon, and who was regarded by many as the most accomplished and most scientific teacher of Clinical Surgery of his day and generation.

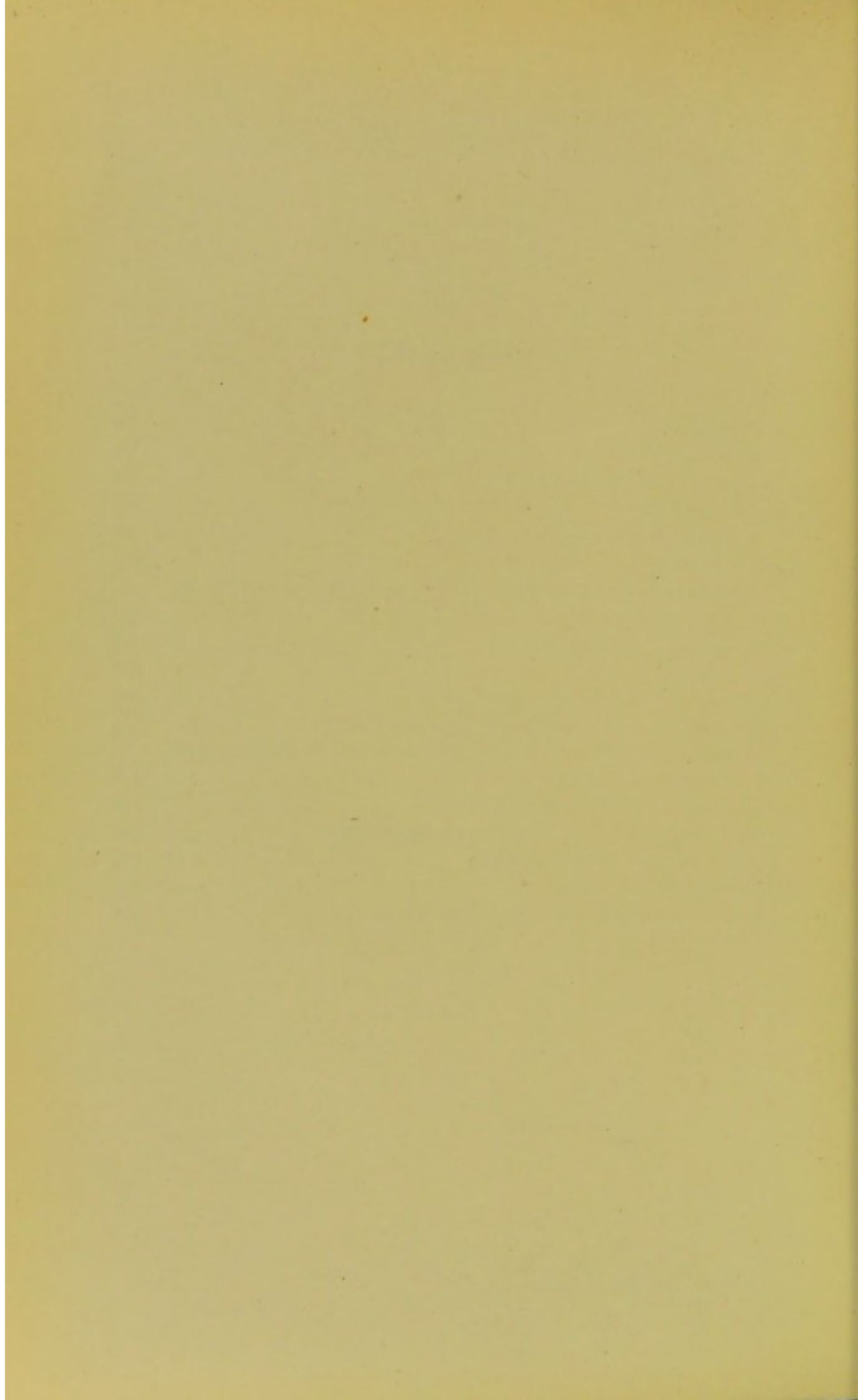
JAMES HODSDON.

6 CHESTER STREET,
May, 1900.



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MEMOIR

R.M.S. "BRITON,"

26th March 1900.

WHEN asked some weeks ago by the editor of the collected papers of the late John Duncan to write a short preface to the book, the writer little expected that this would be done on the way to Cape Town with the Edinburgh South African Hospital. Surely, however, the time and place are not inappropriate, when the personnel of the Hospital includes many of Duncan's old pupils and nurses who were trained in his wards.

John Duncan was born in Edinburgh on the 18th of August 1839, and died in the Island of Skye on 24th August 1899. His father, Dr James Duncan, was an eminent and most popular practitioner of Medicine in Edinburgh, and enjoyed the confidence and esteem of an enormous clientele of his fellow-citizens, by whom he was much beloved. He, like his son, died away from home when he was on holiday, in 1866, greatly mourned by his numerous and attached patients.

John Duncan's mother was a sister of the late John Hutton Balfour, Professor of Botany in the University of Edinburgh. Duncan was educated at Dr Graham's School in Queen Street, and at the High School. He left the latter Institution when 15, and entered the University in order to take the degree of M.A. His original choice of a profession

was that of the Scottish Bar, but with that keen sense of what he believed to be his duty, and consideration for the wishes of others which characterised him throughout life, he abandoned the idea and devoted himself to the study of Medicine. His father's practice having become overwhelming, and his younger brother having determined to enter the army, Duncan was convinced that he should follow the profession of Medicine in order to assist his father.

Many tales might be told of Duncan's student days. I do not think any one could ever accuse him of over-attendance on lectures; he absorbed systematic knowledge and worked hard in the wards. Undoubtedly he was a leader amongst the students of his day. At the age of twenty he led the Conservatives in the first Rectorial Election in 1859. Feeling ran high, but never a word was breathed against John Duncan. He was recognised as a Bayard without fear, a doughty but generous opponent. He did not lead his men to victory, but the defeat was not an inglorious one.

I cannot omit, in speaking of Duncan's student days, a word about "Mac's," a tobacconist's shop opposite the College, where cronies used to meet around an old table, and smoke the pipe of reflection. This was a favourite haunt of John Duncan's. He was looked upon as the perennial chairman. In after years he was a valued member of the Round Table, Medico-Chirurgical and Æsculapian Clubs, and his love of the social side of club life was probably fostered by close attendance at "Mac's."

While yet a student, Duncan was elected Senior President of the Royal Medical Society, and took much interest both in the public and private business of that body. He had been a distinguished member of Professor Aytoun's class of Rhetoric and English Literature during his Art's course, and he gave his presidential address "On Style."

It was not until after Duncan's graduation in 1862 that

I came into intimate association with him. He was Syme's House Surgeon, which was the blue ribbon on the surgical side of the old Royal Infirmary, and I was one of the six dressers. I can speak of Duncan from a dresser's point of view; ever kindly and considerate, he took trouble with us, he taught us, he was loyal to us, and we to him.

Sir Dyce Duckworth, one of Duncan's contemporaries, has written of his fellow-resident in the following terms:—*
“My memory of him goes back just forty years. He was then, physically and mentally, an outstanding figure amongst us. He always appeared to take life easily, while systematically diligent and regular. His strong and cultivated intellect readily grasped and assimilated what was to many of us complex and difficult, and he stored up his knowledge, gathered in the class-rooms and in the Infirmary, together with other mental aliment in the simplest and most gracious manner. His charming temper, good nature, and his sense of humour, grafted on a handsome and commanding presence—the latter barely concealing immense physical power and agility—made him the beau ideal of a companion and *bonus comes*.

“I lived for a year in close intimacy with him as a Resident Physician in the Infirmary, and it is now among the happiest memories of my life to have known him. John Duncan's sturdy intellect and well-balanced and even temper clearly marked him out as a leader of men. He abominated shams, discarded nonsense, and had what too few men have nowadays—the courage of his opinions. He could accomplish anything he set his mind to. We admired him, we loved him, and now we feel that we have lost something that can never be replaced in our time.

“It is well for the public, and still better for the pro-

* *Scotsman*, 29th August 1899.

fession, to have men of this mould in their service. No higher object-lesson can be set to the medical undergraduates of Edinburgh, than to point the moral of the career and qualities of dear John Duncan. *Requiescat in pace!*"

Duncan, with D. J. Simpson, Sir J. Y. Simpson's eldest son, and the late Professor William Rutherford, visited Berlin and Paris in 1864-65. I remember on his return from the Continent that he brought Chassaignac's drainage-tubes to Mr Syme.

Duncan became Assistant Surgeon to the Royal Infirmary in 1868, full Surgeon in 1877, and Consulting Surgeon in 1897. He lectured on Systematic and Clinical Surgery. The following extracts from obituary notices written by old House Surgeons serve to indicate the estimate formed of Duncan by his pupils* :—"With his physical and mental endowments, Duncan was an ideal surgeon; at the bedside, or in the operating theatre, his fine presence and deliberate speech were eminently characteristic. He laid more stress upon knowing what was wrong and why anything should be done, than upon the mechanical details in carrying it out. As an operator he was calm, precise, deliberate, and thorough, and yet when occasion demanded, no one could be more rapid; it might almost be said of him that he rejoiced in difficulties."

"He did not go out of his way to make his lectures popular; he cared little about diagrams or blackboard sketches, and he never relied upon oratorical or dramatic methods to convey impressions to his audience; he discussed rather in a subdued strain, really thinking aloud, and with a culture of diction and wealth of reasoning that made his lectures an intellectual feast."†

* *British Medical Journal*, September 9, 1899.

† Mr Alexis Thomson, in *Edinburgh Medical Journal*, October 1899.

"For myself, I look back upon the days of my discipleship with pride and affection. I more and more admire the dignity, the nobility, and the all-round effectiveness of the surgical teaching in which I was privileged to share as a pupil. To follow Duncan was to witness the constant revelation of a mind of a very high order, and the note of distinction appeared in every feature of it. I think that Duncan's teaching was, indeed, the greatest educational influence I experienced during my course, and it has been a lasting inspiration. Over the globe are scattered many men—not all surgeons—who to the end of their days will carry the imprint of John Duncan's personality."*

He was a candidate for the Chair of Surgery when I was appointed Professor, and I can truly say that my only regret was Duncan's disappointment. His action was characteristic of the man; he was the first to write and wish me success as a Professor.

I cannot dwell upon the years we spent together, without one jar, without one cross word, ever helpful to each other. In help I got more than I gave, because I needed it more. Duncan, however, in any difficulty often spoke to me; as for the help he was to me in a difficult case or in a difficult situation I cannot say how much I owe him. Cautious and not too ready to speak, his advice was pre-eminently sound and trustworthy.

Duncan was the leading spirit in the Extramural School, and it was only fitting that when the teachers associated with the Royal Colleges became united as the Royal Colleges School of Medicine, he should be chosen as the first chairman of the governing body.

A prominent member of the Edinburgh Medical School

* *British Medical Journal*, 9th September 1899; written by an old House Surgeon, now in Sydney, two years before Mr Duncan's death.

has written as follows: *—"The prospect of a Commission to legislate for the Scottish Universities seemed to Duncan to promise great things for the Edinburgh School of Medicine, and we well remember how he used to point out that in the University and in the group of extramural teachers, all lecturing in close proximity, material was at hand for the formation of one of the greatest medical schools in the world. His idea was practically to incorporate the Extramural School with the University, using its members as teachers and as examiners in return for the privilege of teaching for University degrees. He worked hard in advocating these ideas and in supporting them before the Commissioners." His disappointment was correspondingly keen when his views were not carried out.

As President of the Royal College of Surgeons, much was expected of him, and he did not disappoint the Fellows. Calm, judicious, and courteous, he upheld the best interests of the College during his term of office. Naturally averse to effort, it required a strong stimulus to make Duncan use his full powers, but once interested, he went through the work with energy and ability.

Evidence of Duncan's versatility is afforded by the fact, that when appointed a Director of the Commercial Bank he applied himself to the study of finance, and gathered such a knowledge of the subject as to render him an important and useful member of the Board.

He declined more than once the offer of what would have been a safe seat in Parliament, as member for the Universities of Edinburgh and St Andrews.

Socially he was a kindly and genial host, hospitable and delighted to see his friends about him, both in Ainslie Place and at his country house.

May it be said of us, as it can be said of him, that he has

* *British Medical Journal*, 9th September 1899.

left us an honest, honourable record sheet ; in him we have lost a true friend, a genial comrade, and a philosophic and trusted surgeon.

"He was as gentle as he was strong. His apparent calmness and his characteristic laugh concealed a sensitive and even diffident nature. One could not be long in his company without feeling that here was a manly man—broad-minded and whole-hearted ; ready to feel, able to do. Nothing petty or insincere could live in his presence ; he towered above such things, he looked down upon them, and they shrank away. He dedicated his gifts to the highest uses, and truly may be it written of him, that he was one who loved his science and served his fellow-men."*

I cannot lay down my pen without a word about Duncan as a cricketer, a curler, and a golfer. He kept wickets for Scotland, and was a steady bat. He was a keen and skilful curler and excellent skip, who always got the best out of his rink. Whether playing a losing or a winning game, his characteristic laugh could be heard all over the ice. He was at one time President of the Coates Curling Club, where his presence was welcomed by all. All last winter, whenever we met at Coates pond, never a day passed without some one expressing how he missed John Duncan.

He began golf in his later years, taking it up with all the zest that he displayed in those earlier amusements in which he was so proficient. He was a keen player, never knew when he was beaten, and always seemed astonished when he failed to hole a twenty-yard putt. In the Annual Inter-College Match he took the greatest interest.

He was an excellent rifle shot, and won the Edinburgh City Cup in the sixties. Year after year he went to the Highlands for his autumn holiday ; he was a deadly shot both at grouse and deer, and few men could throw a better

* *British Medical Journal*, 9th September 1899.

salmon fly. An artist of no mean order, interested in the study of geology and of astronomy, a successful collector of entomological specimens, his recreations were not confined to sport alone.

I cannot do better than close with the words which Sir John Batty Tuke—one who spent so many holidays with him—delivered in his valedictory address as President of the Medico-Chirurgical Society:—"With John Duncan died a strong outstanding personality. It was not his professional attainments, great though they were, that gained for him pre-eminence amongst us, so much as the pure judicial quality of his mind, the independence of his character, and the manliness of his disposition. These qualities, carried into the conduct of business of whatever kind, made him the trusted adviser of all sorts and conditions of men, whether they were politicians, financiers, members of his own profession, or dependents. His old gamekeeper wrote when he heard of his death:—"He was a noble gentleman, and the kindest of masters." To my mind, Duncan's walk and conversation is well expressed in the words of a great living poet:—

'So walked he from his birth,
In simpleness and gentleness, and honour and clean mirth.'

JOHN CHIENE.

ANGIOMA.

PROBABLY the most elastic and comprehensive classification of the angiomata is the anatomical. Undoubtedly the varieties run into one another, but typically, and in practice, they are perfectly distinct. I should divide thus:—

Angioma	{	Capillary	·	{	Cavernous Tumour	} Telangiectasis.
					Erectile Tumour	
		Venous	·		Phlebectasis.	
		Arterial	·		Cirroid Aneurism.	
	{	Lymphatic			Lymphangioma.	

Of these the second and third generally retain their original characters, but the first has undoubtedly a certain tendency to extend backwards to the arteries, or forward to the veins.

Capillary Angioma.

The capillary angioma is best considered under two heads—the Congenital and the Non-congenital. Surgeons and pathologists, indeed, have constantly described these two forms of telangiectasis as one, and have too much ignored the many points of distinction in their histology, clinical history, and treatment. I shall take them separately.

Nævus. Congenital Telangiectasis.—This may be subdivided in two ways, according as it is regarded from the purely pathological, or from the clinical point of view. On the one hand, we have the simple, the venous, and the arterial nævus; on the other, the cutaneous, the subcutaneous, and the mixed. We shall presently see the value of these distinctions.

Structure.—If after excision a purely subcutaneous nævus be examined with the naked eye, on section it will be seen to resemble very closely a finely reticulated sponge. The reticulation is very even, the blood spaces are much of one size, the average being about as large as the head of a small pin. Individual examples vary somewhat in this respect, some having a wider, some a more irregular reticulation than others. It will be found impossible with the utmost care to dissect out any distinct blood tube, on account of the constant intercommunication of one with the other. So perfect, in fact, is the reticulation that in whatever direction the section be made the lumina appear always to be cut transversely. There is no sign of a vessel running parallel to the section or cut obliquely.

The laminæ between the blood spaces are thin but tough, and smooth and shining on their surface. Here and there through the section denser white lines may be seen imperfectly dividing the tumour into lobes. It is not possible to separate between these lobules, and, indeed, they are often entirely absent. On microscopic examination, the laminæ are found to be fibrous in structure, lined by an endothelium, which is continuous with that of the blood-vessels.

By some recent authors these tumours are described as invariably surrounded by a distinct capsule, and on this point it is necessary to be perfectly clear, because on it has been founded a definite practice.

There are two modes by which a nævus may increase in bulk—the intrinsic and the extrinsic. It may grow by the invasion of surrounding parts, or by pushing them aside through addition to its own internal bulk. I have seen many a nævus run a course which entirely precluded the idea of its increase being otherwise than by involving the surrounding textures in its own mode of growth. Take one example: A child, a patient of Dr Graham Weir, was born with a small mark on the temple exactly like a bruise, and which, indeed, was supposed to have been produced by the forceps used in delivery. But its true character was soon evident, for it grew with such rapidity that in a few weeks it covered one-half the face and palate, occupying the whole thickness of the cheek, greatly distorting the ear, and by enlargement of the eyelids

entirely closing the eye. With surgical assistance, a process of involution was set up, and in two years not a trace remained in swelling, colour, or to touch, except a convergent strabismus from prolonged disuse of the eye. It is, of course, absolutely impossible that such a growth can have progressed otherwise than by increase or enlargement of the pre-existent capillaries of each part as it was successively attacked. I do not think that one is in a position to formulate any theory as to the meaning of this spread by the margins, which is singular among innocent tumours. Usually a portion of tissue, taking on an abnormal development, simply grows. It does not induce its neighbour to grow likewise. It may even by pressure induce atrophy. But there are exceptions. There is the continuous fatty tumour which is most common on the back of the neck, and there is this telangiectasis. It is difficult to understand the nature of the influence exerted.

In this mode of growth the formation of a capsule cannot take place. The tumour simply melts more or less abruptly into its surroundings. Nor is this by any means the least common mode of extension.

Again, the purely subcutaneous nævus is comparatively rare. The large majority of subcutaneous nævi involve the skin to a greater or less extent. Now, so far as the skin is concerned, capsulation is of course impossible. In the purely cutaneous there is no capsule whatever, and in the mixed the skin is perfectly continuous with the tumour.

But in many cases undoubtedly a capsule does exist, and there are plainly certain conditions which conduce to it. There is first the fact that sometimes the internal growth may be too rapid for the external invasion. We have in this respect an analogy with the progress of some malignant tumours, which, like nævus, invade their neighbours. In the subcutaneous sarcomata, for example, there is often a wonderfully perfect encapsulation from this cause. So the nævi push aside so fast that they have not time to invade, or are so slow in invading that they cannot help pushing aside and condensing. Next we have a tendency to encapsulation from the natural anatomical relationships of parts. Here, again, we have an analogy in the behaviour of malignant tumours.

They are slow to pass beyond the tissue or organ in which they began. They encapsule themselves, therefore, with regard to a new structure, and especially to one which is dense, while they extend rapidly in the old, whether it be bone, gland, or viscus. So the nævus. It may become adherent, and may involve fascia. The former occurs under pressure, the latter is very rare. But the anatomical separation, except when inflammatory adhesion arises, is usually distinct. You can strip them off the fascia by tearing, or with your finger. Yet again, a capsule is formed when a subcutaneous or mixed nævus is old and retrogrades. There is then no longer any tendency to invasion, and round a nævus, as round a bullet, the cellular tissue tends to condense.

So far, then, as the capsule is concerned, we find its presence to be irregular. It is never to be found in the cutaneous portion, rarely in a nævus which is extending with rapidity, but the deep surface of the tumour is usually well defined, and if it be purely subcutaneous it is often invested by a capsule throughout.

I have said that many nævi melt into their surroundings. The margin, however, both in skin and deeper structures is usually pretty abrupt, whether it be encapsuled or not. Immediately beyond the area of capillary change the tissues are normal. The arteries which supply the growth are very small, but fairly numerous, and the veins, though larger in proportion, attain usually no great size.

But in the vascular arrangements there are many modifications. Thus it is not unusual for a congenital nævus to pulsate from the beginning; and I have dissected one of them in which it was possible to isolate, to a certain distance, several tortuous and dilated arterioles throughout the tumour. This may be called the arterial nævus. I have never, however, in the congenital tumour seen any form which corresponds to the cirroid aneurism, or even to the aneurism by anastomosis of the adult. Similarly there is a venous nævus, and by the colour it is often possible to say whether, in a particular example, the blood is more largely arterial or venous. When the skin is affected in the mixed variety, it is generally easy to trace in it the enlarged venous radicles

running over the tumour. This is doubtless chiefly structural and in the nature of the growth, but may also arise from the same cause which produces venous dilatation in the skin over any rapidly increasing tumour. But there is also a form of congenital nævus in which the very existence of the capillary metamorphosis is masked by the enormous dilatation of the veins. A most remarkable example of this form was exhibited by my colleague, Mr Joseph Bell, to the Medico-Chirurgical Society. The patient was a man of 27. He was, according to his own account, born with dark blue marks on his left shoulder and arm, in which large veins were clearly noticed, and for this was taken to Dr M'Call Anderson when still a baby. There had been no change in the appearance of his arm, except a slight extension of the disease, within his recollection. He had fractured the left humerus when twelve years old, but the bone had united perfectly. The skin and subcutaneous tissue from below the left elbow to the shoulder, on the inner and anterior surface, and stretching across the root of the neck to the middle line, were occupied by a mass of tortuous and dilated veins. Mostly they were soft and compressible, with contractions and enlargements, much as is seen in varix of the leg; but here and there phleboliths could be felt, and the skin in many parts was distinctly nævoid. In parts also a firmer texture was to be distinguished subcutaneously, which, I think, could only be of cavernous structure. He had applied for relief on account of a swelling under the right sterno-mastoid, which had been coming and going for two years, and which probably was an extension of the growth. Its immense extent, its continuance into adult life, and the size of the veins, which in many parts were as large as the forefinger, made it a most exceptional case.

Pathology.—The pathology of the disease is not altogether clear. Presumably the process of formation is somewhat as follows:—The capillaries dilate, and in the process of dilatation the intervascular tissue becomes compressed and atrophied. This atrophic action is shown in many ways, even if it might not be predicated *a priori*. Over a nævus on the scalp, for example, the hair grows sparsely, short, and thin;

and in other parts the cutaneous follicles and glands in great part disappear. In some cases, indeed, this goes so far that the spontaneous or surgical cure of a *nævus* leaves behind a condition of skin somewhat scar-like in appearance. This atrophic, semi-inflammatory action advances to the destruction of such capillary walls as may be brought into immediate contact with one another, and so opens up communication between them. Such abnormal communications are known sometimes to open up even between large arteries and veins. It seems not unlikely that the cases of aneurismal varix without traumatic origin are of this character. At least it is known that abnormal vascular communications may form in a quiet way and perfectly direct. It is, therefore, from analogy probable that the reticulation of a *nævus* is thus brought about. I have myself only had the opportunity of examining those in which the meshwork was completely formed, and in which there remained no possibility of recognising the original capillary structure. This is not the case in the cutaneous forms. In them there is often a pure multiplication and dilatation. It is easy to verify this fact by putting parts on the stretch. You have then displayed a most beautiful network of visible capillaries, often in parallel rows, with perhaps a winding arteriole or dilated vein. But other observers acknowledge the intermediate stages in all varieties. Billroth, for instance, describes the plexiform and cavernous. Recklinghausen, Paget, and others, while also pointing out that some *nævi* consist of a mass of tortuous and dilated capillaries and others of erectile tissue, explain that in the former there is multiplication, not merely dilatation of vessels. They describe this multiplication as taking place in two ways: by the extrusion of buds from the pre-existent vessels, and by the formation of new blood-paths in the tissues. Rindfleisch holds the process to be a cell infiltration, possibly inflammatory, of the connective tissue, which by subsequent contraction drags upon and dilates the vessels.

This last explanation of the pathology has difficulties in the way of its adoption. At the same time, the *nævus* has distinct relationships to inflammation. It is, for example, peculiarly prone to become inflamed under such causes of

inflammation as friction or traumatism; and we are in the habit of utilising this tendency for the purpose of cure. In the cutaneous form, again, which is specially apt to last into adult life, and so give time for such developments instead of an atrophy of the intervascular textures, there is frequently an overgrowth. This, in some instances, assumes the form of fibrous nodules and outgrowths, while in others an association may be noted with another variety of congenital aberration, the hairy mole.

Clinical History.—Nævus is generally described as congenital. In some cases, indeed, it is so. But, as a rule, there is nothing visible at birth, or it may be only a slight stain resembling a bruise. It declares itself, however, in a few days, a fortnight at the latest; and it may be said that it does not commence after that period. It is very variable in its rate of growth. At first it may be exceedingly rapid. It may attain a very large size, but it does so early, if at all. After the first few months of life its increase is generally proportional to that of the rest of the body, more often less than more. Frequently it remains without increase.

There is always a great tendency to spontaneous disappearance on the part of the subcutaneous and mixed varieties. Certainly more than half are thus naturally cured. It is, indeed, rare to find in the adult a nævus of this kind. This no doubt is partly due to the interference of the surgeon, which is specially successful in these forms, but also arises from spontaneous involution, to which probably the surgeon's success may in part be attributed.

There are instances, however, in which the disease persists through life. The cutaneous variety, for example, the portwine stain, as it is called, is exceedingly persistent. This is especially the case on the face, and notably when it is associated with moles or hypertrophies. It is unfortunate that it is precisely in these cases that the surgeon's art is least available, the persistence being partly a cause, partly a consequence, of that fact. But the subcutaneous and mixed varieties also sometimes, though rarely, fail to go away. I have already referred to Dr Bell's unique case of venous telangiectasis as a notable example. I describe elsewhere

subcutaneous nævi which develop arterial connections in adult life. It would seem that the subcutaneous tumour is prone to outlast adolescence in proportion as it may be associated with arterial, venous, or, as we shall presently see, some forms of degenerative metamorphosis.

The cure is evidently brought about through a fibrous change. In a large number, I think, I can predict, with tolerable certainty, whether or not they are about to disappear. An advancing nævus is characterised by the vividness of its colouring, by its softness and compressibility. On the other hand, if it be about to retrograde it is distinctly firmer; and this induration is somewhat irregular and nodular, parts of the growth soft, parts hard. There is an inflammatory element in it, followed by gradual atrophy, produced, no doubt, by constriction of the vessels through fibrous contraction of the intervascular inflammatory products. I have already pointed out that the nævus is sensitive to inflammatory causes. In a child a few months old, a nævus surrounded the elbow and attained a very large size. Notwithstanding the utmost care, the folds of skin which lay in contact with each other ulcerated, and the ulceration began to spread over the rest of the swelling in a most alarming manner. Fortunately the growth of the nævus was arrested by electrolysis, and in the end, I think, the cicatrisation of the ulcers hastened its disappearance. Under friction from the clothing also, the cutaneous portion will sometimes entirely disappear. It is exceedingly common to see a nævus the margin of which is of a brilliant red and distinctly spreading, while the central part is of a dull livid tint, or perfectly white. This is no doubt due in large measure to the fact that the more prominent portion is that which is most exposed to external sources of irritation. At the same time, a nævus cures from the centre habitually; and it almost looks as if, like other parts of the body, it had a definite life-history, the older parts undergoing involution while the younger evolve.

There can be no doubt that the disappearance of a nævus is largely influenced by the period of life. It sounds paradoxical, but it is true, that the critical period of the first teething may lead either to increase or diminution; and the more

severe the constitutional disturbance, the more distinct are its effects upon the *nævus*. The second teething and puberty also are not without influence in conducing to atrophy of these growths. Perhaps, however, the rule is that, modified by these conditions and accidental circumstances, the *nævus* gradually fades under independent tendencies inherent in its own nature. In some cases, injury or irritant, accidental or surgical, starts the process; in some it seems entirely unprovoked; and there are instances of *nævi* which, after resisting many efforts to bring about their cure, have quietly obliterated themselves after these efforts have long ceased.

But other changes may occur than those already detailed. The atrophy may proceed irregularly, and portions of the tumour becoming blocked off from the circulation may form cysts. These cysts present contents, in the shape of altered blood, which differ little from those of cysts arising in consequence of blood extravasation. Perhaps this fluid is usually more thin and watery, and I have not often found the deposit of fibrin round the walls which is seen in *hæmatocele* or extravasation into cellular tissue. They are most often multiple, of varying size, and are surrounded by a quantity of fibrous tissue, the cicatricial remains of the rest of the *nævus*. I am not sure that these cysts are always blocked off and subsequently dilated blood-vessels, because on one occasion I found a cyst whose contents were purely serous mingled with others containing a dark fluid. It is probable that in some cases at least they are simply an example of the cystic degeneration which may attack every form of tumour.

Probably the most frequent degeneration, however, is the fibrous. The *nævus* undergoes the fibrous change which I have described as the precedent to natural cure; but the subsequent atrophy and involution fail to take place, or do so only imperfectly. It forms a tumour, then, of no great density, but resembling rather a somewhat closely-packed connective tissue, more or less distinctly encapsuled.

Cases have been recorded in which a sarcomatous change has taken place. Considering the association of malignant disease with different forms of local irritation, it is impossible to deny the possibility of such an occurrence. It is certainly

rare, and I have not met with an example in nævus unconnected with the pigmented or papillary mole.

Etiology.—We really know no more of the causation of nævi than we do of most forms of abnormal local growth. Some have been attributed to injury; but, independently of the *a priori* improbability, there is reason to believe that the mistake has arisen from the resemblance to a bruise which they sometimes present in their early state. Injury may modify them undoubtedly, by the production of inflammation, by changing a simple into a pulsating nævus through rupture of arterioles, and possibly in other ways, but I do not think that it can produce them.

There can be no doubt of their hereditary character. As might be expected in a growth plainly developmental in character, the tendency of the parent is in this respect often impressed on the offspring. I have known many illustrations.

The influence of maternal impression I cannot regard as in any case adequately proved. It is rare to have the suggestion made even by the mother; and when it is, inquiry will in the greater number show it to have been an after-thought.

Symptoms.—The appearance of the cutaneous nævus will vary much. The name of "port-wine stain" indicates the prevailing tint with tolerable accuracy. But just as the colour of the lip or cheek varies in different individuals, so the nævus may present every tint from purple to brilliant red, though usually there is a considerable dash of blue. The blueness is greatly increased in many cases by cold, and in all may alter from time to time under the influence of temperature, exertion, and state of health. The network may be so fine that it is impossible to distinguish the separate vessels, and impossible also, by pressure or tension, to cause the discoloration entirely to disappear; but usually larger vessels of a venous hue may be recognised permeating the prevailing redder colour, and from them the blood may be easily expressed. The margin fades pretty abruptly into healthy skin, but it is common to see little peninsulas and islands of a lighter tint stretching out from the general mass. It is scarcely raised above the surroundings except in those cases to which I have

already referred, in which fibrous nodules are associated with it. The growth of hair over it varies much. When it occurs on the scalp the hair is much diminished in quantity, but on other parts of the body the fine down may be even more abundant than is natural; and sometimes, as I have already said, it presents the appearance of a hairy mole.

The cutaneous *nævus* has its special site upon the face. If it occur on the body it is usually of small extent, and shows symptoms of belonging rather to the mixed class, by tending to be somewhat elevated, and to pass deeper. It has also the more brilliant hue of that class, although it is possible that the higher temperature of the covered parts may diminish the appearance of lividity.

The mixed variety is always elevated more or less above its surroundings, if small, as a rounded swelling; if large, with a somewhat nodulated outline. In some the only alteration of colour is a certain bluish tint which shines through the integument, but in the great majority the skin is thoroughly incorporated in the growth. Sometimes it shows extensive discoloration over the whole surface, sometimes simply meandering venules; and a very frequent appearance is a margin of brilliant red, with a central part more or less of a dull whitish-blue.

It is capable of being diminished by pressure, when any irregularity of its consistence becomes more manifest; and it shows its erectile character by increasing on exertion, or when the child cries. The mixed variety is also prone to occur about the head and face, but is much more common than the cutaneous on other portions of the body.

The purely subcutaneous *nævus* is rarely large, and presents all the characters of the mixed form except the coloration. It is decidedly rare.

There is a distinct absence of symmetry in all varieties of *nævi*. On the head and face the mixed and subcutaneous varieties often occupy the middle line. The nose is a common site, and I have three or four times seen the tip converted by *nævus* into a blue, unsightly knob. They are not very uncommonly multiple, but I have never seen two symmetrically placed. Sometimes on the face they run up to the middle

line, and then abruptly stop; but, on the other hand, they also frequently cross it more or less extensively.

Diagnosis.—It is impossible to mistake those in which the cutaneous portion forms a prominent feature for any other form of disease. The mixed variety may be mistaken for meningocele and spina bifida; the subcutaneous or degenerated, for fatty, cystic, and sarcomatous tumours.

Undoubtedly the most important diagnostic difficulty arises in cases of meningocele, and I have on several occasions had these meningeal tumours sent me as *nævi*. I was on one occasion asked to go to the country to electrolyse a *nævus* which I had not seen, and found that it was a meningocele. The position was not without unpleasant features. The skin over a meningocele is frequently in appearance indistinguishable from that over a *nævus*. In both it may be quite unaltered, but in both it may present the same bluish-red hue from dilated blood-vessels; and although the meningocele is more prone to have a livid transparency, and to be traversed by large veins, yet even when these characters are marked it requires some practice to recognise them, because, after all, it is a matter of degree. The consistence of both is often very similar, and the degree of compressibility may be identical, varying according to the size of the orifice in meningocele, the rigidity of texture in *nævus*. The mere fact that the growth occurs in the middle line should always put one on guard. *Nævi*, it is true, are found in the middle line. I have seen them in every situation where a meningocele may be, from the root of the nose backwards to the occipital protuberance. Situation decides nothing. But the meningocele is seen nowhere else. In that line, therefore, lies the difficulty. Cohesion to the bone calls for additional care. If absolutely mobile it is a *nævus*. But often it is not easy to determine the mobility; and I have seen a *nævus* over the anterior fontanelle rigidly adherent to, and no doubt involving, the membrane, as it did the skin. If on compression a neck-like connection, and still more certainly if the contour of the connecting orifice can be felt, a very important point of distinction is made out. If a distinct history that it is not absolutely congenital can be obtained, it will go far to establish its *nævoid*

character ; but the difficulty of obtaining a reliable history is proverbial. Should doubt still remain, the exploring needle may be harmlessly used in either tumour. If clear fluid be obtained, and if the needle be felt free in a cavity, it removes all doubt, but it must not be forgotten that a meningocele blocked off from the interior of the skull may have its fluid reduced to trifling bulk by lymphic deposit on its walls.

The same considerations apply to the diagnosis of spina bifida from nævus. I have twice had cervical meningeal tumours sent me as nævi ; and on one occasion I had myself great difficulty in arriving at a decision in a case of spina bifida in the lumbar region. I inclined to consider it a nævus, so close was the resemblance in all characters ; but in the end its nature was made clear by an alteration in consistence and relative position, which enabled one to distinguish the cleft in the spine. I had abstained even from tapping because the consistence, firm and solid, suggested its natural disappearance in either case.

The congenital, or nearly congenital origin, the shape, compressibility, consistence, and situation, serve to distinguish a nævus, even when purely subcutaneous, from other tumours. The sarcoma as it occurs in infants is apt to be extremely vascular, and to produce discoloration of the skin. It is therefore most liable to be confounded with nævus, and of this I have seen several examples.

Treatment.—It cannot be too strongly insisted upon that no nævus in a child should be submitted to treatment unless it be clearly increasing, or, as very rarely happens, doing harm by its presence. The reason for this lies in the fact that the majority of nævi disappear spontaneously, and that, on the whole, nature's result is as good as any that can be produced by art. The justification for treatment, moreover, is not usually the danger of the disease, but its unsightly aspect. The danger attending these tumours comes from hæmorrhage, the result either of wound or ulceration. In the former case it bleeds very freely, nay, even dangerously, but this may be controlled, at least temporarily, by pressure ; and I have known one heal and become permanently cured, after a wound of considerable size produced by a blow on the head. More-

over, the chance of such an accident is very small in a young child during the period when it is still undetermined whether it will increase or decrease. If ulceration occur from friction of clothing or other cause, repeated small, and sometimes even severe, bleedings may take place; and the mere presence of ulceration, therefore, although it also will often heal, and even lead to spontaneous cure, I look upon as sufficient justification for interference. Again, it may be wise to apply treatment in such a case as this:—A child was sent me by Dr Sidey, with a *nævus*, mainly subcutaneous, of the root of the nose. I was unable to ascertain that it was increasing, but I applied electrolysis, because the swelling was inducing a squint.

Putting aside, however, these cases in which there is risk or harm from its presence, to await the natural cure is the wiser plan in a tumour which is stationary. Expectancy is better and safer than interference.

On the other hand, it is right to apply treatment should the *nævus* be enlarging. It is impossible to tell what size it may attain; and the larger a *nævus*, the more difficult is it to bring about a cure without risk and without scar.

The methods of treatment which have been employed are very numerous. I do not intend to discuss them all, but I shall endeavour to point out those which may be usefully applied under different circumstances. There is no method of universal applicability. One is better in one variety, another in another; and according to the situation and size, different means must be adopted. I shall discuss them in the following order:—

1. External application of escharotics or irritants.
2. Subcutaneous application of escharotics.
3. Excision.

I leave out of account such harmless placebos as the application of contractile collodion or other form of gentle pressure, which may be applied during the period of expectancy, and which may amuse the patient's friends, and possibly may sometimes aid the natural resolution.

1. The external application of escharotics or irritants.

There are three conditions under which this form of treatment should be adopted:—

(a.) On nose, cheek, or elsewhere, a tiny *nævus* is sometimes to be seen, not larger than the head of a pin, with little vessels radiating from it. It rarely increases; it usually goes away of itself. But it sometimes persists or grows; and its cure is so easy and simple, that I think it may be classed among those which ought to be treated whether they are growing or not. To dip a pin in strong nitric acid, and thrust it into the centre of the spot, is at once effectual. The same result may be produced by a heated wire, by a galvanic cautery point, or by the insertion of an electrolysis needle (the negative pole by preference), and the production of a little eschar thereby. The nitric acid or the heated needle are effective and simple.

(b.) There is a form of *nævus* which shows itself to be of the mixed variety by its slight elevation, but in which the subcutaneous part is exceedingly scanty. When this occurs on the covered portions of the body, it is frequently irritated by the clothes, and becomes obliterated in the centre. But at the margin there may remain a circle of brilliant red, which sometimes extends with considerable rapidity. In these cases I have been greatly pleased with the application of the sodium ethylate, as proposed by Sir B. W. Richardson, to the marginal zone. The liquor of the British Pharmacopœia is a good preparation. It is efficacious, not accompanied by much pain, and leaves a less conspicuous scar than most other powerful escharotics. If flexile collodion and salicylic wool be applied immediately, the eschar separates without suppuration, and very often a second application is not required.

(c.) Our only method of treating the port-wine stain is by means of external irritants. When it exists on parts of the body not exposed to view, it is better to leave it alone altogether. On the face it is so unsightly that an effort should be made to cure it. Unfortunately it cannot be said that our efforts are very likely to be successful. At one time I was inclined to give up the attempt, after trying multiple puncture, simple and with the cautery, Squire's multiple knife, and many other things. But as they so rarely disappear of themselves, and as I have latterly with persistence obtained somewhat better results, I believe we ought at least to give treatment a fair trial. It will require much patience, how-

ever, on the part both of patient and surgeon. Much time and care must be expended. The difficulty is to bring about a cure without destroying also the skin in which the vessels reside, and so leaving a mark at least as disfiguring as the *nævus* itself. So far as I can see, our only chance is to bring about a dermatitis severe enough to lead to obliteration of the vessels, but not to produce ulceration. One attack of dermatitis, moreover, is rarely successful, unless the *nævus* be very small and pale. We have to repeat it over and over again through a period of many months; but looking to the effects of long-continued frictional irritation, I have been encouraged to persevere, and, in some cases, have met with success. I have tried brushing with strong nitric acid, repeated blistering, iodine, perchloride of iron, and strong mercurial inunction. On the whole, I should say that iodine made the best application. The liniment, or the Edinburgh tincture, should be used and carried to strong irritation, to be repeated again and again as the cuticle peels off. The objection is the discoloration, so long continued and so conspicuous; but it is least likely to leave a permanent mark from its own action, and is at least as effectual as any other irritant.

2. Subcutaneous use of escharotics, coagulants, or irritants.

This may be done in three ways:—

(a.) By the subcutaneous injection of such substances as tannin, carbolic acid, or perchloride of iron.

(b.) By the galvanic cautery.

(c.) By electrolysis.

I have given up entirely the first and second of these methods in favour of the third. The injection of coagulants is simply a coarser way of introducing under the skin substances similar to those more definitely and effectively introduced by electrolysis. The effect of this greater coarseness is, first, that it is difficult to draw the line between doing too much and doing too little, between causing a slough of the whole mass and leaving the tumour as it was. In the most cautious hands I have seen a destruction of tissue produced which gave rise to inconvenient scarring and deformity. Secondly, unless a preliminary temporary ligature be performed, injection is attended with some risk. Several cases

of sudden death from the injection of coagulants have been recorded.

I have on two or three occasions inserted a platinum point or wire into the nævus, and, having then heated it by means of the electric current, have moved it about with the view of destroying subcutaneously the diseased texture. I do not find, however, that this method possesses sufficient advantage in the way of precision or convenience to make me advise its use in preference to electrolysis in nævus, although in certain forms of erectile tumour the subcutaneous cautery has merits to which reference will afterwards be made.

The subcutaneous destruction of a nævus, then, is best done by electrolysis, and it only remains to consider what cases are fitted for this treatment, and in what way it may be accomplished.

It is necessary, in the first place, clearly to understand what electrolysis actually does, because for our purposes there is, at least in the ultimate results, nothing of the mystery so often attached to electricity.

In the *Edinburgh Medical Journal*, 1867, my friend Professor Fraser and I gave an account from experiment of the alterations in the blood which are produced by electrolysis, and in another paper I explained its action on the tissues of the body. It may be said shortly that the effect is twofold. There is first the primary electrolysis, whereby the blood and tissues are decomposed into simple constituents, the oxygen and acids appearing at the positive, the hydrogen and bases at the negative electrode. But, secondly, these nascent substances necessarily react on anything with which they have chemical affinity in their neighbourhood, whether that be electrode or tissue; so that, for example, the positive needle in a certain time may be entirely worn away. This secondary electrolysis gives their character to the sloughs produced. At the negative electrode the slough is diffuse, large, very slightly contractile; at the positive, limited, small, and tending to condense in healing; the one corresponding to an alkaline, the other to an acid cauterisation. Practically, then, in electrolysis a nævus we introduce subcutaneously so much caustic potash and nitric acid made out of the tissues on the spot.

We introduce them in such infinitely minute division, and we so destroy tissue in producing them, that their action is purely local, and they cannot be carried into the circulation, while the small quantity produced is yet extremely potent, because it is nascent. By doing this subcutaneously we avoid the evils of putrefaction, and we conserve the skin. If properly performed, the effect is that, after a little tenderness, the slough is slowly absorbed without suppuration and without mark.

The cases in which electrolysis should be used are the mixed and subcutaneous varieties of *nævus* which are situated on exposed parts of the body, as the face or hands, and those situated elsewhere, which may not be amenable to ligature or excision. This limitation has not been sufficiently defined by those who have since adopted electrolysis, although even on first introducing the method to the profession I deprecated its indiscriminate employment.

The advantages of the subcutaneous use of electrolysis in *nævi* on exposed parts are sufficiently evident. It cures the disease without leaving the slightest trace of its action. The little needle punctures entirely disappear as a prick from any other needle does, and the absorption of the diseased structure is accomplished without the slightest puckering or scar. In bringing about this result it is absolutely safe, and the limits of its action can be more precisely defined than in any other subcutaneous injection or operation.

I have now been operating on *nævi* by this method since 1870, and I find that in that time I have subjected to electrolysis nearly two hundred, so that some idea of the value of the method may be obtained.

On an average three operations are required in each case, but experience has enabled me distinctly to diminish the number of sittings by doing more at each without damage to the skin, and by increasing the interval between them to allow of more perfect absorption, and so bring out the full benefit of each operation. In some large *nævi* I have operated eight to ten times.

I have not had one fatal result or even anxious symptom in any case. The only danger, indeed, is in the anæsthetic,

without which it is unpleasant to work in children on account of the pain. Even in adults I find it necessary to use an anæsthetic. They describe the sensation as that of a severe burn, and have always demanded it at the second sitting if I have persuaded them to do without it at the first.

The final result has not been definitely ascertained in about 10 per cent. of the cases. These, chiefly Infirmary cases, have not finally reported themselves. I am not aware of any in which there has been failure to cure, and all were improving when last heard of.

In six operations I carried the action too far in my endeavour to get as near to the nævoid skin as I could, and so produced a cutaneous slough. In two of these I then resorted to other measures. Four were cured with more or less marking by cicatrix. I may instance the case of a medical student at twenty, who had a nævus of the left auricle. It had begun to grow rapidly and become pulsatile during the past year or two. When I saw it first, the whole auricle was an egg-shaped pulsating blue mass. One operation absolutely arrested the pulsation, and reduced the auricle to its natural shape and size, with the exception of two or three nodules at the margin. In endeavouring to destroy these, I was unfortunate in producing two unsightly crescentic sloughs. The auricle was so thin that I did not succeed in my effort to destroy the vessels without interfering with the nutrition of the part.

In nine operations, which include four of these six, some suppuration occurred. All of these nævi did ultimately perfectly well, but in all I blame myself for not attending sufficiently to the aseptic condition of the patient's skin or my needles and fingers, or for carrying the action too far. Some were in pre-antiseptic days.

In five cases some trace of the former disease remains. This comes from the fact that sometimes after the tumour is cured the nævoid condition of skin remains to a greater or less extent. Experience of examples in which the colour has long continued, leads me to believe that this will probably disappear with the lapse of time. In some, a certain whiteness or scar-like appearance of the skin which had been

affected persists for a certain time, and I think will occasionally prove permanent.

These results give a general idea of the value of the method. By far the larger number of the nævi so treated were situated on the head and neck, for the reasons I have already detailed. One or two examples, in addition to those which I have published already, may be adduced to show its applicability to cases which could not otherwise have been subjected to treatment.

Thus, a little girl two years of age was brought to me with a nævus occupying the whole thickness of the cheek, extending backwards to the auricle, which it had enlarged in the upper part to the size of a walnut. It required seven operations to produce a cure, but the ultimate result was most satisfactory. Although the skin over the greater part of the swelling was much discoloured, it improved *pari passu* with the subcutaneous portion, and in the end presented very little even of that excessive whiteness to which I have before referred.

Nævi are frequent enough in the interior of the mouth. Usually they are extensions of external tumours through the thickness of the cheek, or by direct superficial continuity over the lips. They occur independently, however, and I have used electrolysis with success in the tongue, the soft palate, and the inner surface of the lower lip. In such cases a gag must be used, and it is necessary to attend carefully to the precautions for the prevention of bleeding from the little punctures after the needles are withdrawn. I had in one case to introduce a stitch.

Again, a child, aged three years, had a nævoid tumour of the foot. It involved the greater part of the outer margin, and penetrated from sole to dorsum between the fourth and fifth metatarsal bones. The growth entirely disappeared after four applications of electrolysis.

In a private case, a patient of Dr Taylor, the nævus not only occupied the soft tissues of the foot, but had occupied and distended the fourth metatarsal bone to such an extent that the needles could be passed through it as through parchment. After the cure by electrolysis, the toe, as the child

grew, became considerably recessed. It is the only occasion on which I have recognised *nævus* of bone.

A child, six months old, was brought to me from London, with a rapidly growing *nævus* of the right arm, which reached from shoulder to wrist, rendering the arm at the elbow one half larger in circumference than the other. It was specially prominent to the front and inner side, but in places extended all round. There was another growing *nævus* on the left cheek. The latter was easily cured, and the one on the arm quickly checked. It had diminished to half its size, when the parents had to return to London, where the late Mr Stevenson continued the treatment at my request. The child was brought to me four years afterwards to see if anything could be done for a faint cutaneous discoloration on the lower part of the forearm, which was all that remained of the tumour.

I have selected these cases for mention, because they are typical of varieties of *nævus*, and because I think that by no other method could they have been cured without risk and without disfigurement. The method of operating is exceedingly simple and easy, but requires some care and attention to detail to attain perfect results. It is unnecessary, nowadays, to go into minutiae as to the effects of the different forms of electricity. Any means by which a constant current can be produced will do perfectly well.

The strength of the current is of little importance, so long as it lies between 20 and 80 milliampères. With the one you work slower, with the other faster, and you determine the duration by the effect produced. But it is well in many cases to err on the side of slowness rather than quickness. The quicker you are, the narrower is the margin between sufficient destruction of the *nævus* and involvement of skin in the slough. Moreover, the near neighbourhood of important nerves ought to make one exceedingly cautious. I operated on a large *nævus* on the upper and outer part of the leg in a young gentleman who was a patient of Dr Foulis. I estimated that at no point were the needles at any time within an inch and a half of the nerve, yet the external popliteal was paralysed for many months, although it ultimately completely recovered.

That, and possibly another, to which I shall refer elsewhere, are the only cases of nerve injury I have seen. They indicate the propriety, when there is the least chance of such a contretemps, of introducing a rheostat and galvanometer in the circuit. The rheostat is also useful when you are operating without an anæsthetic, so that you may slowly increase the current from zero to the desired amount without administering shocks to the patient, which are more dreaded even than the burning pain which accompanies electrolytic action.

With these precautions, and having familiarised yourself with the battery you employ, and its action on saline fluids, white of egg, and such like, outside the body, you may safely work as I have said without rheostat or galvanometer. An ordinary medical battery is quite sufficient, if it be in working order, but I found in the Infirmary that it so frequently was not, that before our electric installation I was in the habit of getting six Bunsen cells freshly charged for each occasion, in the smaller nævi using only three or four of them. Of course with an electric supply from the town and suitable apparatus for modifying the current, the greatest convenience is attained. But where that or the means of charging a storage battery is not at hand, I feel assured that to take the trouble of charging a Leclanché, Bunsen, or Smee when it is wanted, gives satisfactory results, these having the cells shut up in a box out of sight.

The needles should be insulated with vulcanite. I know of no improvement on those originally introduced by Professor Fraser and myself. I have been told of a case in which the vulcanite covering fused during an operation. This can only have occurred because the needle in the centre had been reduced to extreme tenuity to receive the vulcanite, and the exposed points been allowed to touch in the tumour. The resistance of the thin portion of the needle must have been so great compared to other parts of the circuit as to cause it to become overheated. Such a combination is very unlikely to recur, and is easily avoided by trial before insertion. The length of the exposed point must correspond to the size of the nævus, varying from an eighth to three-quarters of an inch. For the sake of sharpness, steel is the best material; but the

positive pole, if of steel, requires resharpening after each operation, because it is acted upon electrolytically.

Both poles should be introduced, because in that way you get most work in the shortest time. I place the positive needle in a portion of the tumour where it can do no harm, and will not interfere with subsequent manipulation. I may perhaps alter its position once or twice in the course of an operation. It does little, though useful and firm work. The negative, on the contrary, I hold constantly in my hand, and move about from place to place. Its action is diffuse, and therefore the more valuable, but also naturally requiring to be watched lest it reach the skin. If the needles be left stationary, the amount of action diminishes after a few minutes, on account of the slough with which they surround themselves.

It is necessary to watch very closely the growing induration round each needle. It increases slightly even after the needles are withdrawn, and the action must be stopped before the skin is involved. Perhaps this is the only part of the operation which presents any difficulty in which experience is of value, and in reference to which it is difficult to convey the results of experience to others. The amount of swelling of the tumour and the doughy tension whereby you judge, are not easy to describe. The chief criterion, however, to my mind is the first delicate change of colour in the skin. In early operations it is well not to go so far as this. In any case it indicates that you have reached the utmost limit, and the needles must be at once withdrawn.

If the needles be very slowly withdrawn while the battery is still working, so as to cauterise slightly their track, not a single drop of blood will flow. If this be not done, it is necessary to apply gentle pressure for a few minutes.

The punctures should then be covered with a little antiseptic wool and flexile collodion, and if there be the least suspicion that the action has been carried too far, and the skin cauterised, this covering should be extended over all.

In large *nævi*, too much must not be done at one sitting or in one situation. The slough has to be absorbed, and it is better to establish several small subcutaneous sloughs at different parts than a great mass at one.

I find that, except in cases where I have been over-cautious and done too little, it is better not to repeat the operation under six weeks. This, however, is to be determined by the rapidity with which the induration may disappear, and by its effect on the growth as a whole. A very small slough may determine involution over a very large area, and so long as that is in progress it should be left alone. Every year I have been tending to lengthen the intervals. Of course one meets with difficulties in this respect. Patients from the country are anxious to get home again. But I am sure it is better to let them go and come back, even though the interval be thereby much lengthened, than to hurry on another operation before you can tell the effect of the first.

Excision.—When a nævus is purely subcutaneous, or when the cutaneous part bears a small proportion to the subcutaneous, provided always that it is situated on the covered portion of the body, it is best to excise it. The purely subcutaneous nævus is not common, but it is very frequent, especially in large growths, to find the skin vascular over perhaps only a third or less of the surface. If it be a large and prominent tumour, and the sides of the wound can be afterwards united by stitches, the diseased skin should be removed with the tumour. If a very large portion of such skin be allowed to remain, it is not always certain to be cured. I have even seen cutaneous extension of the nævus after the subcutaneous part had been removed. Still that is rare. After the nævus has been excised, the vascular skin usually cicatrises perfectly, and slowly assumes a normal hue, and this is always the case if only a small portion be left.

I prefer excision in these cases, because by it you have at least a great probability of healing by first intention. There is no slough to separate, as with ligature. There is no repetition of the operation, as with electrolysis. In these cases, moreover, you may rely with some certainty on the presence of a capsule, which greatly facilitates removal. But even when the capsule is adherent or absent, there is no risk of hæmorrhage if you can cut well beyond the margin of the growth, as in most instances it is easy to do.

But it is only in these cases that it should be used. It can

never in any case be justifiable to produce a scar on the face or neck, and perform formidable operations on account of a disease which in itself is practically without risk, which frequently disappears spontaneously, and which by other methods and with a little patience may be cured without a mark and with perfect safety. Practically electrolysis and excision occupy the whole ground in the treatment of *nævus*. On the exposed portions of the body, electrolysis is certain and safe, and, above all, leaves no scar. But it has the disadvantage of being slow and often requiring repetition. On the covered parts of the body, excision is rapid and effectual. Its disadvantage is that it leaves a scar. Moreover, even on the covered parts of the body, when the *nævus* is large or deep seated, the safety of electrolysis entirely outweighs the rapidity of excision.

It will be seen, therefore, that what was once a favourite plan of treating *nævus*, the ligature, has now lost its field of usefulness. There may be a few suitable cases of comparatively small *nævi* in which the skin is much affected, and the subcutaneous texture very scanty. Perhaps in some of them the resulting cicatrix may be smaller than that which follows excision, and they are difficult to cure by electrolysis without a scar. As a rule, however, even on the face, excision is better than ligature.

If ligature be used, the best plan is to place under the tumour two crossed needles, and to let out the blood by puncture while the silk is being tightened as strongly as possible. The strictest antiseptic precautions having been used, the whole is then covered by antiseptic wool and flexile collodion or celloidin, kept fixed down by daily touching at the margin with fresh material.

In one or two very large and prominent *nævi* I have tried subcutaneous ligature, both with silk and elastic thread. The cord is passed under the skin round the base of the *nævus*, and the object is to cut off the tumour from its deeper affluents and leave it to be nourished from the skin only. The method is, no doubt, capable of producing a cure, but it completely failed with me in a large *nævus* of the buttock. I do not think it is equal to excision and electrolysis in their respec-

tive spheres, and I have therefore abandoned it as well as the other methods of ligature.

Non-Congenital Capillary Angiomata may be divided, like the congenital, into the simple, the arterial, and the venous, but to these may be added the mixed, to which the name of aneurism by anastomosis may legitimately be given.

1. The first variety is the cavernous tumour proper, which consists solely of altered capillaries, in which there is no pulsation or bruit, nor marked venous dilatation. The tissue consists essentially of blood spaces, with free and large intercommunication, the trabeculæ composed of white and glistening connective tissue lined with an endothelium continuous with that of the blood-vessels. It is supplied by arteries which, if they be enlarged, are so only immediately before they enter the tumour, and the veins which issue, if dilated at all, soon assume their normal calibre. Perhaps this class of tumour is more frequent in connection with internal organs than externally. It has been described as occurring in the spleen, liver, and elsewhere; but the structure, markedly cavernous, does not appear to differ from the subcutaneous variety in any important respect. When deep seated, they are not amenable to any form of treatment, and are usually not to be diagnosed.

The external variety may begin at any age from early youth to middle life. It is of exceedingly slow growth, but with no tendency to retrogression. It has little or no proneness to produce ulceration. In some cases it may attain a very large size, but only after long duration. It is ill-defined, tending to the round or oval form, and with a consistence exceedingly like that of a fatty tumour. It is, of course, more or less compressible, but this distinction is not always very easy to make out, because its structure has a good deal of the fibrous in it. A good example of the innocuous character of these tumours I met with in the case of a gentleman from the Highlands, who consulted me with a view to electrolysis. He was 50 years of age, and the tumour had existed for thirty years. It occupied the submaxillary region, reaching from the lobe of the ear to the middle line, and its large size and unsightly appearance made him anxious for its removal. It was of soft consistence and ill-defined, in feeling very like

one of those continuous fatty tumours sometimes found on the nape of the neck. Its centre was crossed by a scar, to which it was adherent. He had seen many surgeons, and one of them had attempted its removal, but had been deterred from completing the operation by hæmorrhage and the absence of any definite capsule. He had then sewn it closely up, and union took place by first intention.

2. The next variety of cavernous tumour to which I would refer is one which may well be called the venous. In it, while undoubtedly telangiectasis or capillary metamorphosis exists, the chief feature of the disease is phlebectasis or dilatation of the veins. In its natural history it resembles the varieties I have already described, but with that greater tendency to inflammatory reaction, to ulceration, and to possible hæmorrhage which might be expected from its similarity to varix. It is subcutaneous, and the huge and tortuous veins, in which often phleboliths may be felt, give the leading feature to the disease. Pressure empties them at least partially, and, unlike the other forms of cavernous tumour which are but slightly reducible, it may thus be very largely diminished in bulk. In any form this variety is rare.

3. Another variety of cavernous tissue tumour is that in which the cavernous metamorphosis of the capillaries is associated with ectasy of the arteries and arterioles which supply it, but in which the veins are not notably involved. This growth begins in the young adult like the others, before middle life, but a form not distinguishable from it may owe its origin to a late and retrograde development of congenital nævus. That rare affection, which has been called osteo-aneurism, belongs to this variety. Although it is certain that what has been called osteo-aneurism is in most cases a sarcoma, in which enlarged and multiplied vessels form the meshwork for the cells, there can yet be no doubt that innocent pulsatile tumours of bone do exist. As a rule, the arterio-capillary cavernous tumour occupies the soft parts.

I have said that it sometimes originates in congenital nævus. We have seen that certain nævi pulsate from the beginning, but the tumour to which I refer now is one in which the active and pulsatile character is assumed later, and

which presents several other points of distinction. Two examples of this pulsatile angioma may be given: The first was a young man of 19, who presented himself at the Infirmary with the following history:—From his birth there had existed on his left cheek a small *nævus*, partly cutaneous, but chiefly subcutaneous, about the size of a walnut. Six months before his admission, as he thought because of a blow, it began for the first time to pulsate. Rapid enlargement followed, and at the period of admission a tumour, strongly pulsating, and three inches in diameter, occupied the site of the *nævus*. Its innocent character was sufficiently proved by the fact that three sittings sufficed to cure it by the application of electrolysis.

The second case was one which, although less clear, I am also inclined to regard as of this nature. It was that of a gentleman, *æt.* 47, whom I saw in consultation with several of my colleagues, and who had been examined by surgeons of eminence in other places, with somewhat uncertain diagnostic results. A pulsating tumour of large size, with distinct bruit, occupied the upper and posterior portion of the thigh. It reached as high as the ischial tuberosity, was deeply seated, and moderately well defined. From early life, but not from childhood, a tumour had existed, distinctly non-pulsatile, and of very slow growth, if, indeed, there had been any since it was first noticed. Since pulsation began about two years before we saw it, the increase had been decidedly more rapid. The point in doubt was hardly so much whether or not this was originally a cavernous *nævus*, as whether or not it continued innocent. Its long continuance, its similarity to the case I have already given, and to some others which have come under my notice, inclined me to class it as a pulsatile cavernous tumour, and to advise electrolysis. The patient, however, did not consent, and a year or two afterwards I heard that it remained much in the same state as when we saw it.

In these cases there is a rounded and fairly defined tumour with a systolic, not a to-and-fro, bruit. It is more frequently seated on the extremities than the other varieties of angioma. In other respects its clinical history does not greatly differ from them. If it originate in a *nævus*, however,

and if the pulsation be not very strong, there is a possibility of spontaneous cure by fibrous or inflammatory change. I witnessed the gradual obliteration of one of small size, and but slightly pulsatile, which was seated over the lower part of the trapezium. They are amenable to treatment.

4. Under the head of aneurism by anastomosis I describe a variety of tumour in which arteries, veins, and capillaries are all involved. It commences in early adult life. Once begun, it continues to grow slowly and irregularly, but surely; and often, after many years—now stationary, now increasing—it may attain truly gigantic proportions. It does not, in my experience, affect specially the scalp, but is generally seated on the upper part of the trunk, or on the neck. The chief risk is from injury. It has very little tendency to produce ulceration, although I have met with tumours in which the skin at parts appeared to be dangerously thin. These thin portions lie over the large venous connections, and I should regard aneurism by anastomosis as having risks in regard to rupture somewhat similar to, but not so great as those of varix: not so great, because there is little tendency to inflammatory action, as in varix.

Perhaps the best idea of such a tumour may be got from a description of the largest I have met with, in a patient of Dr G. W. Balfour, aged 42. Beginning at the centre of the right clavicle, it extended backwards over the shoulder and root of the neck to a considerable distance below the lower angle of the scapula. It entirely enveloped the lower half of that bone, but did not invade it or produce absorption. The tumour was nodulated, of varying consistence, and somewhat ill-defined. The pulsation was strong over the whole mass—varying, however, much in force; partly from varying nearness to the surface, partly from the arteries being more dilated in one part, the veins in another, the capillaries in a third. A loud and distinct systolic bruit could be heard throughout. In three separate areas, in the lower part of the posterior triangle of the neck, this bruit was replaced by the harsh, continuous, to-and-fro, intensely thrilling murmur, which has been well likened to the noise made by a blue-bottle fly in a brown paper bag, which is found in aneurismal

varix, and which always indicates close communication between an artery and a vein. In each area there was a spot where, by pressure with the finger tip, this bruit, and, to a considerable extent, the neighbouring pulsation, could be readily extinguished. Curiously enough, when the lowest of these spots was pressed upon, the bruit in another was also arrested. When I saw it, the tumour was of very old date, and when I heard of him ten years afterwards the man was still alive, with little alteration in the mass, except some increase in bulk.

Treatment.—After my early success with electrolysis in cirroid aneurism and nævus, I was at first inclined to believe that a like success would follow its application to all forms of angioma. I was supported in this belief by my first case, one of pulsatile erectile tumour of the cheek, which was thus easily and quickly cured.

Two cases, however, which were placed under my care by Dr George W. Balfour, rudely shook my faith. The first was that gigantic example of aneurism by anastomosis in the neck and shoulder, to which I have already referred. For many months I worked away with electrolysis, certainly without injury to the patient, but with as little injury to the tumour. I dotted all through it masses of induration, but as they were slowly absorbed no diminution could be observed in the bulk of the tumour. Ultimately I abandoned the attempt in despair.

The other was a remarkable case of pulsatile cavernous tumour, which occupied the submaxillary region, and projected both outwards and into the floor of the mouth. It had existed for twenty years when I saw it first. About twelve years before that time the lady had consulted Mr Syme as to the propriety of her marriage. He gave it as his opinion that her life was too precarious to justify the step. The arguments of the lover were, however, more powerful than those of the surgeon, and the lady not only married but buried her husband. This tumour also I did my best to cure. On one occasion I even went so far in the destruction of tissue that the absorbents were unequal to the task given them, and an abscess slowly formed in the tumour. Much alarmed by the pain and the serious prognostic of her surgeon in the

country, she came to town. I had great hope that the tumour might thus be cured, but the abscess opened and healed, and the pulsation remained as vigorous as before.

Influenced by these cases, I somewhat hastily came to the conclusion that in these varieties of cavernous tumour electrolysis was of little use, and for a time I discouraged patients from undergoing the operation. A longer experience, however, has taught me that this idea also was erroneous, although it is still difficult to define exactly in what forms of the disease electricity ought to be employed.

Empirically, I think I may safely say that electrolysis will succeed in tumours of small size—that is, in those which can be overtaken in one or two sittings, whatever be the precise variety. I have, moreover, succeeded in several examples of pulsatile tumours which have been developed in adult life out of pre-existent congenital nævus, and the complete success of electrolysis in the treatment of nævus makes it likely that this is not a mere coincidence.

A priori, if we look to the nature of the electric action, and to the structure of these tumours, we may, I think, with reasonable probability, at once explain the failures, and find indications of the class which is likely to be benefited. Electrolysis is simply a process of cauterisation, of destruction of texture—so much tissue is thereby subcutaneously destroyed. But if we wish to avoid the evils of inflammation, abscess, sloughing, and hæmorrhage, the amount destroyed must not be greater than the absorbents can deal with, and time must be given for their action. There is therefore a limit to the size of the part destroyed, and to the shortness of the interval between the operations. Now, if we consider the structure of erectile tumours, we see at once that absorption of destroyed portions may be accompanied not necessarily by diminution of bulk in the tumour as a whole, but possibly by enlargement of the blood spaces which remain. The large size of the lacunæ also renders it not unlikely that they often succeed in re-establishing their lumina, when the blood and tissues have been only partially affected by the operation. Again, the great whirl and turmoil of the blood in some tumours must make it difficult for the secondary deposition of lamin-

ated fibrin to form. In aneurism by anastomosis especially this must be the case. Probably also nœvous blood may not lend itself so readily to the formation of fibrin.

It is evident, then, that the conditions which favour the application of electrolysis are smallness of lumen in the blood spaces, a moderate blood current, a size of tumour such that we can destroy a large portion of it quickly without throwing too great a strain on the absorbents.

But it is equally evident that it is impossible to determine beforehand the existence or non-existence of some of these conditions. I have, therefore, gradually arrived at the opinion, that considering the absolute safety of the operation, we ought in doubtful cases to give electrolysis a fair trial before proceeding to other and more severe remedies. In operating upon these cases we may regard it as certain that the mass of the tumour must piecemeal be destroyed. Nevertheless, I do not doubt from what I have seen that the operation should be directed, in the first place, to the obliteration of the affluents, and the points of closest communication between arteries and veins. If obliteration of these points cannot be attained the treatment may probably be abandoned.

Should electrolysis fail, the point to be next decided is whether further measures should be undertaken. For a large number I should answer in the negative. I have already given many examples of the innocuous character of these growths, and no one who has read accounts of the operations that have been performed for their removal can fail to recognise the danger of such proceedings. Still, every case must be decided on its own merits, by balancing the risk of the proposed operation against the risk incurred from the tumour through hæmorrhage or pressure on vital organs, and the suffering it causes from its bulk, its situation, or its bruit.

If it be determined to interfere, ligature of the affluents and excision are the only methods worthy of consideration. When it is feasible and safe to inject coagulants or to ligature the tumour, electrolysis presents a safer and equally certain alternative.

It cannot be said that the ligature of the afferent artery

or arteries is a reliable operation. I have twice seen it performed, and on both occasions without benefit. Still it has, in some instances, brought about a cure, and it is easy to conceive that the tumour may be in such a situation that no other operation can be practised.

Excision, if it be possible to perform it in cases which have resisted electrolysis, is of course effective. Although the recurrence of vascular tumours after excision has been noted, it occurs only under two conditions: one, when the tumour has really been a pulsating sarcoma; the other, when a portion of the growth has been left behind. One would expect that any portion so left would be obliterated by the inflammatory action. But I saw on one occasion an erectile tumour of the neck ligatured in such a way that a portion about the size of a hazel-nut was left unstrangled. Now ligature, even more than the knife, is calculated to occlude, yet recurrence took place in this case, and a second operation was required.

It is essential, therefore, to follow John Bell's advice, and cut wide of the disease, not only to avoid recurrence, but also that the communicating vessels may be divided at a point where they are of normal calibre. It is comparatively easy to do so in the tumours which are distinctly encapsuled. In these the arteries and veins are almost normal immediately outside the capsule. But a certain number of these tumours, not perhaps so large as in the case of *nævus*, are not clearly encapsuled, but fade away into the surrounding textures, or are closely adherent to them. Both in reference to these growths and to *nævus* men have generalised erroneously from the some to the all. When the capsule is not distinct, the operation is certain to be difficult and bloody.

I do not think that under any circumstances, or in any form, the cautery is a suitable instrument with which to perform excision; but I should try in a suitable case the subcutaneous application of the galvanic cautery when electrolysis fails. I believe that we shall yet have a considerable extension of the subcutaneous method of applying heat.

Venous Angioma—Phlebectasis.

We have seen in connection with both congenital and non-congenital varieties of angioma that dilatation of the veins may form an important though secondary part of the tumour, but I do not know that there exists an angioma, composed of and developed from veins, in the sense that nævus or cirroid aneurism may be called angioma. Varix, of course, might by a straining of terms be here included. But it is so essentially different in character, etiology, and treatment from all these, that it seems inevitable to put it in a place apart. While, therefore, for the sake of symmetry I mention the venous angioma, I do so only to explain that these commonly used terms really apply to tumours in the structure of which venous metamorphosis plays a part, it may even be an important, but not an essential or original part.

Arterial Angioma—Cirroid Aneurism.

In cirroid aneurism we have a form of vascular tumour capable of being differentiated clinically and pathologically with great accuracy. It is composed of the enlarged branches of an arterial trunk. Usually the smaller branches only are affected, but if the tumour attain a large size, the main artery supplying the part also becomes dilated. Thus, in a large cirroid aneurism of the middle temporal, I have seen not only the temporal artery involved, but even the common carotid half as large again as the artery on the other side. Sometimes the tumour is fed by branches of other arteries with which its own may normally anastomose. In the case just mentioned, long and greatly enlarged branches of the occipital and of the temporal of the other side could be traced to the tumour from their point of origin. The term aneurism by anastomosis has been applied by some writers to this involvement of other arteries. But the variety presents no important pathological or clinical distinction, and requires no special terminology. I have preferred to use that term as it was used by Bell and others to designate a form of cavernous tumour.

The branches of which cirroid aneurism is composed enlarge in lumen, and attain a size about that of the radial, rarely larger. In one case, however, a patient of Dr Moir, the middle temporal was enlarged to the size of the little finger. It was the only branch involved, and extended in closely packed folds for four inches from its origin. The case was peculiar also as occurring in a patient beyond middle life. The tortuosity here described is very characteristic. The arteries not only dilate but also lengthen, and thus become tortuous. They so form a distinct tumour tolerably definite in outline, in which the separate arteries can be easily distinguished by touch, winding and twisting round one another like worms. Each artery appears of pretty uniform calibre throughout, but when the tumour is closely bound down, the pressure may hollow out the bone over which it lies, and there may then be observed a tendency to narrowings and dilations. There is here a distinct resemblance to varix, and still more closely in this, that the gutters, in which the diseased arteries run, no doubt are sometimes true grooves in bone, but sometimes, also, as I have found, are due to inflammatory exudation around them.

Cirroid aneurism occurs almost invariably on the head. The temporal artery is the one most commonly affected, but it may occur on the other branches of the external carotid. Mr Spence figures an example in which the arteries of the palm were the seat of the disease.

It is a disease of the adolescent or young adult. I have never seen it in the child, and it rarely commences beyond middle life.

Cirroid aneurism grows very slowly but somewhat fitfully. It does not tend in any way to spontaneous cure. On the other hand, there is no tendency to spontaneous rupture. This is due to its slow growth and the consequent absence, as a rule, of inflammatory action. In cases where, from its situation or otherwise, it is subjected to pressure, there may be a little thickening, but usually the swelling is supple, with an elasticity due entirely to the tension of the arteries.

The disease is not therefore dangerous. I have seen one which had existed for twenty years, and was not then larger

than a walnut. The risk is chiefly from injury. To wound a structure of this sort would doubtless be speedily fatal, and to arrest the hæmorrhage in time would tax the highest surgical skill. But although not dangerous, it is very troublesome to the patient. It is the seat of a loud bruit, which from its situation on the head is perpetually audible to the patient, in some cases even to the extent of interfering with sleep. The bruit varies in intensity in different examples, from a soft bellows murmur to loud purring with palpable thrill. It is continuous, with a maximum corresponding to the arterial systole.

The etiology of the disease is unknown. That it is not degenerative is indicated by the period of its occurrence and by its situation—that is, it arises under conditions which throw strain on a vascularity presumably free from the changes of old age. The extension backwards to the larger trunks is probably due to the demand it makes on the blood supply, for the enlarged carotid already mentioned was at once, in one day, reduced to its normal size by the cure of the tumour.

The annoyance to the patient is so great that he is usually anxious to hazard much for a cure. The surgeon, on the other hand, aware of the comparative freedom from danger, is unwilling to interfere, unless he can do so with little risk to life and a certainty of success.

Until I indicated the advantages of electrolysis in cirroid aneurism, the practicable means of treatment were twofold—excision and ligature. Although the cautery has been used, it presents many disadvantages, and is not safe, even in regard to hæmorrhage.

Excision is successful, but very dangerous. The descriptions given of such attempts show the formidable character of the operation, which, in some instances, had even to be abandoned in the execution. I have seen one successfully removed from the lower eyelid in a patient of my father's. The gentleman is now alive and well, but the bleeding was most formidable, and the patient for a time in a critical position. No doubt the safety of such an operation might in some cases, and in some only, be much increased by the use

of temporary ligatures and suitable forceps, but it could never be otherwise than dangerous, and often highly so.

Ligature may be applied to the chief affluents or to the main trunk. The former is not always possible, when possible is frequently unsuccessful, and is attended by considerable danger. The latter has been successful in pulsatile tumours of the orbit, which are occasionally angiomata, and may reasonably be practised in the case of growths which are otherwise inaccessible. But the main trunk which usually requires ligature is the common carotid, an operation with a high mortality, and curing only about 10 per cent.

Electrolysis, on the other hand, is both safe and successful. Its safety may be estimated by the fact, that, personally, I have performed the operation without the slightest accident in cases of pulsatile tumour sixty-seven times, and if we add to these aneurism and nævus, on several hundred occasions. I have treated by this means nine cases of cirroid aneurism. All were cured: two by a single operation, two after three applications of the current, three required four sittings each, one five, and one six. Most of these I have already recorded in the pages of the *Edinburgh Medical Journal*. The first, on which I operated in 1866, was large and important. It affected the temporal artery, and was upwards of three inches in diameter, being nearly circular, with a pear-shaped neck below. It was cured in four operations. Mr Syme had declined to interfere by operation, and when I, after the cure, sent the patient to be examined by him, I had the gratification to receive from him a note, in which he said the case marked a distinct advance in the treatment of these growths. Another patient on whom I have operated was a young man of 25, who came to the Infirmary in 1885 from Glasgow, where he is a clerk in a merchant's office. The tumour was one of the posterior auricular, which had existed for several years. It was upwards of two inches in diameter, and was unusual in that its limits were somewhat undefined, and that it had produced some absorption of the bone. The pulsation and bruit were very marked, and it communicated above the ear with a branch of the temporal. Three sittings, with an interval between each of about ten weeks, seemingly affected

a cure. The interval was longer than usual, because the patient had great difficulty in leaving his work, and for the same reason, and because I was confident of its safety, he was allowed to return home on the third day after the first operation, on the same day after the others had been performed. A year afterwards he returned, because he discovered a twisting artery lying horizontally above the auricle. It was about an inch long, and lay deeply imbedded in a groove in the bone. A short application of the needles quickly occluded it.

A very good example of success in one operation was the case of a young gentleman sent to me by Dr Beatson. He had come home from the East for the purpose of being treated, and various suggestions had been made as to ligature and excision, but curiously enough no one had thought of electrolysis until he saw Dr Beatson. It is astonishing that by far the safest and most certain method of treating cirroid aneurism should be so little appreciated. The only occasion on which I combined any other mode of treatment with electrolysis was in a girl of 18, sent to me from St Andrews by Dr Moir. This cirroid aneurism, which originally was very large, extending from ear to vertex, had become almost flat, and had lost its pulsation, as the result of electrolysis, but half-a-dozen affluents from the occipital, posterior auricular, opposite temporal, and anterior branch of the same temporal remained large and tortuous. Two of these curved over the forehead, and were rather unsightly. Moreover, although I thought it probable that they would slowly disappear, I was not assured of this. I determined, therefore, to attack them, and did so, by placing a needle under and a small piece of sheet-lead over these two at their origin, and then tying a silk thread under the projecting ends of the needle and over the lead. Thereafter I destroyed these and the other arteries by electrolysis at the points when they entered the tumour. The result was completely successful, and the marks left by the needles were very small.

The mode of operating by electrolysis is precisely as in other angiomas. The secret of rapid success, however, lies in the recognition that this is an arterial tumour. That is also the reason why the success is so invariable. The posi-

tive needle is made to occupy a position where it may destroy as much of the central portion of the tumour as possible, and which is consistent with sufficient nearness of the two poles. With the negative needle the surgeon then proceeds in search of the main affluents, and destroys them one after another. If there be only one or two affluents, a single operation will suffice. If there be many, or, as often happens, some are difficult to recognise, further sittings are required, I have no doubt that the best plan is to aim at the penetration of the vessel by the needle, and, if possible, through several coils; but I have sometimes found that a satisfactory way was to use the needle so that by its lateral pressure it obliterated the pulsation of the vessel, and to keep it in that position until the vessel was thoroughly electrolysed.

ELECTROLYSIS IN ANEURISM

ALTHOUGH I shall not discuss in detail the pathology or treatment of aneurism, and will attempt merely an appreciation of the value of electrolysis therein, it is necessary in doing so to determine more or less definitely the relative importance of other means of cure.

From the earliest introduction of electrolysis, the object aimed at was coagulation of the blood by electric decomposition. According to this view, there must be essential differences in the mode of action of electrolysis when applied to aneurism and other tumours. In the former, every endeavour is made to confine the action to the blood; in the latter, the intention is to destroy tissue. In the former, it is hoped that the clot may be permanent, and even add spontaneously to its bulk. In the latter, the speedy absorption of the electrolytic products is desired.

It is easy to see, therefore, that electrolysis may be effectual, useless, or dangerous in the one and not in the other, that we cannot argue from its success in erectile tumour to its success in aneurism.

Electrolysis of blood leads to coagulation: first, by primary decomposition; next, by the secondary effect of the nascent products of decomposition; and last, perhaps by oxidation of the positive electrode. The character of the clot, therefore, will differ at the two poles. It is found that there results at the positive pole a small, firm, black coagulum; at the negative, a large, loose, but rather sticky white or pinkish froth; and around these a quantity of thin tar-like fluid in which the

blood corpuscles are shrivelled, crenated, or altogether destroyed.

These conclusions were obtained from experiments made in 1867 by Professor Fraser and myself on various albuminous fluids, and on defibrinated blood, and by the passage of galvanic currents through the living blood in the arteries of dogs. It was then shown, in confirmation of the results obtained by Smee and others, that the products at the positive pole were chiefly due to oxygen and acids, at the negative, to hydrogen and bases. These observations have been strongly supported by what has been found after electrolytic operations on aneurism. Indeed, the thin tarry fluid which I mention above first forced itself on my attention during one of these operations. We had previously, although recognising it in our experiments, not appreciated its amount and clinical importance. We have found, also, that in a closed vessel the upper portion becomes occupied by oxygen and hydrogen. In aneurism, for example, a tympanitic area can be mapped out, increasing as the operation progresses.

It is plain that the fluid and gaseous portions of the electrolyte can be of no curative value, but rather detracts therefrom, and that a useful result can be obtained only by the coagulum at the positive and the froth at the negative pole. Of these, the former is durable but small; the latter is large, but immediately after its formation begins to contract by absorption of the bells of hydrogen entangled in it.

It is evident from these considerations that an aneurism may be cured through electrolysis, if there be complete and immediate occupation of the sac by the solid products, and if the aneurismal wall contract in proportion to the shrinkage of the clot. Or alternatively, it may be cured because the artificial production of solid material provokes to the subsequent deposition of laminated fibrin.

But it is also evident that the blood stream may re-enter the sac by mingling with the fluid product, or because the froth contracts more rapidly than the aneurismal wall; or again, that, if the blood be of non-coagulating quality, the solid matter introduced may fail to induce further deposition of clot or fibrin.

The results of my operations, now including a considerable number of aneurisms, illustrate and confirm these conclusions from experiment.

And, first, electrolysis has not in my hands given rise to any untoward effects, so far as the electrolytic process in blood is concerned. The evils that may be apprehended are the carriage into the general circulation of injurious electrolytic products, the production of hæmorrhage, and the excitement of inflammation of the sac.

So far as I can learn, the first of these possibilities has not happened in the hands of any surgeon. I suppose that the gases and liquids produced are not injurious, and that the solids are of such a character, and so adhere to the needles, and afterwards to the walls, that they are not easily carried away. It is indeed a comparatively rare event that portions of the normal clot become displaced, and perhaps the artificial products may be regarded as having similar probabilities. The chance is not to be left out of account, but it is very unlikely to happen.

But hæmorrhage and inflammation have occurred, and have been the chief causes of fatal results in the operations of other surgeons. They are produced by the destructive action of the needles upon the tissues through which they pass, and the complete immunity of my own cases is due to care in avoiding the electrolysis of solid textures, and in passing the needles through a part where the wall of the aneurism is of considerable thickness.

I have had slight accidents, but without serious consequences, and from entirely preventible causes.

In one of my early cases guttapercha was used as the insulating medium, and a portion was stripped off in withdrawing the needle. I afterwards picked it out of the subcutaneous texture without evil result. In another case the insulating material was glass, and we were much alarmed by its cracking. In short, until vulcanite was introduced, the coating of the needles gave trouble.

In an early case a small subcutaneous abscess formed. We were not then imbued with antiseptic principles, and I

attribute it to the fact that our needles and fingers were not rendered aseptic.

In yet another case, although there was no external hæmorrhage, considerable subcutaneous ecchymosis occurred. The pressure of the finger tips on the little punctures had not been sufficiently firm and prolonged to prevent the blood flowing from the orifices in the sac, or rather probably the pressure had been badly directed, because I endeavour to make the needle tracks somewhat valvular, and it is necessary to cover with the finger not only the external orifice but also the estimated situation of the internal.

But I have had neither external hæmorrhage nor inflammation of the sac, because I have insulated with vulcanite the needles where they lie in the textures, and have avoided touching anything but blood with the exposed points.

Only one risk ought with such precautions to attend these operations. In two of my cases of aortic aneurism death by internal hæmorrhage occurred within a week after the operation. In one the opening took place into the pleura, in the other into the trachea. I do not know that the treatment and death are to be regarded as cause and effect, but it seems not improbable that the strengthening of the wall in one direction may have thrown greater pressure upon other points already little able to resist it.

But the great safety of the operation would be of comparatively little importance if the character of the blood products, as I have described them, should prevent electrolysis from having a permanent or satisfactory result, and it must be confessed that there is a tendency for its benefits to be merely temporary. In short, in rendering the operation absolutely safe, is it possible that I have in large measure taken away its curative power? Plainly, if its value depends on the power of galvanism to coagulate the blood, it cannot be so, because by taking away the pain of the operation I have been able to apply stronger currents over a much longer time than former operations. Moreover, from some of the cases to which I shall presently refer, it is evident that much good may be done by this coagulum pure and simple. But, on the other hand, it must be confessed that some of the

most striking curative results have been obtained by Petrequin and Abeille in external, by Ciniselli and others in internal aneurism, with uninsulated needles, and with comparatively short, though painful, operations. Abeille describes his patient as uttering loud shrieks and pouring with sweat. The mere fact that there was pain is proof that the needles were cauterising solid tissue. The mere fact that the needle was made of uncovered steel proves not only that, but that the very sac itself was cauterised. In my operations on aneurism the patients lay perfectly undisturbed, and on one occasion till the whole of the exposed point of the needle had been made to disappear by electric erosion. It is plain, therefore, that my operations consisted purely in coagulation of blood; theirs in coagulation of blood, which alone they aimed at, plus cauterisation of the sac, which they endeavoured to avoid. In my operations there has been no mortality; in theirs the mortality was high. In my operations, on the other hand, there were practically no cures, only amelioration; in theirs a certain percentage of cures were produced. I believe, therefore, that to some extent I have lost power in attaining safety. An important question is, How may the former be regained without losing the safety?

Now, to obtain a fair idea of the value of electrolysis in aneurism, it is necessary to have regard to the two varieties—internal and external. The operation has been performed in thoracic aneurism oftener than in all others put together, partly, no doubt, from the greater frequency of internal aneurism, but chiefly because the relation of electrolysis to other forms of treatment is very different in the two varieties.

Let us take first, then, *thoracic aneurism*.

I do not know that I can do better than give some description of one or two cases in which the phenomena may be regarded as illustrative of the results of operation.

CASE I.—The first case in which I used electrolysis in aortic aneurism occurred in 1866. The patient had already bled profusely. There was no return of bleeding after the operation, but the patient died exhausted after a week.

CASE II.—J. L., æt. 52, patient of Dr Macgregor. When I saw him (June 1867), had an aneurism projecting to the right of the sternum on the level of the third rib. It was about the size of an orange, with strong fluid pulsation, and evidently the tendency to death was by external hæmorrhage. The operation was performed twice, with an interval of a month. The sequence of events was similar on the two occasions, viz., tension of tumour, some escape of gas into the cellular tissue, after a few days, relief to the patient and diminution of the tumour, and lastly, after a fortnight or so, slow increase again. Dr Macgregor notes after the second operation, "Pulsation very slight, and more like the general lifting of the whole bulk of a solid body than the expansive feel of an aneurism." But the growth increased again, the operation was not repeated, and the patient died. Of the post-mortem examination Dr Macgregor observes, in sending me the notes, that "it clearly proved that a considerable quantity of blood had undergone coagulation by the galvanism," and, he adds, "had the orifice in the artery been not quite so large, and the galvanism more frequently or longer applied, success might have been the result."

The appearances he records are most interesting. "The aorta was largely dilated and lined by a perfect sheet of calcareous matter, which shone like porcelain and rang to the stroke of the knife." This is, of course, a condition of affairs which renders any thought of permanent cure out of the question. But, on the other hand, it is also recorded that "it communicated with the external sac by a smooth opening the size of a two-shilling piece," and again, "the sac could contain a closed fist, and the central mass (coagulum) was about the size of a hen's egg. This was surrounded by liquid blood, which filled the space between it and the wall of the tumour." The orifice, it seems to me, was not too large, and looking to the amount that had been accomplished, I have always regretted that illness prevented me from leaving town to repeat the operation. The fact that the liquid blood surrounded the coagulum is a striking commentary on the facts I have already detailed as the result of experiment.

CASE III.—The next patient is probably as good an example as one can have of the effects of the method of performing electrolysis in aortic aneurism.

J. S., ironmonger, æt. 44, a patient of Dr Burns, consulted me on 31st December 1871, for a thoracic aneurism, of which the external salience was six inches in diameter. The most prominent part was to the left, where a nodule the size and shape of half an orange at once attracted attention. This had grown very rapidly, was red and angry looking, and on its summit bore a dark blue circle as large as a crown piece, with an inner patch like a threepenny piece denuded of epithelium and apparently gangrenous.

Next day, 1st January 1872, the needles were inserted at 2.14 P.M. They were placed parallel to each other and to the surface of the thorax, one inch and a half apart. One inch of the vulcanised portion of each was embedded, and I was careful that the exposed points moved freely in fluid blood and did not at any time rest against the aneurismal wall.

At 2.30 it was observed that the dark blue portion of the tumour was assuming a brown tint, that the size and tension of the part in which the needles lay had increased, and that its nodulated character was diminishing.

At 3.5 the blueness had given place to a yellowish brown, the expansile pulsation was much diminished, and the tension had so increased that a little bloody serum began to exude from the patch which had seemed to be gangrenous.

From the first I knew that in using galvano-puncture I ran the risk of hastening ulceration or even of causing immediate rupture of the skin, already to all appearance little thicker than a sheet of paper. I had witnessed already the tension produced by the gaseous and other products which may be so abundant in electrolysis of an aneurism that you can tap out a tympanitic area in the highest portion of the sac. I had determined, therefore, to persevere till I had some assurance that a clot of sufficient thickness to prevent bleeding had shut off the portion upon which I was operating from the rest of the tumour. Now, as such a clot would require to be of

considerable size, reaching in its circumference to the aneurismal wall, I felt that as yet it could not have done so. But, on the other hand, exudation had begun, and it seemed doubtful whether the electrolysis would first produce rupture or coagulation. With trepidation I continued the operation. The exudation did not increase, and when at 3.30 I removed the needles, pulsation was reduced to a gentle heaving motion.

During the night he was restless, and a considerable quantity of fluid, closely resembling thin tar in colour and consistence, trickled over his left shoulder into the bed. The flow of altered blood continued off and on during the next two days.

On 3rd January, Mr Chiene and I examined the tumour. It had been partially covered with cotton wool and collodion and a bandage, but without removing these we could ascertain that the central part of the aneurism felt like a bladder partially filled with fluid. To the right was a semi-circle, an inch broad, which gave the sensation of perfect hardness and solidity. There was no pulsation either in the fluid or the solid ; while, still further to the right, the aneurism pulsated as strongly as before the operation.

On 1st February, finding it loose, I removed the case of cotton wool and collodion which had hitherto covered the blue part of the tumour and the needle punctures. One half of the tumour was flat with the chest, and the remainder was greatly diminished in size. An ulcer in process of cicatrization, and now not larger than a shilling, was situated to the left of the sternum, its edge fully half-an-inch distant from the nearest part of the swelling, and more from any point where there was the least indication of pulsation. It represented the dark circular spot formerly described as occupying the most projecting portion of the aneurism, which was now quite flat.

23rd February.—As the tumour was again increasing I operated again. The electric action was continued for more than an hour. Towards the end of the operation gas escaped from the ulcer with a loud hissing noise.

The phenomena were the same as before, and we were

delighted to see the tumour slowly diminish, and the ulcer begin again to cicatrise very rapidly.

Early in March, however, it became evident that the aneurism was enlarging internally. Blueness of the lips and cheeks, œdema of the neck, shoulders, and right arm, dyspnœa and alteration in the quality of the voice, were the gradually increasing signs of its growth. The case seemed hopeless; but I was about again to introduce the needles, when a fresh misfortune occurred. The little sore over the sternum was now (23rd March) about the size of a split-pea. It was attacked on that day by erysipelas, resulting in septic cellulitis; thereafter he gradually sank, and died on 12th April.

I shall not enter into a further description of the cases of aortic aneurism in which I have used electrolysis. None of them have been cured; but I think, in every one, there has followed the same diminished pulsation, and in most, diminution of the tumour. The duration of the diminished pulsation has been curiously variable. In some it returns partially, in others in full force, within twenty-four hours. But with the majority, the history is, as in L.'s case, of a slow increase of pulsation after a varying number of weeks, accompanied by an increase in size, which indicates that, after a period of improvement, the aneurism is again progressing.

I think it is not of much use to discuss statistics.* Everything of value is said in that respect when it is stated that the greater number of cases are in their results similar to my own; that the deaths directly due to the operation, or even hastened in any degree by it, were the effect of the plan of manipulation adopted by the operators; but that, on the other hand, complete success is claimed in five or six cases out of a total of perhaps a hundred aortic aneurisms which have been thus treated.

We are now, it seems to me, in a position to form some estimate of the value of electrolysis as it has been hitherto performed in an aortic aneurism.

We are dealing, however, with a condition the natural history of which must be carefully studied before we can definitely determine the rôle of electrolysis. The prognosis is,

of course, so grave that it may almost be said that an aortic aneurism produces death unless some other lethal disease intervene. But, on the other hand, it is subject to strange vicissitudes, to rapid advance and remarkable improvement, and this applies alike to the most advanced and the earliest stages. Rupture into the cellular tissue, as I saw in a patient of Dr Balfour's, nay, even external rupture may occur, and yet the patient may live for years. It is the most common experience to see a red nodule form on the surface of an aortic aneurism and even begin to get blue and to desquamate, while yet, presently, when it seems on the very point of bursting, the pulsation slowly diminishes, the nodule flattens down and hardens, and the patient improves for the time. An inflamed condition of the sac undoubtedly tends to the formation of "laminated fibrin."

Now, I do not think the last word has been said about this laminated fibrin. How much of the stratified mass which occupies and occludes an aneurism is lymph poured out by the wall of the sac, how much is deposit from the blood. Thinking over the experiments that have been made on the coagulation of blood, and observing the character and history of the internal change in aneurisms and in blood cysts in general, I cannot avoid coming to the conclusion that stratified fibrin is mainly clotted blood. I picture to myself a deposition in layers from the very first, in consequence of the inducement to coagulation being most intense at the periphery, and the stratification gradually becoming more marked in the process of organisation upon the clot. Like blood-clot, everywhere it becomes more or less invaded by leucocytes, acts as a scaffold for the growth of granulation tissue, and in this process gets decolorised and organised.

I do not know, however, that for our present purpose the precise pathology of stratified fibrin requires to be determined. It is sufficient to understand that an inflammatory state of the wall tends to its formation, and that certain conditions of the wall of the orifice and of the blood are also necessary to a cure. If, for example, the wall of an aneurism be loaded with calcareous plates, it is plain that the inflammatory influence upon the blood must be much less than when the

tissues are capable of producing inflammatory products on the surface, that from such calcareous matter no formative material can enter the clot, and that as the clot contracts it is impossible that the sac can follow its contraction or promote this by pressure.

It is manifest, therefore, in relation to electrolysis, that the cure of an aortic aneurism by blood-clot produced in the centre of the tumour and adherent only at points, is and must be doubtful, because it is not aided by inflammation, by resiliency, or by the conditions which make for organisation. Its chance lies, therefore, in the fact that a coagulum once formed tends to increase, and, unfortunately, the coagulum produced by electrolysis is of such a nature, that it is exceedingly apt in yielding up its gas to diminish with rapidity, and so allow the re-occupation of its site by fluid blood.

In aortic aneurism, therefore, it seems to me that if one aims at treating by the electrolytic production of coagulum only, it is useless to try it unless you have some legitimate hope that you can occupy by one or by quickly repeated operations practically the whole sac, so as to exclude the fluid and circulating blood. In as much as, if the aneurism be intrathoracic, it is impossible to determine, with any approach to accuracy, whether it be sufficiently sacculated, its orifice sufficiently small, and its wall sufficiently vitalised, I have always held that the rôle of electrolysis in thoracic aneurism was to avert or delay the tendency to death by external hæmorrhage, and that the effort of the surgeon should be by long and strong electrolysis directed, as far as may be, to dam off the outer sac from the inner. I believe still that considering that the method is quite innocuous, it may advantageously be thus employed.

But I have for some time thought that the cases of reported cure of thoracic aneurism by Ciniselli, De Cristoforis, and others required further consideration. In view of the numerous failures to cure by myself and others who have followed my methods, failures which we anticipated, because we thought it unlikely that the occlusion of the extrathoracic sac could, otherwise than in rare and exceptional instances, induce a sufficient deposit secondarily in the intrathoracic

portion. In view of this position it was necessary to study carefully the reported cases of cure. Two points of difference emerge. The first is, that the operations were performed before the aneurism had fully penetrated the thoracic wall; and the second, that they were performed with uninsulated needles. The effect of the first is, of course, that the sac is probably in a much better condition for that contraction which we have seen to be so important; and of the second, that cauterisation of the sac was necessarily a part of the operation. It would seem that in these operations, as in my own, the idea that the good result depended on the amount of blood-clot immediately produced was quite erroneous. It was impossible that by the methods adopted by alternation of current and otherwise, the amount of blood-clot could have been at all considerable; had it been so, the slough round the needles where they passed through solid texture must also have been large, and therefore highly dangerous. The natural conclusion, therefore, is that the improvement in these cases has been chiefly due to the subsequent deposition of stratified fibrin in consequence of inflammation of the sac wall at and around the small sloughs produced by the needles. If this be so, and it is difficult to escape this conclusion, the problem is entirely changed. It is no longer a question of how to produce the largest amount of coagulum with the least possible injury to the wall—a problem which was perfectly solved by the introduction of vulcanised needles—but the objective now is, to produce precisely that degree of inflammation which will lead to the deposition of fibrin, and yet avoid that inflammation of the sac which both spontaneously, and after every form of operation for aneurism, has often led to disastrous results. It is plain that here we must aim at something which can be applied with extreme precision, and that the method by electrolysis is at once brought into contrast with many other operations which are calculated to produce irritation of the sac even although some may have originally been introduced with the view simply of producing a coagulum.

There are three methods by which electrolysis may be used with this object in view:—

1. Insulated needles may be introduced into the sac and an attempt made to touch the aneurismal wall at various parts with the exposed points of the needles.

2. Fine uninsulated needles may be used and the sac cauterised only at their point of entry.

3. Fine wire may be introduced through a canula, and a current of electricity then passed through it.

It is plain that these methods are thus brought into contrast with the plan of introducing wire or other material and leaving it permanently inside, and with that of simply introducing needles in a temporary way and for the purpose of causing irritation of the sac.

It is probable that only experience can ultimately decide between these various plans, but on *a priori* grounds it seems unlikely that the permanent retention of foreign material inside an aneurismal sac will turn out to be the best method, whether accompanied or not by the passage of a current of electricity. Its continuous presence would be extremely apt to render the inflammation excessive, and it is certain that it must tend to interfere with that contraction of the walls which is so important to permanent cure.

It would seem, therefore, that so far as operative measures are concerned, we shall ultimately have to decide between the methods of irritating the interior of the sac by electrolysis and by scratching with a needle.

If electrolysis be used (in view of the five cases of recorded cure), I should think that in aortic aneurism, before it has penetrated the thoracic wall, the best plan would be to pass exceedingly fine needles and transmit a moderate current through them for a short time. I do not see any advantage in Ciniselli's plan of alternating the current. Its sole result is to diminish electrolysis—that can be attained by shortening the time. On the other hand, where there is a sac outside the thorax, the larger insulated needles should be used, so as to avoid cauterising the needle track, and to enable the sac to be touched in various places by their exposed points. From their greater rigidity they are more easily manipulated.

In comparing electrolysis with the mode of using needles suggested by Mr Macewen, we have no sufficient data to go

upon. Electrolysis is shorter, but more painful than the needles. Neither can be expected to be free from risk, or to present a high percentage of cure in aortic aneurism. The same questions, however, must be considered in connection with external aneurism, in which the results obtained must to a large extent determine the value of the methods in thoracic cases.

External Aneurism.—We have here at our disposal a great variety of operative procedures, most of which undoubtedly have their use in suitable cases.

Modern surgery has diminished, it has not of course eliminated, the difference in point of risk between operations which involve a breach of surface and those which do not. But this diminution in risk has at least the effect of causing us to reconsider our attitude to them in connection with each variety of aneurism.

I am not sure that we do not underestimate the value of medical treatment. I had one interesting case of aneurism in Hunter's canal in my wards, which had been treated by Mr Spence by ligature of the superficial femoral. It had, when the patient left the Infirmary, almost but not quite disappeared. A year afterwards, however, it began slowly to enlarge, and he came again to the Infirmary. I put him upon potassium iodide, and elevated the limb, with the effect that in ten weeks it was absolutely cured. But putting aside the medical treatment, we have at our disposal for external aneurism the various methods of obliterating the cavity by the introduction of coagulants, the different modes of arresting or diminishing the force of the circulation, and the old operation of evacuation.

It is, of course, perfectly evident that these operations must have very different values in different aneurisms; that evacuation, or ligature, or electrolysis will have very different relations in respect of feasibility, risk, effect, according as they are applied to a gluteal or iliac, an idiopathic or traumatic, a sacculated or fusiform aneurism.

It is essential, therefore, after a preliminary glance at each *modus operandi*, to consider it in reference to the varieties of aneurism. I have already in connection with aortic

aneurism, both on *a priori* grounds and as the result of experience, expressed the opinion that the introduction of wire, or such like solid material, with or without the passage of an electric current, has little likelihood of being adopted. In external aneurism, the introduction of chemical coagulants must, I think, be regarded in the same light. They cannot, of course, be used except in cases in which the circulation can be arrested for a sufficient time. My experience is confined to seeing the introduction of perchloride of iron into a gluteal aneurism performed successfully by Nélaton upwards of thirty years ago. But I have seen a good deal of the use of both iron and carbolic acid in *nævus varix*, and other blood tumours, and my impression is that they are difficult to manage, that they are apt to do too much or too little. If that be the case in angioma, it is certain that in aneurism they would prove even more unruly.

There remain, therefore, the two subcutaneous methods to which I have already referred—electrolysis and Mr Macewen's operation of needling. On the respective merits of the two methods it is yet too soon to form an opinion. As a contribution thereto, I offer three cases, which I have already published elsewhere, but which will bear repetition in abstract. One was a traumatic aneurism of the radial, which was effectively cured by electrolysis. I do not mention it because I consider this the best method of treating such a condition, but because it was the first case in which the modification was made of using the needles not merely for the purpose of coagulating the blood, but also of cauterising the sac.

CASE IV.—In 1888 I received into the Infirmary Mrs Y., *æt.* 47, suffering from aneurism of the innominate and first part of the subclavian. Various medical plans of treatment had been tried without success.

On 26th June 1888 I operated according to the plan which I had so often previously employed. A current from six large Bunsen cells was passed for thirty-five minutes through insulated needles. Chloroform was not administered, and except from the introduction of the needles, the patient experienced no pain. The effect of the operation was an im-

mediate reduction of the violence of the pulsation to a very great extent. After a few days, however, a slow but steady increase of pulsatile force was observed, so that in the course of three or four weeks the condition was much as it had been before the operation. I then determined to put in practice the ideas to which I have above referred.

The ordinary needles coated with vulcanite were used, but a fluid rheostat and a Gaiffe's galvanometer were interposed in the circuit. In our everyday operations upon *nævi* these are unnecessary, because the effect produced is determined by sight and touch; while in former aneurismal operations they were unnecessary, because electrolysis of the blood is of course not painful, and the duration of the operation is determined by the pulsation.

Chloroform was not given, because, although considerable pain was anticipated and actually caused, the patient had much endurance, and it was thought that the pain might aid us in determining the extent to which electrolysis should be pushed.

By means of the rheostat the current was gradually increased to 20 milliampères, as represented on the galvanometer. Unaided by sight or touch we have to determine the amount of cauterisation by the strength of the current. The exposed points of the needles were brought into contact with many portions of the wall. They were not allowed to remain in contact for more than a few seconds with any one portion, and the whole operation did not last longer than fifteen minutes. The current was allowed to continue while the needles were slowly withdrawn. The same precautions as I have always used in introducing the needles by a valvular opening, and in applying pressure on withdrawal, were observed. A slight amount of swelling and tenderness lasted for a few days. A steadily increasing thickness of wall could be noticed for two or three months, when the improvement ceased; and by the end of five months and a half, it seemed desirable to repeat the operation because deterioration had again begun.

The operation was repeated four times in essentially the same way. The opportunity was, of course, taken to vary the

strength of the current as well as the period of contact and of operation within moderate limits.

The last operation of this kind was performed on 20th June 1890. From that time for two years there was steady improvement. The pulsation was distant and heaving, not expansile, and the tumour much smaller. From 1892 till the end of 1893 it remained stationary. For three years, in short, she had gone about her ordinary duties and work at her home in Berwickshire, and came in twice a year to let us register progress. In June 1893 I noted, "My impression is that there has been little change either way for about a year, but she is so well that it seems unnecessary to interfere."

Early in 1894, however, it became evident that the tumour was enlarging and the pulsation stronger, and after a time, when the deterioration became more rapid, she returned to the Infirmary. On three occasions, with intervals of a fortnight, I operated according to Mr Macewen's directions, using on the first occasion four, on the second six, and on the third eight needles. I think there can be no doubt that some thickening of the wall took place. It did not last, however, and I had determined to introduce the needles again, when the patient was urgently summoned home on account of illness there.

In June she returned, and the needles were again introduced on four occasions. This time there was little effect, and she was again obliged to go home. She did not return, and died from the effects of the internal pressure of the aneurism.*

We tried at different operations the effect of lengthening the time and varying the number of the needles. We were disappointed to find that when the needles were kept in longer than five or six hours there was very considerable redness and swelling of the skin round some of them, and that in the latter series of operations this was on more than one occasion positively alarming. It must, of course, be noted that by that time the disease was getting far advanced, and

* The author evidently forgot that Mrs Y. returned to the Infirmary and died there. The specimen, which is in my museum, shows the aneurismal sac to be almost entirely filled with laminated clot.—ED.

was rapidly advancing. The state of parts was much worse than when the previous series was performed, but even in that series we had considerable irritation. The skin, needles, and hands were all prepared as for an antiseptic operation, and the needles, while in, were carefully packed round with gauze. On no occasion had we any such trouble when the needles were kept in for a short time, or with the electrolytic operations. It is difficult to say whether this was due to the prolonged mechanical irritation, or to the deeper follicles of the skin containing undestroyed organisms. It is well known that an absolutely perfect aseptic condition of skin is difficult to secure. The means taken were thorough scrubbing with soap and water and ether, followed by a 1 to 40 carbolic compress for twelve hours.

I have no doubt that both with electrolysis and with needling there was very considerable improvement, and that life was prolonged thereby, at all events in the earlier part of the case. I am inclined to think that the progress of the aneurism was hastened by the inflammatory reaction in two of the later operations. But, as may be expected to occur in a certain proportion of cases, the conditions, whether of blood or aneurismal sac, were here evidently unfavourable for the production of laminated fibrin. It is evident that whether one or the other method be used, it is difficult to anticipate the amount of irritation necessary, yet not injurious.

CASE V. presents various points of interest. J. A., æt. 36, admitted June 1891, for popliteal aneurism. Treatment by rest and flexion had been already tried before admission. My experience of the flexion treatment in other cases had proved to me that the criterion of its effectiveness lay simply in its power to obliterate pulsation in the aneurism. If by flexion an aneurism could be made so to press upon its artery as to obliterate it, success was probable, not otherwise. In this case flexion had no distinct effect. I did not therefore repeat the flexion, especially as it had led to paresis of the external popliteal nerve and considerable œdema of the leg, with petechial spots.

After allowing a few days for recovery from this condition

I applied an elastic bandage according to Dr Reid's plan. My only previous experience of this method had been in the case of a patient aged 61, who suddenly, while walking down Leith Street, had experienced violent pain in the popliteal space, which was followed in the course of a few weeks by the formation of an aneurism. The application of the bandage on these occasions for lengthening periods (my colleague, Mr Miller, in charge during my absence, maintained it on the last occasion for six hours) had ended in perfect cure. In our present case, an application of four hours produced absolutely no effect on the aneurism, and very seriously increased the popliteal paralysis. I then electrolysed the aneurism on 8th July. The needles seemed to move very freely in fluid blood, and were used gently to cauterise the wall of the sac in many places. This caused considerable pain, and after five minutes was discontinued, but the needles were maintained for fifteen minutes longer in the centre of the fluid blood, while the current was continued with the view of producing a certain amount of clot. In the early part of the operation a current of 30 milliampères was used, but during the latter fifteen minutes it was increased to 90, because then, of course, it caused no pain. A very distinct improvement at once ensued, and steadily advanced for some weeks.

On 11th September Mr Hodsdon, who was in charge during my absence, repeated the operation, being satisfied that improvement had ceased. For three weeks, again a steady advance was made, but on the 29th a singular chain of symptoms set in. On that day the patient complained of pain in the tumour, and his temperature rose at night to 100° . Daily the pain increased, and nightly the temperature rose higher, while the pulsation steadily diminished and the tumour grew firmer. On the 10th October it is recorded that the pain was still increasing, that the temperature was 103° , that the tumour had assumed a dusky red hue, was swollen and somewhat doughy, but that pulsation had almost disappeared.

It was evident that there was inflammation of the sac, and I hoped for a cure, but the patient was clamorous for relief to his suffering, and after resisting for two days longer, I weakly yielded to his urgency.

On the 12th October I applied a tourniquet and incised the aneurism. I found the cavity, which was as large as the fist, occupied by black clot, with the exception of a space the size of a hazel nut, in which the blood was red and fluid, and which communicated with the vessel. There was a high bifurcation, and a slit-like opening in the popliteal artery lay just above it, so that I was obliged to ligature all these vessels. The wound healed without one drop of pus, and the patient was discharged cured.

In reference to electrolysis, the point of interest lies in the fact that on the eighteenth day after the second operation inflammation of the sac set in. We need not consider the strange delay, because, although I do not doubt that this inflammation was in some way connected with the operation, I have no hypothesis to offer to account for it. When operations with uninsulated needles have in other hands led to inflammation of the aneurismal sac, death or recovery has usually resulted in less time than marked its beginning in this case. We undoubtedly aimed at producing inflammatory action of a mild character, and obtained it in such a way that, after each operation, satisfactory progress was made without pain. As undoubtedly, had I had the courage of my opinions, the aneurism would have been cured had we left it alone after the second operation, because there was no sloughing and no pus on the one hand, and on the other abundance of clot. But it is, I confess, disconcerting to find that we have to lay our account with a possibility of so acute an inflammatory attack coming on so late in the treatment, even although it must of course be exceptional.

If we view in the light of these cases the comparative merits of electrolysis by this method and the process of needling, we may perhaps gain some idea of their respective risks and advantages. It is plain that both aim at bringing about a cure by producing inflammation of the sac. The question to be answered is, Which can do so with least risk and greatest certainty? I take it that with both there must be a considerable percentage of failure. If the sac be a mass of calcareous plates, or so lined by blood-clot or unorganised material as to prevent the needles being brought into con-

tact with the wall, no result will ensue, or only that which may be brought about at the point of entry of the needle.

That both are capable of producing the inflammation which leads to cure is proved by results. We have no data which would justify the ascription of greater certainty to one or other. Both are obnoxious to the charge in that they may produce an excessive and dangerous inflammation, as these cases show, and there is no reason to believe that one would be more apt to do so than the other. It is evident that there are individual idiosyncrasies, and that it would be well in every case to make the first operation a mild one. For example, with electrolysis I should be content at first with 5 or 6 milliampères, and contact of 8 or 10 seconds at each spot.

In these essential points there seems little to choose. In the matter of simplicity the needling has no doubt the advantage, but the use of electricity is so greatly extending that that probably is not of importance comparatively. The pain experienced is considerable with anything over 15 milliampères, but a much milder current is capable of effecting our purpose, and can easily be borne. With needling, the pain is not great. With electrolysis, the operation is over in a few minutes; with needling, its duration is very irksome.

I confess, therefore, after as impartial a consideration as I can give to the two methods, that I do not know which in its essential character is to be preferred. Possibly we may find that after comparing them with other modes of treatment, we may be able to assign them special rôles in special cases.

Aneurisms may be divided, so far as treatment is concerned, into those which are situated on the main artery of a limb and those which are not, and the former may again be subdivided into those in which the circulation can be commanded on the proximal side and those in which it cannot.

The most typical examples of aneurism not on the main artery, are the gluteal and sciatic. In these, the only cutting operations are, the laying open of the aneurism, and the ligature of the internal iliac artery, which must necessarily be attended by very considerable risk. On the other hand,

the introduction of needles, with or without electricity, may be done with practically no danger, and the chances of success are great, because it may safely be pushed to the production of the necessary inflammatory reaction. No introduction of coagulants, whether mechanical or chemical, can for a moment be compared with them, either in safety or efficiency.

In the case of the main artery of a limb, when the circulation can be commanded on the proximal side of the aneurism, of which the typical variety is the popliteal, we have at our disposal various plans of treatment. I should myself be inclined, in the first place, to resort to compression; and of the various forms of compression I should choose that in which the arteries are emptied of blood by the elastic bandage, while the aneurism is kept full. It is advisable in carrying out such treatment that a certain measure of support be afforded to the aneurism, as I have found that from neglect of this precaution great congestion, and even small extravasations are apt to occur.

If, as may happen, compression fail, I should place next in order of merit the ligature of the main artery. Antiseptic surgery has rendered such an operation so safe, and its efficiency is so great, that I should place it before the means of treatment we are considering. Mr Syme maintained that this ought to be preferred to compression, because if compression had been tried the ligature was likely to prove less effective. Of this we have no practical proof, and theoretically a precedent compression ought to add to the safety of ligature without impairing its efficiency, even if Dr Reid's method be adopted.

Lastly, in such cases I should adopt one of the methods we are discussing, and, as the case of J. A. shows, the resources of civilisation are not even then exhausted—we can still lay open the sac.

But aneurism may occur on the main artery of a limb where arrestment of circulation on the proximal side is still possible, but more difficult and dangerous. There is, for example, the aneurism of the axillary or common femoral. Compression here loses its value, because either of its difficulty or danger. Shall we then open the sac, or ligature on

the proximal side, or endeavour to excite internal inflammation. I do not think that Mr Syme's endeavour to revive the old operation in such cases was altogether successful. He was quite accurate in saying that in a certain proportion the orifice in the artery is small, and I think it probable that, if this be ascertained in any particular case, the most effective plan, as being absolutely certain and not much more risky than ligature, is to open the sac. As between these operations, then, each case must be judged on its own merits. With a history of sudden pain in the part and formation of a swelling thereafter, one might fairly predicate a small orifice. On the other hand, I assisted at an operation of Mr Syme's on an iliac aneurism, which turned out to be fusiform and required ligature—above, near the iliac bifurcation; and below, of the superficial and deep femorals. In that patient, if any of us who carefully studied the tumour had reasoned rightly, we should have known that it was fusiform, because it extended for many inches, both above and below Poupart's ligament.

But putting aside this question of the domain to be assigned to one or other of these operations, it appears to me that both should be postponed to acupuncture or electrolysis. Although less certain, these undoubtedly are very greatly safer.

As the situation of the aneurism gets nearer to the heart, the condition approaches more and more nearly to that of aortic aneurism. The possibilities of surgical interference become more and more limited, and we are restricted more and more completely to the introduction of needles as generally the best, and often the only method of local treatment.

The aneurism of the carotid presents so great a similarity to that on the main artery of a limb, that it scarcely requires separate consideration. One word of caution I should like to give. I on one occasion saw a patient with a strongly pulsating swelling high on the common carotid. It had been proposed to ligature the artery, and I was inclined to agree with the diagnosis and proposed treatment. As, however, the patient desired to go elsewhere for a time, and as there were no pressure or other urgent symptoms, I asked to see the

patient again. Two months afterwards the swelling and pulsation had so greatly diminished, that it was evident that further delay was demanded. After six months it had nearly disappeared, and I have not seen her again. Since that time I have seen two other cases of undoubted dilatation of the carotid. Like the first, they occurred in middle-aged neurotic women. One was specially interesting to me, because I witnessed in the space of about a year the enlargement of the upper part of the left carotid to more than twice its normal size, its gradual subsidence, and the subsequent development of a smaller enlargement, but with distinct tortuosity, on the right side, which also slowly went away.

In carotid aneurism the effects of embolism would, of course, be more serious than on a limb, but the probability of its occurrence is so small that no apprehension of that sort need deter from introducing needles. On the whole, I should prefer ligature if the aneurism were sufficiently high.

Traumatic Aneurism requires perhaps a word. There can be little doubt that in the great majority of cases the treatment should be by opening the sac. But it is easy to understand that there might be circumstances in which the risks of such a method might lead to the adoption of the needles. In one variety, electrolysis seems to me to be undoubtedly better in most situations than either the simple needles or any cutting operation—I mean the arterio-venous. On the smaller arteries I have met with invariable success. In the two cases of aneurismal varix of traumatic origin in the orbit, which were put under my care by Dr Argyll Robertson, we had a perfect success, which could scarcely have been attained by any other means. Certainly no other method is at once so safe and so effective. In operating in these cases the important point is to ascertain the wounded spot. This can often be done by lateral pressure with the puncturing needle. When it is found that by such lateral pressure the circulation can be arrested or greatly modified, there and thereabouts the needle is maintained and the current made to pass. I cannot speak from experience with relation to large vessels, but I am quite sure that I should try it there also in preference to any of the operations, including even

Mr Spence's, which I have seen performed on such arteries as the femoral and brachial.

The purely fusiform aneurism, wherever it exists, does not even in advanced stages lend itself readily to the formation of laminated fibrin. It is not a condition, therefore, in which either electrolysis or needling is likely to be very effective. Ligature, there can be no doubt, is here the appropriate treatment.

INFLAMMATION.

WE all suppose ourselves capable of recognising inflammation when we see it, but yet there is no subject on which confusion of ideas is more prevalent. This arises chiefly from the fact that we are continually using the word in different senses, and endeavouring to explain it by things which we do not understand. In common use the word is loosely applied to a combination of characters—pain, heat, redness, swelling. But these are mere symptoms which neither include everything that is inflammatory nor exclude that which is not. Neither is the combination essential, because any one of them may be omitted without destroying the identity of the disease. Clinically, such an idea is useful; pathologically, it is, of course, without value.

But when we come to ask what is inflammation pathologically, we find all sorts and kinds of opinion. One man maintains that the healing of wounds is by inflammation; another, that these are essentially distinct, and that a wound may either be inflamed or not inflamed. One holds that only the processes which follow the action of micro-organisms are entitled to be called inflammatory; another would extend the title to the results of a simple bruise.

In short, we use the word both to describe a collection of symptoms and a pathological process, and in so far as it applies to the latter we are far from agreed on the process to which it should be applied.

It is quite impossible that we should be able to determine the boundaries of inflammation until we understand the nature of life. Living things react to the influence of their environment, and inflammation has been defined as the

reaction of life to injury. But before this can be accepted as an adequate definition, plainly we must know what is life and what is injury. Well, it may be answered, we do not know what life is, but surely we understand what is meant by the word injury. It is gravely to be doubted if in this connection we do. I suppose we should include in it a bruise, or a cut, or a fracture, but it is quite plain that there is an infinite gradation from the most violent destruction of parts down to the most gentle pressure or friction. We have no doubt that the bacterium coli is injurious to the bladder; may it not be a healthful stimulus in the intestine? It is evidently, then, merely a question of where we are to draw the line; and inasmuch as there is no natural division in the influences of environment on life, so, necessarily, there is no line of division in the reactions of living things to their environment. The same confusion of ideas and the same use of words which hide ignorance, prevails in every other definition. When it is said that inflammation is essentially a process of devitalisation, we are simply defining by a word which itself is in urgent need of definition. It is another way of expressing "reaction to injury," if by injury we intend to imply that which tends to put an end to life. But it is absurd to say that the reaction is a process of devitalisation, when it is in truth an effort of life to avoid death. In short, there is no possible definition of inflammation so long as we cannot define life. We must content ourselves with studying processes (and much has been done in this way), and as far as may be possible in agreeing that words be used in certain definite senses.

If we use the word clinically, let us understand that we mean it clinically. We know, for example, that, pathologically, an ulcer is an inflammation, but we also know what we mean when we define it as inflamed. But do not let us convey the use of this clinical term into our pathological investigations. Then let us use it with a certain clear significance which we recognise must be purely conventional in our present ignorance. When the reaction of life to its environment is to be called growth, when maintenance, when stimulation, when inflammation, is not so

easy to determine, but let us recognise that such lines must be drawn for the sake of clearness, but that we must take them for what they are worth, as paper distinctions which will vary with advancing knowledge.

I do not see how, with our present knowledge, we can avoid the belief that all the processes of life are similar in type, growth, development, maintenance, decay, and inflammation. That being understood, all disease may be divided into the teratological and the inflammatory. On the one hand we have abnormal but inherent tendencies in reference to absolute or relative growth and development, *e.g.*, monstrosities, innocent tumours, and such like; while on the other we have inflammation proper, which is recognised as such by all, but we have also fevers which may be designated inflammations of the blood, blood being undoubtedly a living tissue, malignant disease (probably micro-organic in its etiology), tuberculous, syphilitic, and other inflammatory processes which owe their peculiarity to the specialisation of the producing cause.

CLASSIFICATION OF ULCERS.

MY experience as teacher and examiner leads me to believe that the usual tendency to neglect common things and devote attention to the rare and curious, is strikingly illustrated by the want of knowledge which students display of the appearance and treatment of that most common of surgical diseases, an ulcer. This want of knowledge is aggravated by the fact that the text-book descriptions are often dogmatically typical, and their classifications confused or illogical.

It ought to be understood, on the one hand, that a complete natural classification is, from the complexity of the problem, practically impossible; and on the other, that none can be regarded as satisfactory in which some ulcers are arranged on an etiological, others on a macroscopic, and yet others on a microscopic basis. We must try to avoid the condition of dermatology in general, which has suffered much from the man in the street having set himself up as a scientific nomenclator. Names and tradition, when old and established, ought to be accepted. We must not, however, accept them for more than they are worth, or suppose them to be scientifically accurate, when very likely they are expressions of a bygone pathology. Let me say a few words to you, then, in the first place, as to how you may so arrange things in your mind that you shall be able to apply your knowledge to particular specimens when they may come before you.

Now you may look at ulcers from the purely phenomenal point of view and classify according to appearances, or you may investigate causes and classify etiologically.

A classification from appearance or from the morbid anatomy must often leave out of account the true and essential factor, without which the ulcer would not exist, and a complete etiological classification is impossible, because the etiology of many ulcers is very complex, because the fundamental or primary cause is frequently overlaid by the accidental, and because different causes may produce very similar results. Moreover, our knowledge of intimate processes is insufficient for either method. We are practically, therefore, driven to use both plans, and in doing so the fact has too often been missed that they are essentially distinct, and that the attempt to use them together must fail because they are not mutually exclusive. The consequence is a haphazard list of ulcers, incomprehensive and confused because perpetually overlapping.

But if we take care to keep them apart, both systems may be of signal use to us.

Let us take first the consideration of ulcers according to the phenomena they present.

In that case, by much the most convenient way is to refer them to what is called the healthy or healing sore. Some have said, "That is not an ulcer at all. An ulcer means destruction of texture, this is reparation. They are not comparable conditions." The argument is unsound, because the healthy sore is that process by which nature heals a breach of surface, and to which you therefore desire to bring your ulcer in order that it may heal. Naturally, therefore, you say to yourself, "How does this particular ulcer depart from the typical and desirable form; why has it done so, and by what means can I bring it into line?"

I need not describe to you the healing sore—that you get fully in all your books. We study it in its surroundings, its edges, its base, and its discharge, and we refer to it in saying this or that ulcer departs from the healthy in one or more of these respects.

Now an ulcer is an ulcer, *i.e.*, departs from the healing type, because a process of inflammatory destruction usually is going on in it, and a classification according to its condition is therefore largely determined by the character and degree

of inflammation. In every inflammation, formative and destructive actions are struggling together. When it attacks a bone you see necrotic, carious, sclerotic, osteophytic, and other processes more or less prominent, and sometimes all together. It is precisely so in an ulcer of the soft parts, and so people speak of an ulcer with excessive or defective action, or again divide them into the Weak, the Callous, the Inflamed, and the Phagedenic.

It has always seemed to me that for general purposes that is a very good division, and at least the words, weak, and callous, and inflamed are too firmly established to be uprooted even if we so desire. I show you a specimen of each.

The great pale œdematous granulations and "blae" edges seem to me well expressed by the word "weak," and in like manner the word "callous" conveys very well to the mind the thick-edged sore and solid-looking leg in which there is a strong but perpetually frustrated effort to heal. When the destructive processes predominate, and the pain, heat, redness, and swelling are in evidence, the "inflamed" is the natural name.

But then you must remember that these are types, and that the modifications and comminglings are infinite. They must be used only as types, and not even as types are they inclusive of all forms. There are certainly ulcers which you cannot include under any or all of them, even from a phenomenal or inflammatory point of view. Let me illustrate. Take first the type of types, the healing sore. Under our modern antiseptic dressings we have discovered two modifications. The first is that creamy, laudable pus is not a necessity. You may get what has been called healing under a crust, and you may artificially prepare a crust for the ulcer to heal under. A very pretty experiment may be made in this way in cases in which it is necessary to apply the actual cautery. If you prepare the part properly, and cover it with flexible collodion or celloidin, or such like, and keep it at rest, at the end of a certain time you will find the sore healed, and over it a tiny, shrivelled representative of the destroyed textures, or, perhaps, none at all. Just as with a killed portion of bone the surroundings may eat it up

altogether, or may throw a small piece off. It is done without pus, and if at any time (it is a very slow process) you take off the dressings and look at it, you will find, till near the very end, the dead parts continuing centrally in adhesion to the living, and the typical healing sore represented by a belt of epithelium, and a narrow band of granulations between it and the slough.

The second variety with which we are now familiar is that which may be called the exuberant healing sore. In protecting by antiseptic dressings from surrounding contamination, it is impossible to prevent a certain gentle stimulation by antiseptics, by warmth, and by moisture. The consequence is that the granulations become florid and large, the sore grows gently convex instead of concave, and the discharge is thinner and less copious than the *pus bonum et laudabile*.

Even our central type has recognised variations.

And so it is with all the typical ulcers. Now and again you see a weak one, or a callous, or too often an inflamed, but in the great majority of cases they are mingled or varied. It is often better, instead of trying to give a name, to say to yourself: "This sore departs from the healthy type because in whole or in part its edges are thick or undermined, its granulations large, or small, or absent; or red, or pale, or yellow; or because in some other way a modification occurs." After all, you classify merely that you may understand, and the next question you put to yourself is—What process is, for example, going on that has caused this modification in appearance; how is it one ulcer has large pale granulations, another greyish-yellow, or another thick edges, or yet another dark blood extravasations in its substance? And you find that the large pale granulations are so because they are œdematous, and you start immediately on a search into the meaning of œdema, and begin to think of heart, and kidney, and liver, and veins, and curious neuroses; or you find that the grey-yellow hue comes from coagulation-necrosis of cells; and the action of chemical substances on albumen, and the interaction of micro-organisms, and the body cells rise before you; or again, you find that the extravasation has to do with the structure of granulation tissue, in which there is no nerve, but only blood-vessel

and cell, and you remember the analogy of sarcoma. The whole of surgery is summed up in an ulcer.

By an analysis such as this, by thoughts such as these, you are at once thrown back for practical purposes upon a study of causes and a classification by etiology, and it becomes evident to you further that the explanation of superficial appearances which we have been classifying do not exhaust the etiology of an ulcer. Let us, for example, take this very common ulcer of the leg. You see that in the main you may class it as a callous ulcer, by its thickened edges and surroundings, and its smooth base, but that at one part it has become inflamed and sloughy. Now, empirically, we know that such an ulcer is special to the lower third of the leg. The anatomical conditions of the limb, the thinness and tension of its soft parts, the pressure of the circulation, especially the difficulties of its backward flow in the erect posture, impede the healing of a breach of surface. Exposure to friction and septicity complete the condition. Nature is perpetually making an effort to heal it by throwing an increased supply of blood to the part, but the retrograde circulation being insufficient, there is immediately a tendency to œdema, to lymph formation, and the piling up of effete material. Granulations strive to form, but are as quickly destroyed by organic and mechanical irritants; and a smooth base or an active inflammation will indicate the degree to which these have been allowed to act. This, then, may very well be called the ulcer of the leg. Indeed, as a means of diagnosis, it is useful always to remember that if a sore is found above the middle of the leg, it is not due to the causes we have just been discussing. It may be constitutional or local, but it is no longer an ulcer of the leg. But the converse is by no means always true. Doubtless, in every case, the circumstances of the lower third of the leg produce a modification in appearances, but there is nothing to prevent whatever may cause an ulcer elsewhere being here also operative. Now in this special case there is one other condition which has been even more effective than those we have mentioned, which, in fact, has determined the ulcer. You will observe that the brawny leg is eczematous, that from the ankle for a considerable distance

upwards it is strongly pigmented, and that it and the ulcer are a dark blue-brown. Although you see no salience, you can, by passing your fingers lightly over the surface of the limb, easily distinguish a tortuous internal saphena vein running in an irregular gutter deeply cut in the solid œdema. You have a varicose vein which has impressed its character on the ulcer. Of course this carries you farther back to the question—Why should varicosity produce and maintain a varicose ulcer? It is not so easily accounted for as one would at first sight suppose. But for our present purpose it suffices that we have here an ulcer which depends upon a variety of causes, and that before you can adequately describe it, you have to call it a varicose ulcer of the leg which has become callous and inflamed.

In the same way it is essential for practical purposes that you classify all ulcers on the one hand according to their origin, and next, according to the condition with reference to the healing sore which they present. Thus you have on the leg, ulcers which depend, like the varicose, on circulatory aberrations, such as the senile ulcers connected with atheroma and others, which are associated with neurosis, as that which was described as the cold ulcer by Paget, and which has its relations on the one hand with Raynaud's disease, on the other with ergotism. You have ulcers which may occur on any parts of the body, as those which are due to the organisms of tubercle, or syphilis, or malignant disease, or which arise from nerve or tissue injury. In short, with a little care, a tolerably complete classification of ulcers may be made on this basis which shall be logical and sound, and only requires to be added to as fresh discovery is made. And so, as I have already said, you require two concurrent systems of classification. No doubt every ulcer has a tendency to assume certain forms in virtue of its primary cause. The pale aspect, the blue undermined edges, and the thin discharge of the tuberculous sore, are very characteristic. But every one of them may, according to circumstances, present very various degrees of inflammation, and may be sloughing, phagedenic, inflamed, weak, or callous.

The chief importance of these classifications lies in their

bearing upon treatment. And here again we must refer to the type of the healing sore. At once two classes of questions arise. The first concerns the bringing of an ulcer into this state ; the second, the management of it when there ; and it is plain that the second must largely depend upon the first, and that certainly one part of the treatment of a healing sore must consist in obviating the tendency to departure from it.

Now the bringing of an ulcer into a healthy state necessarily involves the counteraction to its causes. You inquire, as you did in classification, what has produced this ulcer, and why, at this moment, does it present this special form of inflammation ?

OSTEOMYELITIS.

E. F., a little girl aged 11 years, was sent into the Medical House in February 1891, suffering, as was supposed, from rheumatic fever. The diagnosis thought at first probable in the medical wards was acute tuberculosis with the chief incidence on the cerebral meninges. After a few days, however, swelling appeared about the left shoulder, and she was transferred immediately to my ward. I incised, found the upper part of the humerus surrounded by pus, opened the medullary cavity, and discovered that it was studded with foci of yellow-green inspissated pus, which extended throughout nearly the whole length of the bone. I chiselled away the roof of the medullary cavity and scraped away the whole interior. After a long illness the patient got well, but not without two farther operations for the removal of small portions of necrosed bone.

One feature of this case is that no immediate cause for the attack could be discovered. That some breach of surface affords a mode of entrance for the staphylococcus is certain, but in the majority of cases no special local inflammation indicates that the open door has been a decayed tooth, a scratch, or an internal ulceration. Very constantly, however, the attack is referred to exposure or injury. Most curiously, I had at one and the same time in my wards two little boys with osteomyelitis of the tibia. The history was almost identical. Each had spent with his parents a long and happy day on the seashore—one at Musselburgh, the other at Portobello. The next day both were exceedingly ill, and with the local signs of ostitis of the tibia. A blow or a second blow on the same site

is often recorded, but, curiously enough, I have never met with a case in which it has followed a simple fracture. I suppose that one or more wandering cocci having found their way into the circulation, find also a suitable breeding ground in bone, a tissue to which they are partial, and which, being from one cause or another in a state of depressed vitality, is unable to resist the attack. I take it that the general systemic reaction is usually produced by the local disease, because we find it to vary with the success of our local treatment, and that had the cocci not reached this appropriate soil they would have been harmlessly eliminated.

That the soil must be highly specialised we see in the proclivity of certain ages, certain tissues, nay, even certain parts of tissues. In bone, the favourite point of attack, as in this case, is the end of a long bone, whence, as it spreads, it tends to pass along the shaft. Not that the epiphysis or any cancellated texture is immune, but it is interesting to note that even if it be attacked it is only when the onslaught is positively fulminant that it reaches through it to the neighbouring joint.

Another point of importance in the case is the difficulty of early diagnosis. There are many in whom the local incidence of the disease is at once evident. But there are others, such as this, in whom the osteitis is a somewhat late phenomenon. Ulcerative endocarditis may complicate and obscure the diagnosis, pneumonia is not uncommon, and in this case we see that in the early stages the disease closely simulated rheumatic fever, and later strongly affected the central nervous system. There are three diseases, indeed, which in their incipience closely resemble each other, viz., rheumatic fever, some forms of acute tuberculosis, and septic infection.

Yet another difficulty is illustrated by this patient. It is universally recognised that the moment septic osteitis is diagnosed the proper course is to incise immediately down to and through the periosteum, with antiseptic precautions. But it is by no means so clear when we ought to be content with this measure. It is often successful. I have over and over again, even in acute necrosis of the femur, which in

old days was so fatal, at once arrested the disease by simple incision. I am not sure that we quite understand its success, because the septic organisms are already there. But it is successful. Not always, however, because the disease is not always limited to the periosteum. It may have already attacked the medulla, and the difficulty I have felt is so to distinguish as to be able to say in this case we must go farther and open the medulla, in that we may be content with periosteal incision. Of course if, having assured ourselves by the local signs of inflammation and otherwise that there is septic ostitis, we find merely œdema of the periosteum on incision, we are bound to go farther. But in the very early stage the attack may be periosteal and yet pus not have had time to form. An early whitlow is a good example. But even in so deeply placed a bone as the femur the same may be found. A lad, æt. 18, had a severe rigor and high fever, and was brought into my ward with intense tenderness and a little swelling of the lower third of the femur. I incised, and found merely a little opaque serum, but the fever immediately subsided, and the wound healed without even a scale of necrosis.

Later, I think we may to a certain extent be guided by the sequence of symptoms. If the pain is exceedingly severe, and the general symptoms serious, while the swelling is at first slight, we may conclude that the focus of disease is medullary.

THE OPERATIVE TREATMENT OF TUBERCULOUS ARTHRITIS OF THE KNEE.

THERE are at present in the wards five cases of tuberculous arthritis of the knee. They have been operated upon, and their wounds have all healed without the formation of a single drop of pus. One is a woman, aged 25 years, whose limb I have amputated above the knee; three are cases of excision, aged 15, 23, and 27 years; and one is a child, three years of age, in whom I have cut away the diseased synovial membrane. They are, therefore, typical of the varieties of operative treatment for tuberculous arthritis, and may serve as text for a few practical observations.

In a preliminary way, I would strongly emphasise the proposition that tuberculous arthritis is eminently curable. Tubercular disease, if it do not attack vital organs, generally is so. In the struggle for existence between the elemental tissues of the body and the intrusive bacillus, the latter is often worsted.

The study of the conditions which favour one or the other of these antagonists is of the profoundest interest to the clinician, and, without depreciating other factors, we may take for certain that one essential, or at least invariably applicable, condition of successful treatment, so far as surgical and probably also medical forms of tubercular disease are concerned, is the enforcement of absolute rest. It would be interesting, but not germane to our present subject, to speculate on the *modus operandi*, and to inquire how rest, which leads to atrophy of the tissues of the body, should

at the same time enable them to destroy the vitality of the intruders. It may be that in the effete products of work microbes find a suitable soil. Suffice it, however, that rest *is* effective, and that, if made absolute, or, as it is called, physiological, it rarely fails to cure.

But, while this is so, the difficulty of applying it in any adequate way is sometimes very great. In certain parts of the body it is impossible, although I venture to maintain that often, as, for example, in tuberculous glands, the possibilities are not sufficiently utilised by the surgeon. In the poor, again, as compared with the rich, the necessary time is not at our disposal. Among the upper classes the rarity of operation for tuberculous disease is largely due to the fact that immobilisation can be maintained over a sufficiently prolonged period of time.

It is a common history among our hospital patients, that steady improvement continues so long as they are kept in the Infirmary, and that relapse occurs on their resumption of the work that they must perform in order that they may live. After, it may be, frequent repetition of this alternation, an abscess forms and opens. The too common sequel is that, having opened, it becomes septic; having become septic, the disease makes more rapid progress than before.

There can be no doubt that the influence of the different forms of microbes upon each other is very various. Some are antagonistic. There is an antagonism between the microbe of erysipelas and that of tubercle. Fehleisen has shown it in lupus. I have observed it in tuberculous arthritis. I have before me the case of a boy in whom I excised the elbow. Septic at first, it remained so after operation, and, notwithstanding scraping out of the sinuses on more than one occasion, they would not heal, and persistently exhibited characteristic tuberculous granulations. One day he came to the Infirmary, and was sent to the Contagious Hospital on account of erysipelas. With the subsidence of the erysipelas the sinuses quickly healed. The case is not isolated in my experience, and I have myself no doubt of the antagonism. On the other hand, ordinary pyogenic microbes and tubercle are adjuvant to each other. If a

tuberculous arthritis become septic, the prognosis becomes at once most serious, and the too frequent result is exhaustion, hectic, amyloid degeneration, and death.

Such, then, is the common history of a hospital patient with tuberculous disease of the knee. I do not deny that the disease may be arrested spontaneously, even in the poor, at any stage of its progress. But with them it is a dangerous disease, and we have often to consider what measures should be taken when rest fails, either because it cannot be adequately or long enough applied, or because the important hygienic adjuvants are wanting. These circumstances naturally, also, may determine the sequence and period of application of these measures, both in relation to rest and to each other.

If, then, rest fail from one cause or another, surgery has three operative resources. I say nothing of simple incision. If it be time to incise in tuberculous arthritis of the knee, it is time to go further, for its curative value is less than, and its risk nearly as great as, that of other operative measures.

The three operations are :—

1. Partial excision.
2. Complete excision.
3. Amputation.

Let us take them in that order.

Partial excision is a method of operating of ancient date, rendered obsolete for a time by complete excision, but revived in improved form under the name of arthrectomy. The reason for the revival is the increased safety which is given to it by the antiseptic system. That system has so reduced the mortality of all these forms of operation as to make it an element of much less importance than it was formerly in determining the choice between them.

Arthrectomy, then, or partial excision, consists in the removal of all palpable and obvious portions of diseased texture, whether in synovial membrane or elsewhere, without regard to what is left, provided only it be apparently sound. That, I think, is a not unfair description of the operation, and I use the words palpable and obvious, because no one, I

suppose, will contend that it is possible, in such an operation, to insure, or even render probable, the removal of all texture in which the tubercle bacillus exists.

Two advantages are claimed for this operation. It is asserted, and with some truth, that, unlike the other methods of operating, it does not interfere with the growth of the limb. This advantage, of course, applies only in the case of children. But it must not be forgotten that arrestment of growth is often the effect of the disease. A very frequent situation for the development of tubercle is the epiphysial junction, and a common result is the ossification or destruction of the point of growth.

A second advantage has been claimed for arthrectomy, that mobility of the joint can be thereby preserved. There is in my wards at this moment a young woman on whom I operated nearly three years ago, by removing with scissors and sharp spoon the whole of the gelatinised synovial membrane of the knee-joint. The wound healed by first intention, and, till three weeks ago, she has been able to walk about and freely use her knee without pain up to a right angle. She has come back now because of a little pain from prolonged standing and walking, which has completely subsided with a fortnight's rest. About the same time, in a child, I erased a large part of the external condyle of the femur and the whole of the synovial membrane. It required long rest in splints, but for a year and a half she has been walking about without pain and with slight but useful movement of the knee. I look upon these, however, as quite exceptional, and I think that all arthrectomists now advocate prolonged rest after operation, and aim at ankylosis, if not osseous, at least as firmly fibrous as can be obtained. And, undoubtedly, they are right in so aiming. There are great risks in a remanent mobility. I have, in one instance, been obliged to excise the joint in a little boy on whom, a year and a half previously, arthrectomy had been performed by another surgeon. There had followed complete cure of the disease, and the ankylosis was so firmly fibrous that it was only with care that any movement could be detected. Nevertheless, under the strain of walking, the knee had gradually

become more and more bent, until it was nearly at a right angle, and the patient could only walk on his toes. Moreover, sudden strains or falls were apt to produce severe pain, lasting on each occasion for a day or two. Therein, in truth, lies an important difference, so far as the after-consequences of both arthrectomy and excision are concerned, between a prehensile organ like the arm, and one meant to bear weight like the leg. Fibrous bands will stand much strain in the way of direct tension, but little in the way of pressure or lateral jerk.

Another disadvantage of even very slight remanent mobility lies in the fact that it greatly increases the chance of recurrence of the disease. After excision of the knee, if osseous union occur, the disease is cured; and of a large number of such cases I have met with only one in which the disease recurred. I excised the knee in a lad of eighteen years, a patient of Dr Young, of West Calder. Osseous ankylosis followed, and, for more than a year, the limb was perfectly sound and useful. He was, however, a very tuberculous subject, having suffered much from suppurating glands in the neck, and he was lately brought back to me with recently formed sinuses, which led down to carious bone on the posterior surface of the tibia. I scraped with Volkmann's spoon, and he is now well. It is, in my experience, a solitary case.

There is a distinct difference in this respect, also, between excision of the knee and excision of the elbow. I put aside cases in which the operation has been performed under septic conditions, although I believe the same observation applies. But in excising with unbroken skin, the rule is, that union takes place by first intention. In such a case osseous ankylosis of the knee is fairly certain, and also free mobility in the elbow. I have only once seen this recrudescence of the disease in the former when ankylosis was osseous. I have three times had to lament it in the latter. Take one as an example: I excised the elbow in a lad of fourteen years, and having taken care to arrest hæmorrhage, to remove all palpable disease, and to avoid antiseptic irritation, I secured union absolutely by first intention, no drainage tube having

been introduced. For six weeks everything progressed admirably, but at the end of that time the parts became slightly swollen, the cicatrix assumed a broad, puffy, bluish appearance, and sinuses with pouting granulations slowly formed, and were only healed by prolonged rest and scraping. The ultimate result in this case, as in the other, was good, but only attained with care and anxiety, and I attribute the untoward condition solely to the necessity for maintaining the mobility of the joint by daily movement. Nor can it be said that this movement was excessive, for I avoid entirely the old rough and pump-handle treatment, and confine myself to extension with a light weight during the night, and carriage in a sling with very gradually increased flexion during the day.

Now, in arthrectomy of the knee, in which it is difficult to secure osseous ankylosis, because soft parts may be allowed to remain and surfaces are unsuitable, this risk is always present. I have experienced it in two cases.

A young woman, one of the five first mentioned, had the synovial membrane very carefully removed by means of long incisions, one on each side of the joint, in May 1886. Union took place by first intention, and for four months the patient wore a plaster case. For a time she was able to walk with considerable movement of the joint, but for a year the leg has been under almost constant treatment at home, and when she returned to the Infirmary, two months ago, I felt constrained to excise. Another was yet more unfortunate. In a child, six years old, I employed arthrectomy in May 1888. The joint healed absolutely by first intention, and after healing was enveloped in a starch and poroplastic case. The joint was not again seen till twelve weeks after the operation, when a sudden rise of temperature occurred, and on examination, an abscess was found to have burst and become septic. Under this the child went down so rapidly that, during my absence in September, Mr Hodsdon found himself obliged to amputate to save the patient's life.

My conclusion, therefore, in reference to the operation, based on my own experience of it, and not contradicted by

the published results of other surgeons, is that it ought, meanwhile at least, to be very strictly limited. The cases in which it may legitimately be tried are, in the first place, those only in which a thorough and prolonged treatment by rest has been unsuccessfully employed. If the patients come under observation early, this want of success should be rare. A second limitation should be to those in whom the skin is as yet unbroken; because the operation being less thorough, the drainage more difficult, and complete ankylosis less sure, recurrence of the disease is more probable, and the risk of the procedure not less than with more radical treatment. Again, it should be employed chiefly in children; because in them only can the main advantage, continuance of growth in the limb, be of any avail, and in them only may the accidentally favourable mobility be legitimately in some cases expected. I would not assert that further extension should not be given to the operation in exceptionally favourable cases, when, for example, the personal and family history are free from tubercular taint, the patient is otherwise robust, and the disease slow and not purulent. But youth and asepsis seem to me nearly essential conditions.

For other cases of tuberculous arthritis we have the choice between excision and amputation, and I must say that with extended experience I am inclined to increase the number of cases in which I recommend amputation. I suppose that in all cases amputation is comparatively safer, and the point to be determined is where the line should be drawn which indicates that the greater safety is counterbalanced by other advantages. It may plainly vary with variation, either in the element of risk or in the benefit derived by retention of the limb.

And first, it may safely be said that the danger attending either amputation or excision in a young and healthy person with unbroken skin, is so small that the difference in risk between these operations may be treated as a *quantité négligeable*. In such, therefore, there can be no doubt of the propriety of preserving the limb, provided always that the disease can thus be fairly removed. I should not insist

that it be absolutely and entirely removable, because, undoubtedly, the removal of the major part of the disease, the relief of tension, and the completeness of immobilisation in many cases will allow the vitality of the texture to reassert itself, and overcome the small portions of disease that remain. But I regard total removal as an important object for several reasons. These are, first, that a remanent focus of tubercular disease implies a long-continued threat of recurrence ; second, that osseous ankylosis is more difficult to obtain when tubercular foci are present ; and third, that dissemination of tubercle through the system is a result which does arise more frequently than is generally acknowledged when a large, raw surface, especially of bone, is exposed to tubercular infection.

There can be no doubt that tubercular disease may be produced by inoculation. The conditions of such inoculation seldom, of course, arise, but the following case is one of considerable interest in this connection.

A woman, æt. 36 years, was admitted to the Royal Infirmary in November 1887, suffering from three nodules on the hand, each about the size of a walnut. Two were close together on the back of the hand over the fourth and fifth metacarpal bones, one about an inch away over the lateral aspect of the wrist. The lowest had already opened, and all presented the clinical aspect of tubercular abscess. The history was that, in washing some handkerchiefs impregnated with sputum from a sister who was dying of phthisis, she deeply pricked her hand with a safety-pin attaching them together. The little wound inflamed slightly, and a few drops of pus came out, but it healed again in a week or ten days. Seven or eight weeks subsequently, a nodule began slowly to form at the spot ; a week or two later, a second, higher up ; and the third on the side of the wrist, considerably later still. I scraped them out and passed drainage tubes from one to the other, finding them connected by diseased texture, and after a second scraping, six weeks afterward, they healed soundly. The tubercle bacillus was found in the scraped-out material.

It is easy to understand, and it is consistent with experience in operations for tuberculous disease, that the raw

surface may become inoculated by diseased matter left behind, and the progress of the disease be more rapid than before.

Osseous ankylosis must be difficult to procure, and, indeed, there can be no other reason for the long delay in attaining it, and the occasional total failure which occurs in excision of the knee. A fracture through cancellated texture rarely fails to unite. The difficulty rather is to prevent an ankylosis of the neighbouring joint. Now, an excision is specially prepared for union; the surfaces are made as broad as possible, and usually special apparatus, as sutures of wire, are used to secure accurate apposition. But there can be no doubt that delayed union is common. In most of my excisions I find that mobility can be detected easily at the end of eight, ten, and twelve weeks, and I do not find that union is more rapid in aseptic cases, which heal by first intention, than in those which have been done through septic sinuses, and which I have failed to render aseptic. I think it not impossible, indeed, that the aseptic condition may diminish the rapidity and certainty of osseous union. I have been inclined to think that, so far as osseous union is concerned, operations for ununited fracture were more successful in old septic days than now. Much more dangerous they doubtless formerly were; but, if they did not kill, they succeeded, presumably from the hyperæmic results of inflammation. But this is scarcely an adequate explanation of delay in excision. It is, if aseptic, precisely in the same condition as a simple fracture or a compound fracture kept aseptic. But the delay occurs whether it be septic or aseptic. There must, therefore, be some factor special to the condition, and this lies, doubtless, in the state of texture brought about by the disease, and which must be aggravated if foci of tubercle be left behind.

But again, after operations through tuberculous tissue, there is a danger of dissemination of tubercle. This occurs in two ways. Much against my will I operated by excision in a case of tubercular disease of the knee-joint in a young man in whom the disease was so extensive that I strongly advised amputation. He would not submit, and died of

miliary tubercle, chiefly in the lungs, a few weeks afterward. These cases have, I think, sometimes been regarded as septic poisonings, to which, in many respects, the symptoms bear a close analogy. They certainly occur more often than is generally supposed, and especially after operations in which large surfaces of bones are exposed, and in which perfect drainage from these surfaces is in the nature of things impossible.

Another mode of infection has also, I think, failed to attract adequate attention. I scraped out the interior of the os calcis, as I thought very thoroughly, in a tuberculous abscess of the interior of that bone, and in doing so succeeded in rendering it aseptic. Four weeks thereafter, and before the wound had nearly healed, tuberculous arthritis of the wrist declared itself, and ultimately required excision. A boy of thirteen years was brought in with very extensive tuberculous osteitis of the left ulna, which had been going on for many months. He had no other lesion at the time. I removed a large exfoliation and scraped out the interior of the bone throughout nearly its entire length. Shortly after the operation, which was thoroughly successful locally, there appeared one after the other, osteitis of the crest of the right ilium leading to cario-necrosis, necrosis of the right frontal bone, and antero-posterior curvature of the spine in the region of the second dorsal vertebra. I could multiply such cases considerably, and even mention instances in which apparently successive showers of diseased material have been deposited from the circulation. These will serve, however, to illustrate the proposition that dissemination occurs after operation, especially in osseous tubercle, not only in the way of miliary tuberculosis, but also in a more limited manner, when the tubercle introduced into the circulation is probably less abundant, and chooses, as it originally did, a suitable soil for its development.

These considerations, then, must form an important element in the determination of the nature of the operation to be attempted. As regards total removal of disease, they are most serious in connection with arthrectomy, less so in excision, least in amputation. Accidents, again, will plainly

be most liable to occur when the disease is extensive, and one or other mode of operating will be, therefore, adopted according to the extent of the disease. But they will, also, be prone to happen in proportion to the size of the wound, and especially of raw osseous surface, and in this respect the relative value of the operations varies in different cases, arthrectomy in some having the advantage. It is evident, therefore, that no hard and fast rule is to be laid down, and that in every case many factors must be carefully weighed, one against another, before the surgeon can decide upon the appropriate operation.

I have already said that in a young and comparatively healthy person with unbroken skin, the risks of amputation and excision are so small, and differ so little, that preservation of the limb should be the rule. This proposition implies that age, health, and septicity, and to them I would add the social status, are factors of no mean importance in determining our choice. We incline to amputation in preference to excision; the older a patient is the more his constitutional vigour has been impaired or is naturally defective, the greater the damage sustained from putrefactive conditions, and the less the pecuniary and hygienic circumstances are capable of affording the means of a long struggle with disease.

I feel convinced that, with increasing experience, the surgeon tends more and more to conserve the life rather than the limb, the limb rather than the joint. As each advance in surgical therapeutics diminishes the danger of conservatism, he draws his line back in accordance with the logical consequence of the improvement. Where the comparative risks differ but little he preserves, but the tendency with many is to extend a new improvement further than is legitimate, and it is well, in such circumstances, to draw attention to the conditions of the problem, and in this particular case to define, so far as may be, the reasonable limitations of arthrectomy as compared with excision and amputation.

EPITHELIOMA OF THE MOUTH

With Observations on the Etiology of Malignant Disease.

THE ulcers with which epithelioma is most apt to be confounded are the syphilitic, the tuberculous, and the simple inflammatory.

A. B., æt. 46, was admitted to the Infirmary for the purpose of undergoing an operation for epithelioma. A large, ragged sore occupied the right angle of the mouth, and had led to considerable destruction of tissue. Thence it passed to the alveolus of the upper jaw, where the only remaining molar tooth was embedded in a mass of granulations, and still further it stretched over the roof of the mouth as far as the soft palate.

A doubt as to its malignant character was excited by its situation. When the contour of the closed lips was surveyed the ulcer appeared to extend over an equal area of the upper and the lower, but when the mouth was opened it became evident that the chief extension of the disease was upwards, and the patient very distinctly states that it commenced *above* the angle. Now I have never met with a case of epithelioma of the upper lip properly so called. It may extend to the upper lip, it does not commence there. You see it on the inner surface of the cheek, especially opposite the molars, where irritation from jagged points is frequent. You see it on the skin of the lip, as, for example, at the angle of the nose, or where moles or warts have preceded it, but, whatever be the explanation, the fact remains that it is exceedingly rare where skin and mucous membrane join to form the upper lip.

The doubt as to its malignancy was strengthened by certain points in the character of the sore. I laid little stress on the hardness which surrounded it. There are forms of epithelioma in which the hardness is pathognomonic, but it may present every variety, and where there is much ulceration the texture has often little more than the brittle softness of ordinary granulations. Here the denseness resembled the inflammatory in degree and kind. Nor did I attach importance to the absence of distinct tumour growth. It was sufficiently rugged, and in parts exuberant. Moreover, in epithelioma there is every variety in the relation between the growth and the destruction of the new texture. In some, great masses of raw growth project outwards like a cauliflower; in others, a thin rim is the sole representative of tumour growth. But I was much impressed by the fact that there was a space of unbroken surface which separated the anterior from the posterior portion of the ulcer on the palate. It was inflamed, this narrow bridge, and it was difficult to say whether it was mucous membrane or a scar. But whether or no, that is not the usual behaviour of epithelioma. It does not skip portions of surface, nor does it scar over again when it has once destroyed. That bridge was syphilitic like.

It was very difficult to get from him a definite history of his case. We could not ascertain that he had suffered from syphilitic symptoms. But so far as primary syphilis is concerned, veracity very constantly departs upon the entry of the virus, and he was quite sufficiently unobservant, forgetful, and stupid, to be unable to give details of secondary manifestations. Upon two points, however, he was perfectly clear: that the sore began on the upper lip, and that it commenced not more than four months before his admission. It is fair to say that so rapid a spread of epithelioma is uncommon. At least in its early stages, the history is comparatively slow and often preceded by months or years of slight irritation.

Lastly, we took scrapings from the surface of the sore in several places and examined them microscopically. In doing so we first carefully cleansed the surface from discharge,

debris of food, and other matters which can only serve to confuse, and then, taking a little of the surface, squeezed it between a slide and cover glass. Perhaps it is well to take a larger and deeper portion for hardening and staining, but I doubt myself whether greater certainty is thereby attained, and it is not so quick and convenient. We have found by careful examination, in this case, nothing but granulation cells, blood cells, and debris. Now in epithelioma you find either cell nests or cells of epithelial type, tending often to the ovoid, some granular from fatty degeneration, others with huge and lustrous nuclei and nucleoli. I do not say that these things are pathognomonic. I doubt whether anything short of careful examination of a whole growth, if always even that will enable the microscopist to give a decided opinion. But if in a case with the clinical character of epithelioma you find these cells, it makes your diagnosis sure, and if in such a case as this, with the points in the history, situation, and appearance to which I have referred, you find that they are absent, it may safely be assumed that it is not epithelioma.

If not epithelioma, we came then speedily to the conclusion that it was a tertiary syphilitic sore. It was plainly not irritation, for there was no cause of irritation, and its appearance and situation negatived the supposition that it was tuberculous.

I directed, accordingly, that the patient should frequently wash the ulcer with a weak solution of bichloride of mercury, while internally the same drug was administered in combination with iodide of potassium. In a month he returned home with the ulcer absolutely healed.

Tuberculous ulcers are not so liable to be confounded with epithelioma. If anywhere, however, they may present difficulties in the tongue. They are undoubtedly rare in that situation, but the following is a well-ascertained example.

D. P., æt. 51, admitted November 25, 1886. He had suffered for more than six months from an ulcer of the tongue, which had from time to time got "better and worse." He was always a heavy smoker, but had given it up since the tongue became sore.

On the anterior portion of the dorsum of the tongue was a

circular area, an inch in diameter, occupied by dry, greyish, irregular plates of epithelium, separated from each other by cracks and fissures, but merging insensibly into the ordinary epithelium, except towards the left anterior margin. At that point was an ulcer about the size of a florin, with thickened overhanging edge and hollowed base. The granulations, which were large but pale, gave off much watery pus.

The patient was so ill on admission, weak and scarcely able to speak, that a thorough examination was delayed. The temperature, however, never fell below 102° , the whole chest was full of moist râles, and there was much dulness, worst towards the apices. He died a few days after admission. Mr H. A. Thomson found on post-mortem examination the tubercle bacillus in abundance in the ulcer and lungs.

A definite diagnosis had not been made before death, but the weak and exuberant granulations, the soft character of the surrounding swelling, the copious discharge, the position towards the tip of the tongue, and the concomitant condition of the lungs, had excluded the possibility of gumma or epithelioma, and I think there can be little doubt that with farther opportunity and the use of the microscope, what was suspected would have been definitely established. The presence of the so-called psoriasis of the tongue was interesting.

The simply inflammatory sore is perhaps more often difficult to differentiate from an epithelioma than any other, and it is not astonishing that it is so, for the one passes into the other by insensible gradations. Take this case:—G. F., æt. 61, came as an out-patient to the Infirmary in May 1887. He had had a sore on the lip for 15 years, which had healed and broken out many times under the influence of the pipe. Three months before he showed himself it had definitely established itself, and the intensity of the induration and its depth left no doubt as to its character. But it is plain that in such a case there must have been a time when neither history, nor appearance, nor microscopic examination could by any possibility determine whether the invasive period had begun or not. There can be no question that in these doubtful cases the true course is to regard the disease as an epithelioma, and act accordingly.

Etiology.—In asking oneself the question, "What is the cause of epithelioma?" you immediately begin to inquire what are its analogies? The natural answer would be, "To other tumours." Nothing, I think, could be more erroneous. To other malignant tumours? "Yes." To tumours in general? "A very slight relationship." Malignant and innocent tumours are essentially distinct. The innocent tumour has its analogy in teratology. It may arise in a "belated rudiment," or a big blood-vessel, or a fusion of ova, or in other ways with which we are not concerned, for the malignant tumour has nothing to do with them. The innocent tumour grows as an independent entity, pushing aside its weaker brethren. But the malignant tumour has its analogies in inflammation, in syphilis, in tubercle, in glanders, in leprosy, and other diseases of a like type. Their analogies are, indeed, in many respects so close, that it is difficult to withhold assent to the proposition that they ought to be classified etiologically together.

I have already pointed out that so far as epithelioma is concerned, it is prone to be confused, specially with the products of three diseases. That, of course, means that their clinical history and appearance present distinct and numerous points of resemblance—so much so that in many cases it is a matter of extreme difficulty to formulate a definite diagnosis. In the first place, they are all distinctly invasive, they eat up their surroundings, and grow by doing so.

If, especially with antiseptic precautions, any one watches the course of events after opening a periosteal abscess, he will be greatly struck by the changes which occur. The bone exposed to view is at first white and dead, but presently there spring up the most beautifully delicate pink longitudinal little lines, which slowly increase in size, and ultimately coalesce. The dead white bone has been devoured by the soft granulations which now occupy its place. The same power of destruction is visible in inflammatory action in the syphilitic gumma, in the tuberculous nodule, in the malignant tumour. There are modifications of a specific character, undoubtedly, and there are therefore etiological divergencies, but this similarity is striking and important.

But again they are all prone to be carried to, or rather, I

should say, to appear secondarily in distant organs. Some follow the course of the blood path, some pass along the lymphatics more easily, but they have this property in common, that being so conveyed, they breed their like wherever they may find themselves provided with a soil favourable to their growth.

Yet, again, is there not histologically a very close resemblance among them? Is granulation tissue not the type to which they must all be referred? Are there not gradations so infinite that no microscopist can tell when one begins and the other ends? It is certain that in a multitude of cases the most careful examination of the histology is only valuable when aided by a knowledge of the situation and history of the tissue which is being examined.

With points of similarity such as these it is impossible to come to any other conclusion than that they have some strong analogy in their etiology. Now we know that tubercle is the result of irritation by a bacillus; we cannot avoid the conclusion that syphilis is due to a *contagium vivum*, and in inflammation the presence of a microzyme can usually be demonstrated. I believe that the time will come when malignant disease will be proved to have a similar origin.*

Before adopting such a hypothesis, even when probability is so strongly in its favour, it is essential that it should be capable of explaining more or less completely the known facts, and that it should not be incompatible with any. That it fits in with the invasive power and the secondary developments of malignant disease, I have already shown. Let us see if there are any characters with which it cannot be reconciled.

The difficulties in the way of accepting the microbic theory of the disease may be summed up in the following points:—Epithelioma is said not to be contagious. If it be taken along with other malignant diseases to which it is closely allied, there is presented a variety of form which can hardly be attributed to a single microzyme. It is undoubtedly prone to be excited by irritation. It has a tendency to be hereditary. The primary tumour is usually

* This was written about 1888.—ED.

solitary. There is no distinct line of demarcation between innocent and malignant tumours. There is no blood dyscrasia. No micro-organism has been discovered.

Let us take these in their order, and consider how far they may tell for or against the hypothesis.

1. It is by no means proved that epithelioma is not contagious. I have myself watched two cases in which malignant disease of the penis in the husband followed the same disease of the uterus in the wife. In one of them I do not know with certainty that the uterine disease was of the epitheliomatous variety, although certainly malignant. In the other both were undoubtedly epitheliomata. The wife was known to have it for one year before her death. The husband showed it to me, already of some size, four months after her death. He lived for two years. Other cases of a similar nature have been repeatedly recorded. It looks probable from the known facts that inoculation requires an abrasion and prolonged contact, conditions which are compatible with analogy.

But admitting for the moment that it is not contagious by inoculation, such fact is no proof that it does not belong to the zymotic class. There are undoubtedly two great divisions of that class. In one the disease enters by the circulatory fluid, in the other by inoculation only. Again, having entered the blood, there are some microzymes which multiply therein and affect the other tissues secondarily, while some are innocuous in the blood, and find their breeding ground only when conveyed by it to a suitable soil. Take, for example, scarlatina or relapsing fever, and tetanus or hydrophobia, as types of the two classes. Undoubtedly the contagious property, in some shape or other, is characteristic, but the proof of this in tubercle is of very recent date, and so far as its capability in the human being of breeding after inoculation is concerned, the arguments are little stronger for tubercle than for cancer. Contagiousness, then, is probable, and the absence of proof is not incompatible with the microbic hypothesis, for the particular form of microzyme may require a precedent sojourn in the blood, or a special preparedness of soil.

2. The variety of form which malignant disease presents is not in argument of much weight, because according to this hypothesis it may be explained either by supposing that there is more than one variety of microzyme, or by citing the analogy of other diseases in which the most curiously varied phenomena are produced by one and the same germ, according to the constitutional state or local condition—according, that is, to the soil or climate in which it finds itself.

3. Nothing can be more certain than that epithelioma may be excited by irritation. One has only to call to mind that it occurs in scars, especially of burns, in old sores and sinuses. My experience of these is that a very long course of irritation is necessary. It is rare to see it so arise except after fourteen or fifteen years of persistent irritation. The same applies to other forms of inflammatory precedent. The soot wart is a matter of years. The paraffin epithelioma takes a long time. A papilloma or mole only becomes malignant after long inflammation. A smoker must keep his lip seriously sore for many years before cancer appears. Nevertheless, the causation by the pipe is undoubted, and my colleague, Mr Bell, tells me that he has only seen three cases of epithelioma of the lip in women, and they were all smokers. There can be no doubt, therefore, that epithelioma may be set up by irritation. I believe that the same may be said of other varieties of malignant disease. It is not so striking, but you see it in the eczema that sometimes precedes scirrhus of the mamma, in the fact that the mamma and uterus are prone to be attacked, in the facts which indicate that sarcomata may originate in a sudden and violent injury. But there is nothing therein that militates against the microbic hypothesis. The explanation is that a suitable soil is thereby prepared. It has its complete analogy in Chauveau's experiments in bistournage. Septic matter, if it be introduced into the blood of sheep, will produce an abscess in the scrotum if bistournage or twisting of the cord be performed, but otherwise, that is if one or other process be omitted, no suppuration will result. I have seen a syphilitic gumma produced at the site of fracture

of the humerus, and ultimately determine amputation. It is an everyday experience that a strumous arthritis may be developed by injury, and Mr Sayre holds that traumatic synovitis invariably precedes morbus coxæ.

4. The hereditary element in malignant disease probably plays a part, though it cannot be regarded as an important one. It is compatible with any hypothesis. The hereditary character of phthisis is unmistakable, although certainly the bacillus is not transmitted; and equally so is the inheritance of syphilis, wherein very possibly it is transmitted. Whether the very *materies morbi*, or the soil suitable for its growth, is the subject of heredity remains, and will long remain in many such diseases, an unsolved problem.

5. The solitary character of the primary growth is a marked feature of malignant disease. Innocent tumours are often primarily and independently multiple—lipomata, papillomata, enchondromata, osteomata—malignant very rarely. You see them crop out in a shower all over the body secondarily, but even in symmetrical organs the presence of a tumour in each may be regarded as evidence against its malignancy. If, then, it may be said, the disease comes through the blood (and the argument is *à priori* plausible), you would certainly anticipate much greater frequency in primary symmetry or multiplicity. But here again analogy is on the other side. Strumous disease may undoubtedly be multiple, but as a rule white swelling is asymmetrical and solitary. So of tertiary syphilis, and so of cancer. I have seen on two occasions scirrhus of both mammæ. I have seen multiple primary sarcomata, but they are comparatively rare. The similarity holds even in this respect. The argument, of course, fails altogether if we assume (what is not improbable) that the disease is generally of local introduction in the way of local inoculation.

6. The absence of a distinct line of demarcation between innocent and malignant tumours is interesting, but militates in no way against the microbic theory. It is common to all disease. There are no hard and fast lines in nature, but they do remain typically and essentially distinct. I suppose that the closest associations lie in the enchondroma with the

sarcoma, in the papilloma with the epithelioma, but there is nothing incongruous with known facts in recognising the origin of malignant disease in irritated texture, or its power to induce such forms of tissue production as the locality in which it exists is capable of developing. A sarcoma in bone provokes osseous formation, in the parotid or testicle may show signs of cartilaginous tissue such as is not uncommon in these regions as an innocent growth.

7. There is no blood dyscrasia before the appearance of the local disease. But in tubercle, and syphilis, and hydrophobia, does not the same hold good? It is not essential that the microzyme should multiply in the blood or produce immediate palpable effects because it has passed through it. When it finds its proper place it multiplies at that place, and the blood becomes poisoned by the products of its growth through secondary absorption.

In short, I know no fact in the history of malignant disease which is incompatible with a microbic origin, and such a hypothesis explains the phenomena more completely than any other theory of its etiology. On more than one occasion recently, the discovery of the microbe concerned has been announced. The probability is that exact proof of etiological connection will be exceedingly difficult to obtain. It must be a germ of wide distribution and considerable abundance. It is one capable of existence in some form outside the human body, as perhaps may be said of all, and it is certainly one which requires for its growth a highly specialised soil. The difficulty of fulfilling the conditions of its existence is so great that we must necessarily be prepared for a long period of doubtfulness. Has this not been the case with the much easier tubercle bacillus and with that comma bacillus whose relation to cholera is not yet absolutely determined. But the probability is very great that by patience and time its existence will yet be determined.

Treatment.—With such views of its etiology, what course is to be taken by the surgeon in dealing with the disease? Undoubtedly our *à priori* reasoning must have an influence on our practice, but as undoubtedly the ultimate decision must be chiefly empirical. The question we ask ourselves

is—May cancer be cured by removal? We answer it, in the first place, by saying that the fact that it has come to its present site through the blood is no more an argument against attempting its removal than against the excision of a strumous knee, while its greater tendency to be solitary is a point in favour of cancer. But we answer, secondarily, that experience is in favour of interference. In each case we have to consider first, Can it be cured? second, Can relief be given either in the form of prolongation of life or freedom from pain by means of operation? I shall cite only facts which have come within my own experience. I shall not confine myself to epithelioma, because what may be predicated in this respect of one form of malignant disease may within certain limits be applied to others.

To have any value the diagnosis in cases of cure must be beyond suspicion. I shall quote, therefore, only examples in which the clinical character and microscopic appearances have been studied by others besides myself. The first case of scirrhus of the mamma in which I removed the breast, occurred in 1867, and the patient, a lady under the care of the late Dr Warburton Begbie, lived for twenty years, and died at the age of 73 of apoplexy. Naturally I took much interest in it at the time. The symptoms were unequivocal, and sections of the tumour were examined by myself and several pathologists, among others by my friend Dr Wyllie. Another patient in whom I removed the mamma in 1870, and in whom the same definite determination of the disease was made, died of hemiplegia after fifteen years.

A very interesting case was that of an Infirmary patient in whom I excised the left mamma and glands in 1882. She returned five years afterwards with scirrhus of the right, the left remaining perfectly sound. That was also with the glands excised. Two years afterwards she returned with a malignant mass high up in the right axilla. To remove it, it was necessary to take away portions of both the axillary artery and vein. I had a letter from her four years afterwards, from which it seemed not unlikely that her lungs had become affected, but in which she told me that the left side remained perfectly well, and that on the right side her only

trouble arose from some swelling and great uselessness of the arm. The case is interesting because it was double, because the local manifestations were eradicated by operation, and as an example of ligature of both artery and vein.

I know of several other patients of my own, who have survived the same operation for more than ten years, but in whom the diagnosis was not, unfortunately, verified by the microscope.

Cases of Epithelioma

I do not think it necessary to detail the large number of cases of epithelioma of the lip and elsewhere, in which, when recurrence took place in the glands or elsewhere, the original site remained perfectly free, and again I restrict myself only to those in whom the diagnosis was confirmed by the microscope. I have notes of only two such cases in which the patients remained perfectly well after ten years: one was an epithelioma of the lip, another of the back of the hand, for which amputation at the wrist was performed. In neither were glands removed.

Sarcoma.

I have had under my frequent personal observation (because a friend of my own) a patient on whom amputation was performed for sarcoma of the lower end of the femur more than ten years ago. The most careful microscopic examination was made. The patient's father had died of sarcoma of the innominate bone.

It is not quite relevant to the matters in hand, but I should like to mention another case of sarcoma of the lower end of the femur in which amputation was performed. He died between two and three years afterwards, and at the sectio there was found a small sarcomatous tumour of the brain. Every other part was absolutely healthy. How very nearly he escaped the clutch of the disease.

I have not aimed at making a long list by instituting inquiries. I have taken simply those whose subsequent

history, from being chiefly private patients, was familiar to me. And I have mentioned only such as have survived ten years. I daresay a shorter time would serve the purpose, but I have seen a case of recurrence after nine years of immunity.

HÆMORRHOIDS

AS in so many similar affections, the cause of piles lies in an alteration of the relationship between the downward pressure and the power to support it. The pressure may be excessive or the tissues weak and lax. The result is stretching of the tissues affected. Now, all pressure, according to its amount and continuousness, leads to inflammation. If moderate and intermittent, hypertrophy results; if moderate but continuous, atrophy; if severe, an inflammation results, which may even be suppuration and destruction. In hæmorrhoids we have all. In the persistent thickened tags of external hæmorrhoids we see the hypertrophy. The atrophic inflammation from tension is well seen in the ulceration which so often covers an internal pile, gives it a strawberry look, and causes hæmorrhage when the bowels act. This ulceration is precisely analogous to that over an aneurism about to burst, or a tumour pressing on the skin. In all, when a certain thinness is reached, the ulcer begins on the outside, with desquamation of the cuticle, and gradually eats inwards. The more acute forms of inflammation are produced by rapid increase of the persistent causes or accidental incidence of external courses of irritation, and give rise to the well-known phenomena of a "fit of piles."

In treatment, then, the etiology must be followed. You take off the pressure. In this respect the effect of rest in the horizontal position has scarcely been sufficiently valued. It is essential in the "fit" and for hæmorrhage. But it is not to be neglected in the initial treatment of all cases. The treatment of the liver, the bowels, and so on, is well understood, but it must not be forgotten that a loose or frequent action of the

bowels is as injurious as constipation. Locally, the free use of cold water after the action of the bowels, and at night, is sufficient, with the other treatment, in the great majority of cases, to obviate the necessity for operation. It is only in old and aggravated conditions that anything else is required.

If operation be required, I do not believe that it is of much importance which you adopt. They seem to me all about equally safe: cautery, ligature, and excision. I use the cautery because it is less painful and less likely to cause retention of urine than ligature, and theoretically less exposed to the rare, though serious, risks of septicæmia than excision. I confess, however, that I have not met with septic disaster after any of the methods of operating. In all cases the dilatation of the sphincter adds greatly to the ease and precision of removal.

Before operating, it is important always to examine the parts thoroughly. I was once asked by a medical man to operate on his brother for hæmorrhoids, as he was losing much blood at each motion, and I was requested to do it next day. The doctor placed his brother under chloroform before I saw him, because he was nervous. When I came to examine, and, as I always do, passed my finger up the rectum, I found malignant disease of the rectum. The situation, as may be imagined, was unpleasant.

I should hesitate to operate unless I were assured of a healthy condition of the urethra. Quite recently I delayed the operation because on inquiry we found the patient had suffered from prostatic inflammation, and I found it difficult to get an instrument into the bladder. As it turned out, there was no retention when we came to operate later, but there might have been at first, with unpleasant results.

PAIN AS A SYMPTOM OF FRACTURE.*

THERE is a want of perspective in our surgical symptomatology. We do not formulate for each sign its precise relative value. The consequence is, that on the one hand the inexperienced fail, among a mass of unknown quantities, to draw definite conclusions; while, on the other hand, experience, gradually teaching us to single out the indications which are important, tends to the formation of diagnosis upon an insufficient basis. Fractures afford an illustration of this deficient perspective. Can there be anything more unmeaning than the common division of the symptoms into equivocal and unequivocal? What advantage is it to know that mobility is unequivocal if sometimes it does not exist at all, and at other times is present, though it cannot be recognised? Or why call pain equivocal when sometimes it is pathognomonic, and may be the only reliable means of diagnosis? All symptoms are equivocal under certain circumstances, unequivocal under others. The fact is, that we have in surgery a number of hereditary technicalities, which are utterly useless and may be misleading; and among the most absurd of them are such terms as true and false, equivocal and unequivocal. The result is, that we see the surgeon endeavouring to elicit crepitus when a few careful measurements might avert from his patient the most excruciating torture, or striving to detect mobility when the aspect of the limb is amply sufficient for his purpose.

My present object is to give precision to our estimate of pain as a symptom of fracture. Its value has, I believe, been generally under-estimated, and it is certainly desirable

* Reprinted from *Edinburgh Medical Journal*, 1879.

that for this, as for all the signs, the value should be carefully determined.

We shall find that pain may be useful to us in two ways. It may clear up cases that otherwise would be exceedingly difficult, and in easy cases may save the patient farther suffering in the process of investigation.

Pain may present itself in connection with fracture under three varieties: 1, as such, pure and simple; 2, as pain on motion; 3, as pain on pressure, or tenderness.

These three varieties are usually concomitant at the site of fracture. But they are so also in many other injuries, and as positive evidence this concomitance is not of much significance. It may happen, however, that its absence is worthy of note. Thus, there can be no fracture if pain be not felt on moving the part, even though the other forms of suffering be very prominent after an injury. Or again, with some fractures, although it is more common with dislocation, pain is referred entirely to a distant part of the limb. In these the absence of pain on pressure and motion proves that the injury, whatever it may be, does not lie at the spot to which sensation is referred. It is not unfrequent to have a dislocation of the humerus supposed to be a sprain of the wrist, and I have seen a fracture of the neck of the humerus missed from the same cause. This error could not have arisen if the point I have referred to had received due attention.

But according to the circumstances under which it occurs, its position, severity, and combinations, each variety of pain is capable of affording valuable information. Let us take them in their order.

Pain alone, and simply as such, is certainly not often of much significance, even though we leave out of account the possible difficulties arising from referred sensation. In the case of direct violence, pain at the injured spot indicates merely the locality of the injury, and the same may usually be affirmed of injuries produced by muscular action. In the one, bruising of the soft parts; in the other, rupture of muscular or fibrous textures, is not thus to be distinguished from fracture.

In examples of injury from indirect force, the reliance to be placed on this symptom varies with the part injured. In the neighbourhood of a joint it may mean strain or dislocation, but the more nearly its site approaches to the line of weakness in the shaft of a long bone, the more probable is it that pain is produced by fracture.

It never, however, becomes pathognomonic. Other symptoms are required for confirmation, are generally more reliable, and sometimes are as easy to ascertain.

The pain produced by motion may be of greater use. It is logically evident that if we can impress motion on an injured but healthy bone by means of force applied at a distance from the part hurt, and without in any way disturbing its surroundings, the pain so elicited must mean fracture and nothing else. Practically this is not always possible. Force may be applied to the injured bone either as a cross strain or by pressing the ends towards each other. By the former method tension is also most probably brought to bear on the surrounding soft parts. By the latter the result may be vitiated by the neighbourhood of a joint, and in the shaft of a bone is usually rendered of little value by the fact that other symptoms are more distinct and as easily ascertained.

Nevertheless there are circumstances in which the presence of pain on motion becomes important. Let me mention one or two.

It is not, perhaps, a matter of much moment whether a blow upon the chest has led to fracture of the ribs or not, and inasmuch as the treatment is similar for severe bruise and for fracture, surgeons are often content to leave the question undecided. An accurate diagnosis, however, may in medico-legal cases be important, and is always satisfactory both to surgeon and patient, if it can be made without injury to the latter. I think it possible to do so by means of the pain elicited by motion, when it would be unjustifiable or difficult to bring out such symptoms as crepitus or increased mobility. Having ascertained that the pain on pressure of the injured part is situated over one or more ribs (if it be not so, there can be no fracture), the surgeon

should trace these ribs to a distance of some inches. When at this distance from the bruising, firm pressure is made, severe pain will be developed at the fracture if there be fracture, little or none if there be not. It is only necessary to be accurate as to the bone pressed upon.

Again, fracture of the neck of the femur often gives rise to difficulty in diagnosis. The depth and other conditions of the fracture make it sometimes far from easy to ascertain the crepitus and abnormal mobility, and we therefore rely largely on the deformity, as ascertained by careful measurements and comparisons. I believe that the most experienced men are occasionally deceived. There exists a well-known class of cases, in which, although the absence of eversion and shortening has been at once accurately determined, it is discovered (perhaps after the patient has been moving about with difficulty for months) that there is shortening often to a considerable extent. The explanation commonly given is that chronic interstitial absorption of the neck of the bone has taken place. I do not think that this has been well made out. I would not deny the existence of this interstitial absorption, but it is not easy to understand what physical injury other than fracture would be sufficient to set it up. I believe that a fracture, in which the surfaces are crushed into one another, is more frequent here than is generally supposed, and that in some a considerable amount of absorptive action ensues. But I know also that shortening sometimes comes otherwise than by absorption. I have on several occasions met with cases, of which the following is a type:—An elderly man was admitted to the Infirmary last November, having been knocked down by a horse at Hallow Fair. He had a fracture of the lower end of the right radius, and complained of severe pain in the left hip. There was no shortening, no eversion, no crepitus, but, from the character of the pain, I had a strong suspicion of fracture of the neck of the femur, ordered him to be kept in bed, and had his leg measured once a week. The third measurement showed a shortening of one inch, which had, therefore, occurred between a fortnight and three weeks after the accident. I have notes of two cases in private practice, in which shortening similarly took place in the third and in

the fourth week respectively. The only explanation plainly is, that there was fracture which had yielded to muscular action when softening of the impacted parts had sufficiently advanced. Now, the true way to save the surgeon's credit is to make a correct diagnosis at the first—not to hedge by stating that there is only a bruise, but that such bruises are prone to produce shortening. This diagnosis may be made in two ways. One method consists in giving chloroform, and so moving the leg as to elicit crepitus, when you will also probably find that you have succeeded in producing shortening and eversion. The other, and, as I think, the better mode is, having weighed well all the circumstances, to confirm the diagnosis by the character of the pain produced by movement. I have not yet satisfied my mind by a sufficient number of cases, but I am nearly certain that, if the pain caused by inversion be considerable, a fracture has to be dealt with. Pain on eversion and flexion are also greater with fracture than with bruise, but the distinction does not appear to be so marked. No doubt, in a case of chronic arthritis, a bruise cannot thus always be distinguished, but the history and concomitants must then be called to our aid.

Many other fractures have special characteristics in reference to the motions which produce pain. I have not worked out the details, because they do not appear of such practical importance as to require individual mention.

As a symptom of fracture, then, pain on motion is generally of value only in so far as it confirms others. Sometimes it rises to a more prominent position. Often its absence gives assurance of the absence of fracture, for if it be possible to impress motion on a fractured bone it is certain that pain will be produced.

Tenderness or pain on pressure over the injured part is invariably present in all cases of fracture. It may be definitely affirmed that this tenderness is experienced round the whole circumference of the bone in the line of fracture. The question is, How can this law be practically applied? Its usefulness varies according as the fracture has been produced by direct or indirect violence, and I think it will be found that

as a negative sign it is decisive, if not always clinically valuable, and that as a positive indication of fracture it is also of great importance.

In direct fracture from the depth and situation of the bone, it may not always be possible to apply the test with precision; and even when the bone is partly superficial, that is precisely the portion of its contour most likely to be covered by tissues which the blow has also bruised. But it may be that the whole bone can be felt, or that the opposite side from that struck is in a position which can be reached. If it be so, the presence of pain on pressure indicates fracture, and its absence proves that there is none.

But in fractures produced otherwise than by direct violence, pain on pressure attains its highest value. The same circumstances as in direct fracture, no doubt, impede more or less the precision with which the fact can be ascertained. The bone may be so deep or so situated that it cannot be directly pressed upon in the whole or some part of its circumference, and injury of the soft parts in the neighbourhood may under certain circumstances be produced by indirect violence. But these impediments are not so serious as in the former case.

The first impediment does not interfere with the negative value of the sign in injury from indirect any more than from direct violence. If any part of the contour of a bone be subjected to pressure without pain, it is certainly not broken on that particular level. And this is of practical value, especially in the vicinity of some of the joints. The elbow, for example, has been violently wrenched. By putting the arm in the easiest position, and then running the finger along the exposed edge of the ulna, pressing together the condyles of the humerus and their ridges, and feeling the head of the radius, you may distinctly determine that there is no fracture without further examination. And, on the other hand, the depth or situation of the bone does not necessarily impede the discovery of fracture by this sign. It is not essential that the whole circumference of the bone should be exposed to pressure. A very small portion will suffice, if it be immediately under the finger, if the painful spot be in the

usual line of fracture, and if the cause of the injury be undoubtedly indirect. Take a common example of fracture from indirect violence, Pott's fracture of the fibula. Even though there be no displacement, the diagnosis of fracture may be made with certainty, if there be pain on pressure above the external malleolus, and especially if this be accompanied by tenderness over the internal lateral ligament of the ankle-joint. It is only, therefore, when the bone is altogether so situated that the pressure applied loses in precision, that this circumstance interferes with the usefulness of the symptom.

The other impediment—that, namely, of injury to the soft parts from indirect violence—is not, in most cases, one of much moment. In the neighbourhood of joints it must be borne in mind. In the shoulder-joint there are fibrous expansions of the tendons, extensions of the joints, capsule, and bursæ, over the bone, which are prone to be injured and become sore to the touch. There is also naturally a tender spot over the anterior tuberosity, which becomes much more painful in disease or after injury. In this region, therefore, no reliance, or very little, can be placed upon this sign. In the knee, also, the curiously tender spot over the lower part of the inner condyle should not be allowed to lead to confusion.

But with a few well-defined and easily-ascertained exceptions of this sort, the proposition may be maintained, that if the violence be indirect, pain on pressure over a bone indicates fracture, its absence negatives the possibility of the bone being broken. That does not mean, of course, that this sign is always to be placed in the front rank. What I have called the perspective of symptoms must be maintained, and it varies for each case. Sometimes, as in fractures of the shaft of the humerus or femur, mobility stands first, crepitus second, and tenderness only third. In other cases deformity is of greater importance. But not rarely pain may be really the leading and even a pathognomonic symptom.

Let us consider for a moment the diagnosis of the two most common fractures of the body, that of the lower end

of the radius and of the middle of the clavicle. They are both due invariably, or almost invariably, to indirect violence. If a person fall upon the palm of the hand he may sustain one of three injuries from the bending backwards—a dislocation of the wrist, a sprain of the joint, and a fracture of the lower end of the radius. The first is so very rare as hardly to be worthy of discussion in the present connection. I have only once seen it; and then, as I was manipulating to define the relations of the styloid processes, the joint slipped into position. I cannot say, therefore, from experience, what the exact situation of tenderness is. Doubtless it is over the joint itself; but doubtless, also, the diagnosis depends on the deformity.

As to the means, however, of making a comparative diagnosis between the other two, I am absolutely certain. The characteristic deformity of Colles' fracture is marked in at least nine cases out of ten, and there is required for diagnosis nothing more than careful inspection. But in the tenth case the deformity is so slight as not to be easily made out, or is masked by the general swelling. It is in the distinction of these more obscure examples from sprain that tenderness plays its *rôle*. If there be a fracture, the pain on pressure is most severe above the level of the styloid process of the radius, both before and behind; if there be a sprain, the pain is in the region of the joint. It is impossible to have anything more definite and precise.

In fracture of the clavicle, pain on pressure is also pathognomonic. You have only to run your finger along the line of the bone. No doubt, in all cases for purposes of treatment you ascertain whether deformity is present. But that is not necessary for diagnosis, and its absence is not a source of embarrassment. Tenderness here is the important sign; swelling and deformity come next, and no other sign—neither mobility nor crepitus—need or ought to be sought for.

I have not attempted to deal with my subject in an exhaustive manner by going over all the possible fractures in the body. I have endeavoured to show that in each case

a special order of importance ought to be given to the various signs, and that in not a few, pain in some of its aspects is of great importance sometimes as a pathognomonic, sometimes as an essential, and sometimes as a confirmatory symptom.

CONCERNING THE CLOSURE OF ABNORMAL ANUS.*

THE treatment of artificial anus has at various times attracted considerable attention. Till a comparatively recent date its pathology was imperfectly understood; but the recognition of the septum as the main obstacle to closure, and the methods for overcoming that obstacle adopted by Schmalkalden, Physick, and Dupuytren, greatly elucidated the subject, and marked a very decided advance in principle and practice. Still, however, in a certain number of cases the destruction of the septum is not followed by obliteration of the abnormal orifice, and in these circumstances surgeons resort to plastic operations, of which the main characteristics are a bewildering variety and a great absence of success. I venture to add to the list yet another method, because I think it presents fewer chances of failure than any hitherto devised. It rarely falls to the lot of a surgeon to consider the treatment of a case to which such an operation may be applicable. I make no apology, therefore, for illustrating my proposal by a solitary example. It possesses at least the merit of completeness, and assuredly exhibits no undue haste on my part to anticipate the efforts of nature.

The patient was a domestic servant, æt. 45, whom I was asked to see by Dr Inglis in April, 1869. She had been ill for a week before sending for Dr Inglis, and had supposed her illness to be a bilious attack, unconnected with the swelling in her left groin, as this had often previously come and gone without causing discomfort. I found it to be a femoral hernia already red, swollen, and emphysematous.

* Reprinted from *Lancet*, vol. ii., 1873, by permission of the Proprietors.

An incision exposed a loop of the intestine gangrenous in its entire circumference, and rent on its convex surface. I divided the stricture and left the bowel *in situ*. The patient slowly recovered with an abnormal anus capable of admitting two fingers, and through which alone the fæces were discharged.

She was advised to await a possible natural contraction of the orifice, and I did not again see her till June, 1870. In the interval she had worn a truss, which, however, retained the fæces very imperfectly, and during the whole time there had been no passage by the rectum. The artificial anus had not diminished in size, and the septum was distinct and prominent. The orifice of the lower portion of the bowel had contracted somewhat, but was large enough to admit the little finger.

Instead of using the enterotome, I passed a double silver wire through the septum, about an inch and a half above its free margin. It was left loose for two days, and then one of the wires was gradually tightened, until on the eighth day it ulcerated its way out. The other wire had meanwhile, as I anticipated, become slightly imbedded by reunion of the deeper parts, and it now fulfilled the purpose for which it had been left, in being drawn out through the recent adhesions. The same day fæces passed by the rectum, and after a week of considerable irritation, solid motions were regularly and naturally established. The patient was sent to the country, with instructions to remove the truss only for purposes of cleanliness.

I again saw her in December, 1871, and was disappointed to find that still no contraction of the orifice had taken place, although (the truss being regularly worn) all the fæces passed by the natural route. I repeated the former operation, including in the wire nearly an inch more of the contiguous walls of intestine. The condition of affairs was not thereby improved, and it was plain that the septum was no longer an impediment to closure. In April, 1872, I therefore performed the following operation:—

Having thoroughly cleared out the bowel by purgative and enema, I dissected up the mucous membrane all round

the abnormal orifice for more than half an inch, invaginated it, and sewed the raw surfaces together by six points of interrupted catgut suture, which were then cut short. I next pared freely the margin of the skin, and brought it together by means of silver wire.

During the after-treatment the parts were relaxed by keeping the thigh flexed. The diet was restricted to milk with lime-water. On the eighth day, finding the wound entirely united, I removed the stitches. A little froth issued from the small openings left by the removal of a corner stitch. These openings remained patent for some weeks, notwithstanding the use of the hot wire. Nothing but gas, however, escaped from them, and they ultimately soundly healed.

The patient remains in every respect perfectly well, and it is now a year since the closure was effected.

Such an operation is, of course, only possible when the artificial anus is of large size; but it is precisely in these cases that other methods are least successful. The advantages which I claim for it are, that it presents to the fæces the normal mucous surface of the bowel, that it diminishes the strain upon the stitches, and that it largely increases the depth of raw surface, by the adhesion of which the pressure from within is to be resisted. It seems to me theoretically sound, and in this case, at least, was practically successful.

ON LINEAR INCISION AND EVERTING SUTURES IN PLASTIC AND OTHER OPERATIONS.*

TWENTY years ago I took occasion to point out that the difficulties which attend the closure of abnormal openings between surfaces might in some cases be overcome by an operation of splitting rather than paring. I showed, in a case of artificial anus, that the method of stripping and turning the mucous membrane towards the lumen of the bowel presented certain obvious advantages. In that way, plainly, great additional thickness could be given to the textures destined to unite. There was no sacrifice of tissue. The operation opposed to the faces the mucous membrane which normally was in contact with them. To the air the skin was presented, and to raw surface raw surface was adjusted.

It was evident that, if stitches were applied through the whole thickness of the wound, they would to a certain extent tend to diminish that thickness, and, what was of more importance, would be apt to close inaccurately the mucous membrane, or even to turn it inwards, and so allow septic material from the bowel to find its way to the raw surface. To obviate this the special suture was applied to the mucous membrane, which goes by the name of the Lembert suture, and these sutures being made of catgut were entirely buried.

Curiously enough, a few months after I saw this case, Dr Andrew Inglis, whose patient she had been, brought me another almost identical. This second woman had not sent for him until her strangulated femoral hernia had become a

* Reprinted from *Edinburgh Hospital Reports*, vol. i., 1893.

red tumour containing pus and gangrenous bowel. He thought it scarcely possible she could live, and simply incised the tumour. Nevertheless she recovered with a preternatural anus, and I ultimately operated with even greater success than in the first case, because the wound healed throughout by first intention. Since that time I have not had another opportunity of applying the method to artificial anus.

I see from a recent paper that some surgeons still resort to flap methods in these cases, in ignorance, no doubt, of the fact that they never succeed. I suppose that many with greater reason would now adopt the plan of separating the bowel altogether from the abdominal wall, sewing it up and dropping it into the peritoneal cavity. I should myself still prefer my own method as being easier, safer, and not more likely to give rise to subsequent trouble. I have at least so taught up till now, and have at the same time insisted that the principles involved are necessarily correct, and in so far as applicable should be adopted not only in artificial anus, but also in other conditions requiring plastic surgery. I have frequently carried them into practice, and now wish to record not the tedious details of many cases, but a few illustrative examples, fitted to define the circumstances under which these principles may be applied.

Recent wounds.—Fresh wounds may be so situated as to involve two surfaces, and some of these may be advantageously treated by modifications of the method. They are often so treated by every one in daily practice, for, like M. Jourdain, we often talk prose without knowing it. Wounds involving serous membranes do not require it. These tissues are prone to adhesive action, and there are good reasons for including the whole thickness of the wound in the sutures. But when mucous membrane and skin are involved it is a matter of moment that inversion of either towards the raw surface be thoroughly obviated, and, especially if the distance between the surfaces be small, that the depth be increased by suturing the membranous tissues separately and everted. Thus in a case of cut throat, in which the air-passage was almost completely severed at the crico-thyroid membrane, I successfully sutured the mucous membrane by buried sutures, and avoided

the risks of œdema glottidis by tracheotomy. To the same class may be referred suture after suprapubic lithotomy. All are agreed that from various causes it is exceeding difficult in these cases to ensure union by first intention, and that if failure ensue, the patient is worse off than if the wound had been left entirely open. I have not ventured to sew up when the urine was septic, but in three cases in which the urine was aseptic I have stitched the bladder wound closely with Lembert sutures, and separately the rest of the tissues, draining the bladder and placing a small drainage-tube in the external wound. One case, a boy of 10, was entirely successful; one, a man of 32, at the end of a week, passed a few drops of urine for two or three days from the wound; and the other had to be externally drained for a fortnight. These are simply examples of the same principles being applied to ordinary wounds which I extend to other circumstances.

Fistulæ.—Wounds may heal untowardly, and the abnormal union give rise to gaps or fistulæ. Most of them are best treated by a linear incision and the separate suture of the surfaces.

A typical example is the division of the lobe of the ear produced by avulsion of the ear-ring. I have operated on such on several occasions, once upon both sides in the same person.

The following is another instance in point:—Some half-dozen years ago, by a regrettable lapse of time, the Royal Infirmary was deprived of the services of my predecessor in the surgeoncy. At that time I entered upon the duties which had previously been discharged by Mr Bell, and shortly afterwards there came to the Infirmary a patient who had been under his care for cut throat and sequential fistula. Efforts had been made to close the fistula, but as they were not successful, he had been sent home to recover strength, with a view to further procedure. He had so prolonged this period that he fell to my care. The fistula was situated immediately above the hyoid bone, and was three-quarters of an inch in length externally, narrowing to less than half-an-inch internally, where it opened between tongue and epiglottis at

and to the right of the middle line. Evidently it was kept open, partly by the movements of the part, but chiefly by the entrance into the fistula of saliva, mucus, and particles of food. I made a simple incision all round, loosened freely, and turned towards the mouth the mucous membrane, and sewed it with buried sutures of catgut, while the external parts were retained in position by deep silver and superficial horse-hair stitches. It healed absolutely by first intention.

Let me cite only one other example of a similar kind—split perinæum. My first case was that of a lady, the wife of a tea-planter, to whom the accident happened in 1879, during delivery in Assam. She saw Dr Charles of Calcutta, who advised operation, but as she was on her way home, it was delayed until she came under my care in April 1880. I found the rectum largely involved, and she had no control over the fæces. By linear incision the rectal mucous membrane was separated from the vaginal, the vaginal from the skin. Buried Lembert sutures were inserted into the everted mucous membranes, deep stitches of wire drew together the large triangle of raw surface that was left, and the skin was also sewn up superficially. The whole healed without a drop of pus, and she regained control of the fæces. On inquiry, I find that she is again in India and perfectly well, but has had no more children. I have repeated the operation with success in four other cases. They are all I have seen, as perinæal rupture does not come under the care of the general surgeon; but I think I may say that the operation is the one which is best adapted to this form of traumatism.

The plan is more difficult of application in fistulæ which have been produced by sloughing. The induration, adhesion, and destruction of parts in such circumstances frequently demand more extensive plastic operations. But when the conditions permit, it is right thus to broaden the raw surface.

Fistula in perinæo is not a condition that *prima facie* looks favourable for any form of plastic operation. I have, nevertheless, applied the method of linear incision in three cases. I feel bound to record them all, because one was disastrous in result and one only a partial success. The first, in 1876, was

an Infirmary patient who had a fistula half-an-inch long, just behind the glans penis, on the under surface. It had been produced by phagedenic chancre. I operated in the way I have described. Next day the patient had a rigor, and on the fifth day he died of acute septicæmia. From so small a wound I have never had so terrible a result. The evil lay, not in the nature of the operation, but in my then imperfect appreciation of aseptic surgery.

I am indebted to Mr Chiene for a suggestion which I believe he carried out on these lines in a case of recto-urethral fistula, although I do not know with what success. I afterwards followed his suggestion in a patient in whom a fistula between the rectum and urethra had been produced by abscess. He was 40 years of age when, two years before admission, the abscess had opened first into the urethra and subsequently into the rectum. It was difficult to determine the etiology of the abscess, but I suspect it to have been gonorrhœal. The opening lay at the apex of the prostate, and there was no trace of stricture. I split up the tissues between the rectum and urethra, everted the mucous membranes towards these canals, and sewed them up. I found the suture of the urethra difficult, and not being satisfied of its perfection, I left the outer wound open, with a small drainage-tube in it. The rectum healed perfectly, but, as I had feared, the urethral wound leaked, and a perinæal fistula remained. It was, of course, much less uncomfortable than his previous condition, and he refused further interference.

The third case was one of a little boy, æt. 4, who was brought to the Infirmary with a urinary fistula just in front of the scrotum. On exploration a calculus was found lying in a cavity between the skin and urethra, and was removed. As the fistula remained, it was subsequently operated upon, and healed at once.

I have successfully tried this method also in vesico-vaginal fistula, but have found it difficult of application. It is not easy, after splitting, so to manipulate the mucous membrane of the bladder as thoroughly to evert it towards the viscus, but once it is accomplished, the depth of the wound is very satisfactory, and the results are good.

A small orifice in the cheek from cancrum oris was also successfully cured, but these cases generally require a more extensive operation.

In conclusion, let me draw attention to those cases of congenital defect in which this operation may be properly applied, such as harelip, cleft of the soft palate, eyelid, and such like. In harelip certain precautions require to be taken. Thus the incision must be made exactly along the junction of red and white, the mucous membrane and skin. Next, to allow turning of the mucous membrane towards the mouth, it will be found useful to make a transverse incision in it low down on each side. This done, the antero-posterior depth of the lip will be found very satisfactory, and the portion below the transverse section may be converted into a more or less effective papilla.

I have applied these principles in many other cases, but the examples I have given will suffice to show that they have not inconsiderable application. The advantage of the method is that you sacrifice nothing which can by any possibility be useful, that you attain the greatest possible breadth of raw surface, and that you oppose to the natural secretions and to external influences precisely those parts which are normally in contact with them, and which are therefore best fitted to withstand their irritant action.

AN IMPROVEMENT IN STAPHYLO- RAPHY.*

THERE can be no doubt that the proportion of failures in staphyloraphy is considerable. Partial closure is generally accomplished, but very often a great deal remains to be done by second or third operations, and some surgeons have consequently recommended that the union should be brought about only little by little.

Putting aside the difficulties of manipulation, which are prone to result in want of accuracy and bruising, but which care and experience suffice to overcome, the chief cause of non-union is certainly tension. In proportion to the ease with which the flaps can be brought into apposition will be the probability of success. Efforts have been made to obviate tension by long lateral incisions, by the free use of the raspatory, by division of the tensor palati and other muscles, and by osteoplasty. None of these, however, are absolutely satisfactory, because, as I believe, an important anatomical fact has been overlooked.

Those who have had experience of the operation when hard and soft palate are both involved, will agree with me that the failure to unite is most apt to occur precisely at the spot where the hard palate joins the soft. Frequently a little hole remains, while all the rest behind and before it become firmly bound together, and always, or nearly always, that is the spot where separation of the edges first manifests itself. I venture to think that the reason for this is the fact that the soft palate has two layers of mucous membrane, one of which passes forwards under the bone to form

* Reprinted from *Lancet*, vol. ii., 1879, by permission of the Proprietors.

the roof of the mouth, while the other passes over the bone to form the floor of the nose. Now, while every care is taken by means of the raspatory to separate the periosteum from the bone in the mouth, no attempt whatever is made to loosen the soft parts where they pass into the nose. The consequence is not only tension sufficient frequently to destroy union, but also such a tendency to involution that very often, not raw surfaces, but surfaces of mucous membrane, are applied to each other. I have endeavoured to correct this error in the following case, in which the result has been perfectly successful.

C. M——, a boy, æt. 10, had been operated on by me for harelip when he was six months old. At that time a fissure cleft the palate from front to back; now the anterior third of the hard palate has firmly closed. I operated for the cleft palate on August 1, 1879, by the following method:—Having pared the edges of the cleft, I made parallel incisions along the alveolar borders, carrying them well back and through them separating the soft parts from the bone. The raspatory was made to pass backwards as far as it would go. It was then withdrawn, and the bistoury, which had been used in paring the edges (a long-handled tenotomy knife), was inserted in its place, and made to divide transversely along the posterior margin of the bone the mucous membrane covering the upper surface of the soft palate. In doing so the point of the knife was guided by the forefinger of the other hand, and passed through the cleft. This incision doubtless cuts, as by Mr Pollock's method, the whole or great part of the tensor palati, and in doing so materially aids our object; but the point aimed at as essential was that the mucous membrane should be completely divided towards the cleft, so that on the reintroduction of the raspatory any fibres of the soft textures still adhering were easily torn away, and the whole palate from front to back could be pressed downwards.

I was much pleased with the freedom gained. Muscular action, indeed, could still be observed, but I do not think it can be abolished by any form of incision, and I believe it may be sufficiently controlled by a little improvement in the

method of sewing up which I now put in practice. Besides the usual stitches of silk, horsehair, or silver wire—I have found little advantage in one over the other—I put in a silver suture of relaxation as far outwards as possible at the point of greatest tension, and fix it by means of two small buttons.

A single case, of course, proves only that the operation is practicable, but the principle seems to me sound, and the relief to the parts was most striking. In some cases, doubtless, the incision might be more easily made by a curved bistoury.

ON LAPAROTOMY IN TUBERCULOUS PERITONITIS.*

THE extension of laparotomy to the treatment of tuberculous peritonitis has of late excited interest, and in many hands has been attended by success. My own experience extends to 21 cases, seen chiefly in Dr Affleck's wards, but also in other wards and in private practice. Of these, one died two years after the first operation, the abdomen having been reopened several times to relieve tension. In three, the abdomen, when last seen, was still somewhat swollen and resistant; one patient, æt. 28, returned on account of localised abscess in the neighbourhood of the wound; and one, æt. 5, with a fæcal fistula; while another, æt. 6, has recently come to the Infirmary to be treated for tuberculous elbow, with the abdomen perfectly well. The others recovered to all appearance completely, and have remained well for varying periods up to four years. As is the case with hospital patients, many in the end have been lost sight of. As a contribution to the statistics of the operation, these cases are of little value, but I have found them useful individually, and collectively they indicate that my experience of the operation has been favourable. I draw upon this experience in the following remarks.

This mode of treatment is perhaps not the less interesting because its discovery was purely fortuitous, the result of errors in diagnosis and of the performance of exploratory incisions. My own attention, indeed, was attracted by having to perform laparotomy in 2 cases for acute obstruction, in which it was found that the obstruction was due to tuberculous peritonitis of old standing. One died, but the other was

* Reprinted from *Edinburgh Hospital Reports*, vol. iv., 1896.

cured, not only of the obstruction, but also of the peritonitis. Even now the operation is so entirely empirical, that we are unable to explain the how and why of its success. No doubt it has analogies, but none of them are so complete as manifestly to supply the explanation.

The melon-seed teno-synovitis of the wrist may be hopefully treated by incision and evacuation. But thorough evacuation, and as much rest as may be consistent with the preservation of mobility in the tendons, are essential conditions which cannot apply in tuberculous peritonitis.

Simple arthrotomy for gelatinous degeneration of the synovial membrane is in a certain small class of cases followed by cure. But here, again, success is conditioned by that enforcement of rest which of itself, in the early, if not in every stage of tuberculous arthritis, is so beneficial, that one is justified in looking upon operative interference as an adjuvant or a resource after failure.

In some operations for tuberculosis, the object is to diminish or exterminate the invading battalions of bacilli which are proving too strong for the textures, and very often it is directed to counteracting what may be called the accidents of the position, to relieving tension, to exterminating sepsis, to hastening ankylosis. But it would be difficult to regard any of these as adequate to explaining the effect of incision in many cases of tuberculous peritonitis.

Other suggestions have accordingly been brought forward. It may be said that the benefit must be derived either from something put into the abdomen or from taking something out. Now, the things you put in are light, air, and your finger. It is difficult to see how the short-lived admission of light can have any effect whatever. It is conceivable that perhaps the breaking down of adhesion or the stimulus of handling may prove useful. But if the introduction of anything into the abdomen is effective, it is probably the air. It is at least suggested by the division of organisms into the aerobic and the anaërobic, and is supported by a few observations on the injection of warm air into the abdomen in the lower animals. It is difficult, however, to get over the obvious objections that practically in other parts air has no

such effect on tubercular lesions, and that in many of these operations its action must have been of exceedingly short duration.

On the other hand, the possibility must not be overlooked that evacuation of fluid and other abnormal contents of the abdomen may be of benefit, not only by relieving tension, but also because they contain noxious products which maintain irritation.

I do not see, in short, how we are to escape the conclusion that more than one cause is required to account for the advantages of laparotomy, and that the most important are probably the relief of tension, the removal of irritating fluid, the introduction of air, and the mechanical interference.

After all, however, laparotomy in tuberculous peritonitis is empirical, and only experiment and experience can as yet be trusted to determine its value, the best mode of doing it, the cases in which it is to be employed, and those in which it is inadmissible.

In any endeavour to assess the value and the limitations of laparotomy, regard must of course be had to the nature of the disease in which it is employed. This is a manifestation of tuberculosis which presents some peculiarities due to local conditions, but which on the whole follows pretty closely the line of tubercle in other parts.

Here, as elsewhere, the soil is of more importance than the seed in determining incidence, course, and result. Incidence, because whether, if tubercle be introduced, it will flourish or die out, largely depends on the condition of the body, diathetic, developmental or pathological, and on the condition of the part attacked, anatomically, physiologically, and pathologically. A most interesting deduction from life-insurance statistics is that weight of body is almost equal to family record as indicating predisposition to phthisis.

Course and result in like manner are influenced. The granuloma provoked by tubercle, according to the same conditions, may develop fibrous tissue and cicatrise, or, on the other hand, may undergo various degenerations, cheesy, calcareous, or purulent. And conversely, the predominance of one or other of these changes may indicate the more or

less favourable state of the patient, and, while no hard and fast line can be drawn, must influence the prognosis for the better if the product be fibrous, for the worse if purulent.

Now, we find in tuberculous peritonitis all these varieties, and, as is the case elsewhere, certain definite types of the disease may be differentiated.

1. There is, first, a set of cases in which the predominant feature is the miliary character of the tubercle. It appears to be most common in the young. The disease as a whole is most prevalent in early life, but it is common enough at all ages below thirty. I have not met with a case, however, after puberty in which the type was essentially of this kind.

The prevalent grey-pink little tubercles vary much in distribution. In one case both the parietal and visceral layers of peritoneum may be thickly studded; in another they are few in number and cover a limited area, which is usually a portion of the bowel. At such a spot the bowel is markedly reddened, and the coils are often matted with soft adhesions. The parietal peritoneum in several of my cases, as indeed in other varieties also, was much thickened, resembling closely the gelatinous degeneration of tuberculous arthritis; and when there was not a large quantity of fluid, the bowel surface, having undergone a similar change, was apt to be in places closely adherent to it. The quantity of ascitic fluid is very variable, usually clear and of a pale yellowish hue, though sometimes slightly turbid. It was never entirely absent. Miliary tuberculosis of the peritoneum may, of course, be merely a part of a general tuberculosis, but it is frequently the solitary manifestation. The most usual concomitant is pleuritis.

2. The next typical variety is the fibrous. In it firm and often exceedingly dense adhesions mat the bowels together. In one young man I found the bowels so agglutinated that they formed a dense, tolerably smooth mass, movable as a whole, and separated from the anterior wall of the abdomen by a layer of fluid at least 2 in. in depth. This fluid is by no means always present, and in one case, a child *æt.* 11, there was not obtained more than a tablespoonful, and there were numerous adhesions of the bowels to the

wall, as well as to each other. As may readily be understood, these adhesions may lead to obstruction of the bowels. As already mentioned, I have twice had to operate in such a condition, once with complete success. The other, a child *æt.* 8, I saw with the late Dr Bruce. I found the fibrous matting so dense and firm, that, after a most patient attempt to separate the coils, I was obliged to abandon the operation, and the child ultimately died unrelieved. Such results are, however, unusual, and I think it may be said that the fibrous change is probably the most frequent of nature's modes of cure.

3. Another form of the disease is that in which the new granulation tissue or lymph material becomes exuberant. On palpating the abdomen, irregular indurations of various size may often be felt, and a large quantity of fluid recognised. On incision the fluid escapes, and large masses of lymph are found loose or adherent. In one case, a young man, *æt.* 29, on completing the opening, multitudes of loose bodies, of the size and appearance of over-ripe white grapes, flowed with the fluid from the abdomen; while, after they had been evacuated, further bunches of them in large numbers could be easily separated from the wall and intestines. In another, a little girl, one of Dr Affleck's patients, although there were none absolutely loose, thick patches of soft, smooth, nodulated lymph, from 1 to 3 in. in extent, were scattered over the abdomen. Many of them with a little care could be peeled off.

4. A certain proportion of cases exhibit a tendency to suppuration. This occurs usually in pockets among the intestines, the purulence of caseated lymph, and of course may give rise to all the serious accidents connected with abdominal suppuration. The caseation and suppuration of mesenteric glands is perhaps less frequently associated with tuberculous peritonitis than the nature of the disease would lead one to expect.

It is evident that, while these may be taken as types and separated for purposes of description, they must yet present a great tendency to run into one another, and that even in the same case there may be present at the same

time the stages or variations of miliary tubercle, lymph exuberance, caseation, suppuration, and fibrous change. There can be no doubt, however, that from the general and local conditions a diagnosis as to the prevalence of one or other may be made with fair accuracy, and it is certainly useful for purposes of treatment.

It will be evident from what has been said, that in endeavouring to determine the rôle of laparotomy in tuberculous peritonitis, we should bear in mind the fact that the disease is not incurable. The high mortality of tuberculous meningitis would justify almost any operation which *à priori*, or empirically, holds out a prospect of success, however small. Here, however, a certain number recover. The number must remain indefinite, and statistics must continue unreliable, not only because of the bias to publication of curious and successful cases, but also because the prognosis of the disease varies with its developments. We can only make such statements as these—that a certain number recover; that the number varies with the type, being largest with the fibrous, smallest with the purulent; that such symptoms as a high and variable temperature are unfavourable, a level and low one the reverse.

Yet, again, we must appreciate the fact that the seriousness of tuberculous disease varies with its multiplicity and with the character of its diffusion. In no disease is the extremely specific relation of seed to soil as a factor in development so well marked as in tubercle. There is a liability not only to acquisition of the disease, but to its development in special textures. It is generally recognised that some forms of tubercle are prone to occur in young people, others in more advanced life. It is a fact that at all ages some people are prone to glandular, others to pulmonary, others to arthritic tubercle. It may be assumed as a general law that tubercle has a marked tendency to spread by the tissue in which it originated. Tuberculous disease of a joint or subcutaneous tubercle does not cause lupus, even though there be a breach of surface. But these various limitations may be transgressed, and when they are transgressed the case assumes additional gravity, and I suppose that nothing

is more difficult in the surgical treatment of tubercle than to assess the value of concomitant lesions. Practically I have thought myself bound in such cases by much the same considerations as would influence me in operating, say, for tuberculous fistula in ano. What is the lesion which is producing most damage to the patient? Are the concomitants of such a kind that no hope of prolonged life or increased comfort can be entertained as a consequence of operating on the abdomen? These and suchlike questions by their answers determine action.

I am not, of course, concerned with the medical treatment of the disease, further than to suggest that it is not without hope, and that the surgical experience of the advantages of immobilisation is worthy of consideration. But, after the failure of medical remedies, there is no doubt in my mind that the operation of laparotomy is one to which resort should be had in a large proportion of cases. Practically there is no immediate risk. We had not a single case which caused the least anxiety.

There is, however, some risk of the formation of fistula. Fresh granulation tissue is a soil in which tubercle revels. I think we have more cogent proof of this in these antiseptic days than was formerly the case. For example, in excisions of joints for tuberculous disease, if performed when the skin is unbroken, one can rely with great certainty on healing by the first intention. But every now and again, after three, four, or five weeks, the scar begins to broaden, and get a little œdematous. Presently, at one or more points, it yellows and then ulcerates, and you may as well use the sharp spoon freely and at once, because tubercle has attacked the scar tissue. It is plain that this may occur after laparotomy. I am astonished that it does not do so oftener, because in tuberculous peritonitis no attempt is made to take away the tubercle, whereas in excisions every care is used to remove all palpable disease. Still, it does occur. I have already referred to the two cases in which it has occurred after my own operations. In both cases the history was very similar. Both had left the Infirmary apparently quite well. In both, some eight weeks after the operation,

a swelling began to be felt in the abdomen. This progressed until it was allowed to open spontaneously, as the patient did not return to the Infirmary. Here the history diverges. In the young man a simple sinus has formed, and there can be felt adherent to the anterior wall of the abdomen a tolerably hard mass, which is slowly getting less. His general health remains good, and the rest of his abdomen is soft and painless. In the child the opening of the abscess was speedily followed by the escape of fæces, and a fæcal fistula has existed now for six months. There is a striking absence of all pain, and the abdomen is soft and not enlarged. On this account I have not interfered, and because I believe the fistula will close spontaneously, and the necessary operation would not be without risk. At all events, meantime the wound is drawing in, getting fibrous—a condition of very favourable omen in all tuberculous sores—while the fæcal escape is very small and undoubtedly diminishing.

We must be prepared, therefore, for this accident. Otherwise there is nothing in the risks of the operation to forbid its extensive use.

I have myself been inclined to limit the operation to cases in which the peritoneum was alone attacked. That, however, is justifiable only because the procedure is still tentative. The considerations which ultimately will influence the surgeon I have already referred to.

I have not confined it to any type of the disease. I have come to look upon the cases with fluid as favourable for the operation, but I am not sure that this is not a prejudice, because in two cases, in which there was absolutely not twenty drops of liquid, the recovery was as speedy and satisfactory as in the others.

I do not think that the last words have been said as to the essential features of the operation. The only difficulty in its performance arises from the possibility that the incision may lie over a point where the bowel is adherent to the wall. As the peritoneum may be much thickened and gelatinous, care has to be exercised in its division. The fluid having been evacuated, any easily detached lymph masses should also be removed. When these are extensive, the prognosis

is unfavourable. I have already referred to one example in which the operation had to be repeated several times on account of the distension, and in which, although no doubt life was prolonged, there was never the slightest tendency to recovery.

I have been in the habit, when there are adhesions of the intestines to each other, to separate them gently, if they are soft and yield easily. Pockets of serous fluid are thus reached, and it is not unlikely that the fresh adhesions which must form may tend to become fibrous and have a curative influence. It does not seem that any benefit is derived from the introduction of antiseptics into the abdomen. Evidently they are not essential, and may easily prove injurious.

There is considerable difference of opinion as to the propriety of drainage. I have thought that it was reasonable to keep in a tube so long as fluid came through it. It is not unlikely that moisture is favourable to the disease, and that absolute dryness of the peritoneum may in this, as in analogous cases, promote a sounder cicatrisation. It will be found that after the first four-and-twenty hours little or no fluid escapes, and thus guided, I have never found it necessary to retain the tube longer than a week. The only possible evil is the formation of a fistula. But in my experience the wound heals at once on the withdrawal of the tube. The cases already mentioned in which a fistula formed healed in the first place, and the fistula resulted from the slow formation long afterwards of an abscess which opened through the cicatrix.

TREATMENT OF FRESH WOUNDS *

I SUPPOSE that as the recipient of discharge, as a protection from external irritants, as a means of retaining parts in contact, or for other reasons, some form of dressing is required in the treatment of wounds. The ideal surgical dressing is one which, while effectually meeting these ends, requires no interference until the wound is healed. If and when this can be attained, the rapidity of healing will be determined simply by the vigour of the individual, and pain and anxiety will be absent from patient and surgeon.

Many attempts have been made to attain this end. Mr Lister's methods keep it in view; M. Guerin has aimed at it in his cotton-wool dressing; and Mr Gamgee has strongly supported a similar treatment. I have recently made some experiments in the same direction, using the salicylised wool introduced by Professor Thiersch.

Three obstacles to a permanent dressing present themselves. These are the sutures, the drainage, and the anti-septics.

A wound cannot heal quickly unless the contact of its surfaces be reasonably close. The more accurately the edges are approximated, the more certain are they thus to heal. This accurate apposition can only be brought about by sutures. To attain, then, the object which we have in view, it is first of all necessary to have sutures which can be absorbed by the tissues, and we find our wants to a great extent met by carbolised catgut. But the chief difficulty in the use of sutures undoubtedly lies in the tension to which they subject the textures. If the tissues are perfectly lax, and union by the first intention take place, the new material which

* *Transactions of the Medico-Chirurgical Society, Edinburgh*, vol. i., New Series, 1881-82.

unites them is sufficiently strong in three or four days to resist the normal elasticity of the cutaneous envelope. In such cases a fine catgut would last for the necessary time. But inasmuch as, from exudations of blood or serum, or from other circumstances, either the union may be somewhat delayed or the tension somewhat greater, it is well that the catgut be of such a quality and thickness as to secure it from absorption for seven or eight days. More than that is unnecessary. It is a mistake to suppose that the use of a permanent material, as wire, silk, or horse-hair, enables you to resist tension. The fact is that if a wound be perfectly lax, sutures are of use only in maintaining steadiness; while, on the other hand, if the tension is such that the wound requires support against it after the first week, any ordinary suture which may have been left in will have cut its way through the skin, and, so far from doing good, will add to what inflammatory action may be present.

There are cases in which, from deficiency of skin or from other causes, we must be prepared to encounter much tension, and I believe that under healthy and aseptic conditions we may attain union of any surface which we can bring into apposition by manual pressure, and which can be so maintained without cutting off its blood-supply. But in such circumstances we must distribute the pressure and take it off the margin of the wound by buttons or quills, being careful so to apply them as to get the maximum of relaxation at the edge with the minimum of pressure on any one point. It is not in virtue of the permanent nature of the material, but because of the distribution of pressure, that we meet with our success. Even thus tension is not entirely evaded. These sutures are not without their disadvantages. The material which unites the buttons tends, of course, to pass from one to the other by the shortest route, and in doing so produces ulceration, which is none the less real because it is not always visible on the surface. In septic wounds it is frequent, and in aseptic it occurs that suppuration forms in the track of such sutures, and, if it do so, is apt to be dammed back by the button. They ought, therefore, to be taken out as soon as possible. Of them, also, it may be said that in most cases they have

fulfilled their mission in ten days; and unless the tension be very great indeed, and the union very sluggish, I find they may be safely removed in that time.

Plainly, then, catgut, especially if chromicised, may be used to unite the buttons instead of silver wire. When it is absorbed the buttons will lie harmlessly under the dressings, in simple contact with the skin. I have used this means of buttoning in cases where the tension was really considerable, and with admirable effect; but I have not yet ventured to discard wire when much force was required. I have, in short, long given up every suture but catgut for the edges of a wound, and only occasionally use the silver wire and buttons for relaxation.

If, then, we can get a dressing which otherwise we may leave on indefinitely, there is little impediment in the sutures. In most cases catgut may be used, and in the others a renewal of the dressing at the end of a fortnight will allow of their removal. This limitation, however, there is from the side of the suture.

Drainage is in many respects an admirable invention, and in some aspects the recognition of the principle it involves is one of the most marked improvements of modern surgery. But I have no doubt whatever that it may be carried too far. In suppurating and septic wounds it may easily become a source of irritation. As a rule, in these cases, if a drainage-tube have not fulfilled its purpose in a week, the meaning is that it is badly placed or insufficient, and in any case that a new departure must be made.

In fresh wounds, again, it is evident that if we can approximate the condition to that of a bruise we have little need for drainage. No one would think for a moment of converting a simple into a compound fracture merely with the object of evacuating the blood and relieving tension. The tension and extravasation of blood is often very extensive after severe contusion, but we await absorption with the most perfect confidence, certain that suppuration will not occur unless the conditions of constitutional health or local destruction of tissue be highly unfavourable. Now, after an injury inflicted by the surgeon, although in many

cases the constitutional state may be bad, the local injury ought to be more favourable than in a severe bruise. The tissues are sharply cut and perfectly viable, and the vessels are tied so carefully that serious distention should not occur. If, then, there were no other intercurrent circumstance, drainage ought to be unnecessary. But by almost universal concurrence of opinion it is held to be necessary. The reason is to be found in the admission of air and its contents. It is because the results of such admission are the conversion of the harmless fluids of a bruise into virulent and deadly poisons, which can only be prevented from producing their pyrogenous and phlogogenous effects by immediate evacuation. The most perfect examples of the character of these products are met with in abdominal surgery, and the most beautiful results of drainage are there also seen. The bloody serum to which Dr Marion Sims has recently been drawing attention is the source of the evil. But why? No one will pretend that a little bloody serum in the peritoneum would produce an evil effect unless there were a wound. It does not do it after an operation for strangulated hernia. It only does it in a few ovarian operations. Plainly because only in these few has it been poisoned from without. And the results are there rapidly fatal because the surface on which the poison exerts its phlogogenous action is so enormous and its means for entering the circulation so free.

Well, then, if the antiseptic theory be true and its practice moderately feasible, we have no need for drainage on this account. It is not necessary to drain the bloody serum, because the bloody serum is innocuous. But it is said that the irritation of antiseptics increases the flow of serum from the wound, and renders drainage necessary to avoid tension. Practically, I doubt the truth of this observation; at least the discharge is not of such amount or kind as to make any demand for special means of drainage in the majority of our cases. On the contrary, because it is serous, it flows easily through the interstices between the sutures, and it is certain that its flow ceases in a very short time. I find that if a wound remain aseptic drainage-tubing is rarely required.

I have come to this conclusion after trying all sorts of absorbable tubing, catgut, decalcified bone, and other forms. The catgut is unnecessary if the wound be aseptic, is inefficient if it suppurate. The others last too long if the wound goes on well, melt and soften in a few days in purulent discharge. A good example of the behaviour of Neuber's tubes was seen in an amputation of the ankle. It required no dressing after operating, until at the end of a fortnight I thought we might find it healed. It was so, but the large tube which had been inserted at one corner projected unaltered. I could not withdraw it, because its deeper part was firmly incorporated with the tissue. I pressed the soft parts back, cut the tube off short, and a week afterwards found it covered over. It had simply acted as an impediment to healing, and had been utterly useless from the first, for there was not a tablespoonful of dry blood in the dressings. Drainage, then, I regard as usually unnecessary in aseptic wounds in which the ligature of vessels has been properly performed, and in which the deep surfaces are kept in close contact so as to induce any slight ooze to pass outwards.

Undoubtedly, if accumulation were to take place it would delay healing by keeping surfaces apart, although I do not find that in aseptic cases it tends to cause inflammation or raise the temperature. Still it is to be avoided, and there are wounds in which it is nearly certain to occur if special precautions be not taken. In these, indiarubber or glass is the best material, but it ought to be removed as soon as possible, and a special dressing for the purpose is advisable on the night of operation or the following day.

There yet remains for consideration the influence of putrefaction and its prevention.

There are two methods whereby the evils of septicity may be avoided—to prevent the entrance of the germs of putrefaction, and to render the soil unsuitable for their multiplication. The first and most important involves all that minute attention to detail in cleanliness and the use of germicides which Mr Lister has so thoroughly established. His dressings fairly accomplish this object, and the results

attained are admirable. But the second is scarcely less important. It is less important because, if the entrance of noxious influences be prevented, it would matter little what the nature of the soil might be. But undoubtedly germs creep in notwithstanding all precautions, and it is of great moment that they should find conditions unsuitable for their multiplication. I suppose, for example, that the chances of a wound becoming septic are much greater in a diseased than in a healthy man, that the germicide properties of healthy tissues are much stronger than those of diseased textures; and it is certain that the results of septicity are much worse in the weakly than in the strong. This object, of course, involves the care of the patient's general condition. But the soil may also be rendered unsuitable for germination by means applied locally.

I have sometimes thought that perhaps we are too timid in our application of antiseptics to wounds. I have had exceedingly good results from carbolic irrigation. I excised the os calcis in a patient of Dr Sidey's whose wound, septic before, remained so after operation. The temperature rose alarmingly, and we thought the patient would die of septicæmia. Irrigation was established by three worsted threads, which led into the cavity continuously for a fortnight a stream of 1 to 40 carbolic lotion. Yet the patient suffered in no way from carbolic poisoning, while the symptoms of septicæmia disappeared, and healing went on steadily, though slowly. But whether or no highly septic discharges should in some instances thus be charged with an antiseptic, we have other methods of rendering a fresh wound unfavourable to the propagation of germs. Two of these seem to me specially important. Dryness is highly conducive to safe and rapid healing. Moist warmth favours all forms of decomposition, and promotes exudation. Careful arrest of hæmorrhage, and a covering at once dry and permeable, are therefore strongly indicated.

Again, accurately applied compression is an aid to quick union, because it brings together and keeps in immediate contact parts which may at once adhere, and which thus also come, as it were, within striking distance, and so exercise

their germicide properties with success. Further, it helps to arrest the exudation of blood from the capillaries and smaller vessels, and prevents bagging of the wound.

Now the mode of dressing by gauze and mackintosh, while reliable for the exclusion of septic influences, is in these ways imperfect. It is an immense improvement on the old painful daily dressings. But it promotes moist warmth. It does not lend itself readily to equable compression. It cannot be used as a permanent dressing. It requires renewal weekly, because the antiseptic is in that time exhausted at the temperature of the body. Doubtless, exhaustion of the antiseptic would do no harm unless the discharge were sufficient to reach the edge of the dressing. If you cover an aseptic fluid with a few layers of gauze or with a single layer of mackintosh, it will remain for any time and at any temperature untainted. But the mackintosh keeps the skin underneath it moist from perspiration, and even were there no discharge from the wound, putrefaction would occur so soon as the antiseptic was sufficiently exhausted.

Considerations such as I have endeavoured to explain, and many clinical attempts with this and that method, have gradually led me to adopt a style of dressing which enables me in a large proportion of cases simply to lay the patient aside till his wound has healed. The method is as follows:—The wound is carefully and accurately stitched. If it be large, an interval of an inch or less is left between two of the stitches at some convenient part, and if, from the nature of the wound, firm compression throughout is uncertain, a few of the catgut ligatures are left long and brought out at the interval, or an indiarubber drainage-tube is inserted. In most cases it is completely closed. A piece of dextrinised oiled silk is applied. Mr Lister's object in this application is to protect from carbolic irritation; but were it not that it also prevents adhesion of the dressings, I should not consider it of importance when the edges of a wound are carefully approximated. One layer of moistened gauze is the next covering, and over it a thick padding of dry salicylic wool, firmly compressed by a gauze bandage. The layer of gauze facilitates the ultimate removal of the dressings. Without it

the wool, irregularly hardened by the dried blood, forms a sort of cuirass and is apt to stick. I have tried silk instead of wool as a dry dressing. It does not, however, produce the same equal pressure throughout.

I have now used this dressing in a large number of cases, which I desire shortly to enumerate.

I. For long I have been in the habit of using a modification of the method in small wounds, whether produced by accident or by the surgeon. Having smeared them with carbolised oil, as the least irritating of antiseptic applications, and, if necessary, applied stitches, I seal them completely up with salicylic wool and flexile collodion. In cases too numerous to relate the healing has been perfect, and the failures extremely few. In large accidental wounds the dry dressing described is very successful. Thus, in two cases of immense scalp wound, and in one in which the belly of the supinator longus was completely torn across, only the one dressing was required. Septic matter, however, is apt to be introduced in such injuries. Thus, in a severe compound fracture opening into the elbow-joint, and in an extensive lacerated wound of the thigh, the dressing was abandoned for a time on account of suppuration, though both ultimately healed exceedingly well.

II. The following are examples of perfect healing in operations of a minor character. By perfect healing I mean that which occurs where there has been no rise of temperature, no pain, and no necessity for change of dressing till the wound is closed:—

a. Six amputations of the fingers for injury.

b. An amputation of the middle finger for whitlow in E. T., æt. 62, admitted 17th March, dismissed 8th April 1882. I mention this specially as a case in which septic inflammation extending far into the palm was successfully corrected by chloride of zinc.

c. Three cases of operation for strangulated hernia, viz., C. F., æt. 27, admitted 26th January; W. R., æt. 27, admitted 8th March; and C. G., æt. 84, admitted 14th March.

d. J. M'T., æt. 17, excision of sesamoid bone of great toe. Admitted 1st February.

e. C. L., æt. 27, admitted 20th June 1881. Excision of adenoid tumour of the mamma.

f. An interesting extension of the principles involved was made in the application of the method to the opening of abscesses where there is no persistent cause of pus formation, and where pressure can be accurately adjusted. Most of them were out-patients, but these may be mentioned as admitted to the house:—A. P., æt. 29, 11th March; abscess of the spermatic cord; found healed on the tenth day, the dressing not having been removed till that time; J. L., æt. 7, 11th January, large glandular abscess of neck; and E. F., 1st March, deep abscess of axilla, in whom the same success was attained.

g. J. B., æt. 38, admitted 30th March. The actual cautery was applied for ulceration of the cartilage in the knee-joint, with good effect so far as the joint was concerned. The wound was looked at simply from curiosity only twice, with intervals of fourteen and twenty-one days.

III. The following are examples of perfect healing after major operations:—

1. Mrs B., æt. 34, a patient of Dr Soutar of Golspie. The family history was most unfavourable, and she had tubercle in the apices of the lungs. Amputation of the forearm was performed for disease of the wrist. The incisions were made through healthy textures. The dressing was not removed for three weeks, when the wound was found perfectly healed, with only a little dried blood in the dressing.

2. J. C., æt. 19, admitted 14th June 1881. Syme's amputation at the ankle was performed for disease of the tarsus. The dressing was removed after a fortnight, as a matter of curiosity, not that it was required. A superficial abrasion at one corner alone remained. The dressing was renewed, and it was found healed in another week.

3. D. L., æt. 37, admitted 1st December 1881. In this case the patient was rapidly sinking from strumous disease of the right wrist, right ankle, and third toe of the left foot. The wrist and toe were treated, after amputation, by the method I have described, while the ankle was otherwise dressed. The amputation at the ankle was performed

through putrid sinuses, the others through fresh surfaces. These last were healed absolutely when the dressing was taken off for the first time three weeks after operation. As sometimes occurs, however, the general health of the patient having become temporarily disordered, I think by continued discharge from the ankle, ulceration of the cicatrix to a slight extent occurred in both three or four weeks after they had healed.

4. R. S., æt. 18, admitted 20th March 1881.—Syme's amputation at the ankle. This case, already referred to as being delayed by Neuber's drainage-tube, was only dressed once at the end of a fortnight.

5. J. P., æt. 38, a patient of Dr T. Balfour.—Excision of the mamma was performed for scirrhus. The wound was found absolutely healed when it was dressed after ten days.

6. G. B., æt. 15, admitted 1st February.—Amputation at the upper third of the arm for injury. It had been caught between the rollers of a printing-machine. Examined in three weeks, it was found absolutely healed, with the exception of a surface about the size of a pea, where plainly a portion of bruised skin had sloughed. In another week that also had closed.

7. M. P., æt. 46, admitted 28th February 1882.—Amputation at the wrist-joint was performed for gangrene of the hand from obstruction of the brachial artery. It was examined in fourteen days, because she felt "as if there were discharge in the dressing." There was none. It had all but healed, and was absolutely united when we saw it again in another fortnight.

8. A. B., æt. 16.—Amputation of the foot for injury was performed by Syme's method, 19th November. The stump was dressed the same evening, and a drainage-tube which had been inserted was removed. It was not again looked at till the 21st day, when it was found healed.

9. Mrs E. W., æt. 46.—Excision of the mamma for scirrhus was performed 12th May. It was dressed next day, on account of slight oozing through the dressing. It was found healed at next inspection on 26th May, and she went back to Shetland 30th May.

10. J. I., æt. 12.—Excision of the lower end of the humerus on account of old dislocation was performed on 23rd May. A small indiarubber drainage-tube was inserted at the operation, but was removed next day. The wounds were absolutely sound at next inspection on 5th June.

But a method is to be judged as much by its failures as by its successes. I mention all my remaining cases in two classes—1st, those in which the success was not complete, but in which the patients made exceedingly good progress; 2nd, those in which, after trial, the method was abandoned.

I.

1. G. M'L., æt. 53, was admitted to the Infirmary 15th March 1882, suffering from extensive ulceration of the leg, for which I performed amputation below the knee. At the point of amputation all the tissues were much swollen and œdematous. It was necessary to change the salicylic wool five times in six days on account of the serous exudation having penetrated the dressing. It daily diminished in quantity, however, and the sixth dressing did not require removal for fourteen days, when the stump was found healed. There was no rise of temperature whatever.

2. Mrs M. W., æt. 36, had been treated in the Infirmary for many weeks on account of disease of the knee-joint, at first with promising results. Subsequently, however, it became evident that she was gradually sinking, partly from the discharge from the knee, partly from progressing phthisis. I accordingly amputated through the diseased tissue, where a sinus already existed, by Mr Spence's method. For ten days the dressings had to be changed every second day on account of the flow of bloody serum. After that the much longer intervals of ten days or more intervened, and the healing, though slow, was ultimately well accomplished. The temperature, which before the operation had been habitually as high as 101° each evening, never rose above 99° after the operation.

3. D. J., æt. 8, 13th March.—In this case I excised the knee-joint on account of progressive disease, and because it was fibrously ankylosed at right angles. The skin was un-

broken at the time of operation, but we failed to maintain the parts aseptic. Nevertheless the dressing remained unchanged for a week, and only required reapplication at long intervals. There was some increase of temperature for three or four days, and pus was found in the dressings at each inspection. It has healed soundly, though slowly.

4. Mrs W., æt. 46.—Excision of the mamma for scirrhus was performed on 10th May. In this case so much skin had to be removed that buttons with catgut had to be used. Micrococci were found in the discharges, and the dressings required renewal five or six times, but it healed quickly, and temperature never rose above 99.5° .

II.

In the following cases the dressing was abandoned at various dates after operation:—

1. An amputation at the ankle for severe injury of the foot.
2. Amputation at the ankle by the lateral flap, consecutive to excision of the os calcis.
3. Excision of the wrist.
4. Operation for strangulated inguinal hernia.

In two of these cases, Nos. 2 and 3, we failed to correct the putrescence which existed in the tissues at the time of operation; in the other two, although the skin was unbroken, sepsis also occurred. All did well. Having ascertained the presence of bacteria, and observing that the parts were somewhat red and swollen, I introduced drainage-tubes and applied a boric poultice. In a period varying from a few days to a week these symptoms disappeared, and the dry dressing was resumed and continued, with long intervals between the renewals, until healing took place.

These cases constitute, as yet, all my experience of the method. If we include the strangulated hernia, the number of important operations was—

- I. With perfect healing, 14, viz.,—
 - 3 Herniotomy.
 - 3 Amputations at ankle.
 - 2 Amputations in forearm.

- 1 Amputation at wrist.
- 1 Amputation of upper arm.
- 3 Excision of mamma.
- 1 Excision of elbow.

II. With almost perfect healing, *i.e.*, in which dressings required renewal, although very rarely, and the general and local symptoms were absolutely good, 4, viz.,—

- 1 Amputation of thigh.
- 1 Excision of knee.
- 1 Amputation of leg.
- 1 Excision of mamma.

III. In which failure occurred, *i.e.*, the dressing was abandoned on account of inflammatory action, 4, viz.,—

- 1 Strangulated hernia.
- 1 Excision of wrist.
- 2 Amputation of ankle.

Three other major operations were since performed through pre-existent sinuses, and dressed in this manner. In one, in which asepsis was maintained, the dressing required renewal several times, though at long intervals, and it was continued throughout. In the other two putrefaction continued, and the dressing was temporarily abandoned.

The deductions I would draw from my observations are:—

I. That when we succeed in preventing sepsis at the operation we can by this method rely on singularly simple and favourable progress, with total absence of fever, with freedom from pain, and with great rapidity of healing. It is proper to be exceedingly careful in the arrestment of hæmorrhage; but even did a little reactionary oozing occur, it is only necessary to renew the dressing once, or, as I did in some of these cases, to put on an additional layer of wool.

II. That should sepsis lead to inflammatory action, it is necessary temporarily to abandon the dressing.

III. That even in septic cases it is often advantageous to continue it throughout if there be no inflammation, or recur to it when inflammation has subsided, because it is a good excipient for discharge, because it tends to diminish the

amount of pus, and because it aids in keeping the part at rest.

IV. That in fresh wounds so situated that they can be firmly and equably compressed the introduction of materials to secure drainage is usually unnecessary, and that if the drainage-tubes be introduced they should be removed in twenty-four hours.

V. That sepsis is less apt to occur by this method, as it avoids the risks which attend changes of dressing and movements of drainage-tubes, which, in my experience, are more fruitful sources of failure than even the original operation.

GERMS AND THE SPRAY *

IT is curious to speculate on the fate of Listerism had our knowledge been formerly as it is now. How would these methods have been received had we known that under antiseptic dressings organisms may flourish while the wound follows what has been called an aseptic course; that all suppurations of an acute character, and some of a chronic, are attended by microbia although unexposed to the air; that the injection of an aseptic fluid may induce septicæmia; that contamination of a wound through the atmosphere is comparatively unimportant, while the means taken to prevent it are inefficient? On this last point I have made a few investigations, which I desire to lay before you.

A few years ago I began an attempt to discover the percentage which remained aseptic of cases in which major operations were performed when the skin was previously sound. I have found it by no means an easy task. It is plainly impossible to trust such tests as the odour of the discharge or the discoloration of the protective oiled silk, because they may be absent while organisms are present, and may be produced by other causes than bacteria. Search with the microscope and cultivation of the discharges in aseptic fluids appear the most reliable methods, and have been diligently put in operation. Even they, however, are not absolutely satisfactory, because they are apt to give contradictory results. In my experiments flasks became turbid and showed organisms of various kinds after impregnation with discharges in which the microscope had not detected life, and bacteria may be visible in the discharges which are difficult to cultivate. Gradually improve-

* *Transactions of the Medico-Chirurgical Society, Edinburgh*, vol. ii., New Series, 1882-83.

ments were made whereby errors in these respects were to a certain extent eliminated. The various methods of staining came to be at our disposal, and the use of different cultivating fluids was practised. The results did not even then accurately correspond, but at least it was possible, from first to last, to err on the side of caution, and accept as truly septic only those cases in which no doubt could possibly exist. I was astonished, during the research, to find that in more than half the cases, most of which I had hitherto regarded as aseptic, organisms could be at one time or other discovered. My first idea was, of course, that this arose from defective skill in the management of antiseptics. But in any case it was a matter of importance. If the error were manipulative, then the difficulty of manipulation was so great as to make the treatment unsuited for common use; if not, there was a leak in the Listerian hypothesis. Precautions in the treatment were redoubled, and, to check my own results, I obtained from my colleagues dressings from cases which they believed aseptic. The percentage was somewhat reduced, but yet I found that about forty per cent. was the proportion which I could not by any means diminish, and that, in short, almost every case which was not after the first day or two absolutely dry, showed organisms in the fluid exuded.

It was impossible to continue in the belief that the fault lay solely, or even mainly, in defect of manipulative skill. Did they come, then, from within the body or from without? Was it, or was it not, a result of imperfection in the methods of Listerism?

And first, do they come from within? There are various experiments and observations which bear upon this point. That organisms of various kinds easily find their way into the blood by other channels than by palpable wounds may be held as certain. We have in evidence the whole class of zymotic disease, and more especially those in which the presence of microzymes has been directly demonstrated. That from the blood they may pass into and develop dangerous energy in the tissues, the published cases of endocarditis and osteomyelitis sufficiently testify.

The curious experiments in bistournage are doubtless capable of several explanations; but if they may be held to indicate the rarity with which certain organisms are present in quantity in the blood, they indicate, also, their power for evil and the certainty with which, when present in the circulation, they attack tissues devitalised by injury.

Dr Burdon Sanderson has asserted that by injection into the peritoneum of aseptic fluid in sufficient quantity bacteria of virulent quality may be there engendered, an experiment which suggests that a certain proportion between the number present in the blood and the amount of local irritation is necessary to ensure their development.

Lastly, Dr Ogston's observations tend to confirm this view. He found in a long series of acute abscesses the invariable presence of chains or groups of micrococci. I have repeated his observations by his methods in most of the abscesses which have come under my care since his paper was published. In three of the acute abscesses, a bubo and two whitlows, I was unable to find organisms; in the others they were present. All contained micrococci, several contained rod-like bacteria, and one a number of long-jointed rods. I have not found them so commonly in chronic abscesses. In a few, however, they were undoubtedly to be seen. Thus, in two strumous glandular abscesses of the neck and in one of the axilla, I found multitudes of micrococci. Chronic abscess is probably more frequent in the abdomen than elsewhere. Except those connected with caries of the spine, in which I have only once met with them, organisms, generally bacteria, as well as micrococci, are constantly to be found in abdominal abscesses. Two of those which I examined were nephritic abscesses, one being due to the presence of a calculus in the kidney, two were probably from chronic perityphlitis, the others of undetermined origin. It is, of course, possible that in some of these cases contamination may have spread from the neighbouring digestive or urinary tract, and that in others the organisms arrived by the lymph path, but this explanation cannot apply to many. It is impossible to avoid the conclusion that if the abscess be the effect of their presence, their entrance into the

blood is comparatively common; if it be the cause, their great frequency in acute abscess would indicate either their constant presence in the circulating fluid, or that very slight disturbance of the general economy may afford the necessary conditions.

It appears certain, then, that we have here what I have called a leak in the antiseptic system. It is certain that organisms are frequently found in discharges apparently aseptic. It is proved that similar organisms easily enter the economy otherwise than by a wound. It is proved that organisms thus entering may produce local effects of varying degrees of severity, and are prone to attack injured textures. It is proved that they almost invariably attend acute inflammation. Is the conclusion not warranted that they sometimes present themselves by this means in surgical injuries, and that the severity of the resultant effects will be determined by the same causes as influence them under other circumstances? What these causes are we have yet in some measure to determine. Some of them, at least, we know; others we can only conjecture. We know that moisture and warmth favour the multiplication of these organisms, and that dryness and drainage are inimical to them. We know that the healthy textures may absorb a certain quantity of fluid, may destroy a certain number of microbia, but that beyond a certain point their germicide power does not extend. We believe that very various properties belong to different germs, whether from innate generic and specific attributes, or as the results of cultivation, natural or artificial. The results of moisture, of warmth, of drainage, of tissue health, of differing potencies, apply with equal force to germs which come from within and from without. My observations as to the presence of bacteria in wounds suggest to me that they are less harmful when they occur under antiseptic dressings than otherwise. May it be that their passage through the blood often deprives them of a portion of their virulence, or is it that the forms harmless to the blood are so also to the tissues more or less? I excised a knee-joint for ankylosis in complete flexion. The wound healed in less than three weeks, and after the first day or two was dressed only once a week, while the

temperature never rose above 99°. In the scanty watery discharge abundant rod-like bacteria were invariably found, although absolutely in every other respect it followed an "aseptic course." And what adds to the striking character of the case is, that when, six weeks afterwards, in the same patient, for the same cause, I excised the other knee, the result was the same. That is merely one example out of many, and I have no hesitation in believing that had I allowed these wounds, while exuding fluid, to become contaminated from without, the result would have been modified to a greater or less extent.

Is, then, the difference between a compound and a simple fracture, an open and a closed bruise, to be accounted for solely by the presence of bacteria in the one and not in the other? A patient was admitted to the Infirmary who had ruptured, by sudden strain, some of the adductor fibres in his thigh. In the extravasated blood suppuration occurred, and when I opened the abscess I found many micrococci. Why does this suppuration not more often occur? The suggestions given above may partly explain; and it may be that although micrococci come to many, they find blood-clot pure and simple an unfavourable soil. Much has been said of the evils of tension such as existed in the case referred to. Tension does harm, undoubtedly, but not so much alone and in itself as by the nature of the exudation which causes it or to which it may give rise. The invariable tension of a bruise by blood-clot causes little harm. Tension by bloody serum, in developing bacteria from within or from without, may result in suppuration and septicæmia. But it would be impossible within reasonable limits to pursue these thoughts. They are sufficient to enforce the practical importance of the general condition of your patient prior to an operation, and the necessity for precautions against the development of germs coming from within.

But it must be admitted that contamination comes also from without, and to determine the question to what extent the organisms we find in wounds are due to one or other cause, the efficacy of external preventive measures must be ascertained. Can we—independently, I mean, of the necessary

failures arising from human fallibility—by our present modes of antiseptic dressing insure the exclusion of external septic influences? And, first, are the materials with which we cover our wounds able to exclude organisms? It is quite certain that with flasks containing sterilised but putrescible fluids many materials may be used for that purpose with success. It is sufficient to cover with cotton-wool, gauze, or almost any thick or impenetrable material, which has first itself been sterilised. I have found one of the most convenient coverings in experiments with such flasks to be a layer of ordinary pink macintosh, because it diminishes evaporation. It is not infallible. If you hold it up to the light, you will see that the waterproofing is often imperfect, that there are minute holes. Sometimes, presumably from this cause, it fails. Thus on one occasion I prepared six large beakers of Darby's fluid meat, and, having made certain experiments with them, set aside four, which remained aseptic after three days' incubation. At the end of eleven weeks a delicate globe, like fine wool, began to form in the centre of the fluid in one of them, ten days later in a second, and gradually the whole contents became mouldy, while the other two remained unchanged for several weeks longer, and were at length used for other purposes. The macintosh may fail, then, but it does so very, very rarely; and I have frequently laid the flasks and beakers on their side for many hours, with the fluid in contact with the macintosh, without the contents becoming septic. But it is necessary that there be no putrescible channel leading from the outer air to the inner liquid. Such a channel speedily leads to putrescence of the liquid. Thus a flask containing Darby's fluid beef in aseptic solution was covered with macintosh, incubated, and kept a week. It was then laid on its side for a few minutes. The macintosh was made so tight that only a very little moisture escaped. Again incubated, it still kept clear. But after being kept on its side for twenty-four hours, a hardly perceptible outflow being thus kept up, it rapidly became cloudy on incubation.

Some interesting corollaries follow from these facts. Among them, for instance, this, that indiarubber and other materials of a like character, when thoroughly purified, may be used,

with proper care, as in themselves a protection from sepsis; and that the macintosh or gutta-percha tissue are sufficiently safe as the outer layer of a dressing. It is evident, then, that with any of the ordinary forms of antiseptic dressing the only fear of contamination arises from the discharge or other putrescible fluid communicating with the outer world at the margin for too long a period. That this is an occasional source of sepsis, especially when dressings remain long unchanged, is certain, and generally recognised. Mr Watson Cheyne's observations indicate, in his opinion, that this is apt to happen with micrococci when an ordinary gauze dressing has been kept on a very few days. And it must be remembered that the ordinary secretions of the skin, confined by impermeable material, may form the necessary channel as well as pus, a strong argument in favour of dry and absorbent dressings.

These observations of Mr Watson Cheyne's, which may be suggestively compared with Dr Ogston's on micrococci in abscess, receive also an explanation in a little experiment which I made with carbolised gauze. I covered the floor of the incubator with sixteen layers of antiseptic gauze, and exposed it for an hour to a temperature of 98° . I introduced a beaker of aseptic fluid, and left it exposed for twelve hours. It was then covered with macintosh, and remained clear till the fourth day, when it was again uncovered for twelve hours. It now became putrid, as did also beakers introduced on the fifth and sixth days. It is clear, therefore, that at least from the fourth day onwards the ordinary carbolised gauze maintains a wound aseptic, not because it contains an antiseptic, but because of the filtering material of which it is composed, and in which respect it is much excelled by dry cotton-wool and other dressings.

It is probable, then, that a certain proportion of the cases in which we found microbia may be accounted for by delays in dressing and by the nature of the material employed. There is here a tendency to failure, but one which can be guarded against by proper precautions. Any antiseptic dressing in common use may be rendered safe and effective.

But, secondly, are the precautions which we take when

wounds are uncovered, as at the time of operation or the times of dressing, sufficient to protect them? Is the spray, for example, efficient? When engaged in testing wounds by cultivation experiments, I was in the habit of employing flasks for purposes of control. Two flasks, in every respect similar, were brought to the wound, opened and closed simultaneously under the spray, while one was inoculated with discharge and the other not. In two cases both flasks went wrong, and in one the flask which had not been inoculated became septic, while the other remained pure. I began to doubt the spray. *A priori* one would be strongly inclined to believe that the rapid passage of septic material through an attenuated spray would not be sufficient to neutralise its malevolent properties; and that the passage might often be extremely rapid is very likely. I instituted, therefore, a series of experiments with aseptic solutions. It is hardly necessary to premise that every precaution was taken, in the way of washing with strong antiseptic solutions, not only hands and instruments, but also the coverings and external parts of the vessels before opening and closing them, so as to prevent contamination otherwise than through the air. The following was the method adopted:—A weak solution of Darby's fluid meat was rendered aseptic by boiling, and incubated. Two similar vessels containing it were opened simultaneously on the same table, one under the spray and the other without the spray, and were kept open for the same time. They were then reclosed and incubated at a temperature varying little from 98°.

It appeared probable that the fall into the fluid of carbolic solution from the spray might render it an unsuitable soil for germination. Many of the earlier experiments were therefore made with coverings, such as bell jars or sloping or horizontal plates of glass, suspended over the flasks or beakers. It was found, however, that this precaution was unnecessary, because uncovered vessels over which the spray had been playing for long periods were rendered muddy and putrescent with as great facility as others. I at first used beakers an inch and a quarter wide and two inches in depth. We found that at least twenty minutes' boiling

was necessary to render them aseptic, and that in that time at least half the fluid had evaporated. We began with an exposure of one minute, and gradually increased the duration to forty-five minutes, with the following result:—Thirty-eight flasks were exposed under the spray, of which seven became putrescent; thirty-seven (one being spilled) were exposed without the spray, of which nine became putrescent. This is, of course, an unfavourable result for the spray. It might be, however, that some of the failures on both sides, comparatively few as they were, arose from manipulative defect. Thus the incubation period in some of them we found was not sufficiently long, because we subsequently met with one beaker in which cloudiness did not arise till it had been kept for four days in the incubator. Now we had been content with two days' incubation, and in some with only one. In several cases the same beakers had been used twice, and although the two sides were in all cases kept strictly parallel, it was unsafe to draw absolute deductions. It would only become a satisfactory experiment when we reached the period of exposure when putrefaction invariably ensued if the spray were not used. But with these small beakers this could hardly be reached without introducing other errors from the absence of personal supervision. Thus one of them was exposed without spray for periods of forty-five, fifty, and fifteen minutes on successive days without losing its perfect clearness. It was necessary to use larger tumblers. Beakers two inches in diameter and three inches in depth were exposed for periods of twenty, twenty-five, and thirty minutes. Of five under the spray one became septic; of four without the spray one also putrefied.

Seven beakers $2\frac{3}{4}$ inches in diameter were exposed for the same periods with and without the spray. On both sides two remained clear and five became cloudy. We were approaching now a more accurate period of exposure. Beakers $3\frac{1}{4}$ inches in diameter were procured. But we were now confronted with a difficulty which had been gradually increasing with the size of the beaker. We found it almost impossible by prolonged boiling to render the contents of a vessel of this

size aseptic without causing such evaporation as to reduce the quantity of fluid and increase its concentration too much for any useful purpose. By many experiments we came to the conclusion that this was due to the quantity of fluid, and not to the shape of the vessel or its proportion to the amount of contained liquid. I am unable to explain this curious fact, but it is indubitable that in proportion to the size of the beaker we required from twenty to ninety minutes to attain an aseptic state with fair certainty. We overcame the difficulty by Mr Tyndall's plan of interrupted boiling, and we found, after many attempts, that we could insure success with beakers of any size by boiling five times, for one minute each time, at intervals of twelve hours.

Beakers of $3\frac{1}{4}$ inches were now, therefore, tried. It was found that up to twenty minutes' exposure they occasionally remained clear. Beyond that limit I have not succeeded in maintaining a single example unclouded, nine having been used on each side, four with twenty-five, and five with thirty minutes' exposure.

I do not know that it will be necessary to multiply these experiments. I am unable to see where error can creep in. They seem to me definitely to prove that, so far as the destruction of floating germs in the air is concerned, the spray is perfectly ineffectual.* It may be a convenient method of throwing a solution of carbolic acid of the strength of one to forty upon a wound. It may thus be useful in open wounds, but it can be of little value in such operations as those which deal with deep cavities. Nay, I should think it likely to prove more injurious than beneficial in such cases. If you make the spray play across a beam of sunlight, you can see the floating dust in clouds rushing towards and being whirled along with it, so that one can hardly doubt that the margin of the spray is the most dangerous position in which a wound can be placed, and that a slight deflection from a current of air may result in the entrance of this floating matter to recesses where the solution deposited by the spray may never reach.

But these same experiments also show that the risk of

* *Vide* Lord Lister's Address before the International Medical Congress at Berlin—*British Medical Journal*, vol. ii., 1890.—ED.

contamination from the air has been greatly exaggerated. Putrescence invariably follows a sufficient exposure of putrescible fluid to the air ; but the germs which float about cannot be extremely numerous if a surface of an inch and a quarter may be exposed for nearly two hours without effect, and if to be reasonably certain of putrescence a surface of three inches and a quarter requires an exposure of twenty minutes.

The conclusion evidently is, that in the matter of wounds our prime object ought to be the attainment of the most absolute purity in all substances brought into actual contact with them, while the rare contamination which may come through the air is guarded against by washing with antiseptic solutions or by giving the germicide powers of the living textures a fair opportunity of coming into play. This conclusion is amply confirmed by the results of those who have not used the spray. The success of Keith and Lawson Tait in ovariectomy, and the statistics of Spence and Callender, not unrivalled by many others, in general surgery, are thus explained.

If, then, it be taken for granted that the spray is ineffective, other points for investigation are opened up. I hope to lay before you shortly the results of some researches in which I am at present engaged as to the germicide property of the vapours thrown off at different temperatures by various antiseptics. The volatility of the antiseptic used in a dressing has been considered of some importance. I have already pointed out that this is not sufficient to outweigh other considerations, and that the efficiency of a dressing depends rather on its material and on the ability of the antiseptic so to mingle with the fluids that they become unsuitable for the development of microzymes. I have ascertained, however, that the vapours of carbolic acid and eucalyptus, among others, are germicide at the temperature of the body.

It is possible that in this direction we may discover a substitute for the spray, as, for example, by rendering aseptic the air of the operating chamber. It has already been proposed by Mr M'Gill to pump upon the wound the vapour of eucalyptus. The following experiments, however, indicate that this is not sufficient.

1. Three ounces of eucalyptol were placed in a shallow dish on the floor of an incubator. This was closed, and the temperature kept at 100° F. for an hour. Three three-inch beakers containing aseptic Darby's solution were then introduced, uncovered, and left in the incubator at the same temperature for two days. The beakers were then covered and farther incubated, but all remained perfectly clear.

2. If the experiment be repeated, with this variation, that the eucalyptol is exposed in the incubator for five minutes only before the introduction of the beakers, then all become turbid on incubation, even though they have been exposed for only twenty minutes.

3. Six fluid ounces of eucalyptol were placed on the floor of the incubator and kept at a temperature of 60° F. for one hour before the introduction of the beakers, which were left exposed for twenty-four hours. All, three in number, became putrid.

These experiments require, of course, repetition and variation to arrive at exact knowledge. But we can see that the elements of time and temperature are of importance, and that a mere draught of eucalyptol vapour is not enough. It is necessary that it be given off in a certain volume, under a certain heat, and that the air be exposed to it for a certain time.

In this direction, however, farther investigation is required, both as to the efficacy and innocuity of the various antiseptic vapours.

WOUND TREATMENT WITHOUT THE SPRAY *

TWO years ago I brought before this Society what I thought to be strong arguments in favour of the substitution of cotton wool impregnated with a non-volatile antiseptic for the carbolic gauze commonly in use in the treatment of recent wounds. A year ago I related also to this Society a series of experiments which led me to believe that the spray of carbolic acid was useful only in so far as it threw a germicide solution on a wound, that it had no effect whatever in rendering the atmosphere aseptic. As, therefore, the spray is not only inconvenient but also useless, it has been entirely abandoned in the wards under my care, and I desire to-night to lay before you a record of my surgical experience since I carried these changes into effect.†

But first I wish to make a retraction. In one of these papers I spoke, although guardedly, of the possibility of giving up also the use of drainage-tubes, and I mentioned instances of amputation below the knee, at the ankle, and in the forearm, besides one or two other operations in which this had been done with complete success. I have since had further experience of a similar kind. Take one case out of several. A short time ago a girl was brought to the Infirmary suffering from compound fracture sustained in a steam laundry. I amputated the forearm a short distance below the elbow, as well as the tips of three fingers on the other hand. On the eighth day she was out of bed, and the dressings being removed at the end of a fortnight, the

* *Transactions of the Medico-Chirurgical Society, Edinburgh*, vol. iii., New Series, 1883-84.

† Lord Lister "Abandoned the Spray" in 1887—*British Medical Journal*, vol. ii., 1890.—ED.

wounds were found to be healed. Nothing can be more satisfactory than to lay a patient thus aside, and simply watch the pulse and the temperature chart till sufficient time has elapsed for the wound to heal. I thought and still think that under certain conditions this might be relied upon. The first of these conditions is that the wound be dry. The surgeon cannot spend too much time in arresting the flow of blood. Blood mechanically separates the parts to be united, and removes accidental microzymes from the germicide action of the tissues. It is better to have the wound a mass of ligatured points than to have the least flow of blood from the smallest vessel. Next it is a condition of success that by pressure or position the surfaces of the wound be kept in absolute and steady apposition. Lastly, it is essential that applications to the wound be free from irritating properties. Unfortunately we have not yet attained perfection in these necessary conditions. A little reactionary hæmorrhage will now and again occur, however careful the surgeon may be. It is impossible to make accurate and effective apposition in every form of wound. We do not yet know any bland antiseptics.

I had recently a case which is so curious that, while citing it as an example of the evils which accrue from want of drainage, I should like to record it on its merits.

A man, æt. 46, was sent to the Infirmary from the New Town Dispensary with a tumour in each groin. For twenty years a swelling had existed on the left side, which could be made to disappear on pressure. Eight weeks before admission it had rapidly increased and hardened, while the skin over it became red and adherent. I cut down, 12th February 1884, upon this tumour, and excised first a large mass of inflamed glands, each with its core of pus. Partly concealed by the glands I found a fatty tumour, and in dissecting it out came to a sac emerging with the fat from the widened crural canal. The tumour on the right side proved to be purely fatty, and its neck also passed through the crural ring. By dragging upon it, a portion of peritoneum could with some difficulty be brought into view. They were examples of that hernia of hypertrophied subperitoneal

fat, which has been described, chiefly by German authors, as bringing about hernia of the peritoneum also, and which in this case had produced on one side a well-marked lymphatic inflammation. The bleeding being carefully arrested, the wounds were dressed without drainage. On the right side healing took place by first intention. On the left, slight oozing distended the parts, diffused itself into the surrounding cellular tissue, and caused sloughing of the thin and inflamed skin which had been dissected from the glands. The wound remained aseptic, but a slow cicatrisation was the result in a case where effective drainage would have insured a speedy cure.

Yet again, on 5th February 1884, I excised the wrist, in a girl *æt.* 15, for fungous arthritis. The skin was yet unbroken, and I used bichloride of mercury as the antiseptic. When sewed up it was absolutely dry. The temperature next day rose to 101.5° . If in any case the temperature reach 101° , I regard it as an indication that the wound should be examined. It has become septic, or there is malposition, or tension, or some other local or general disorder. In this case I found the wound tense and red, distended by a dessert-spoonful of bloody serum. A drainage-tube was introduced, the temperature fell at once, and the case required no further care.

These were exceptions, no doubt, but they have been sufficient to induce me to resume the habitual use of the drainage-tube in large or deep wounds. That necessitates a dressing for its removal, but this inconvenience is counterbalanced by the avoidance of accidents. I am yet undecided when this dressing ought to be performed. Twenty-four hours is sufficient for drainage in aseptic cases, and I am inclined to think that it is best then to remove the tube. If you leave it for two or three weeks there sometimes comes, in strumous patients, a tedious sinus, although with them, immediately after a wound, suppuration seems as active as in the most healthy and robust. I think it not impossible that we may arrive at a drainage-tube of absorbable material which may meet our wants. Those at present in use last too long and needlessly delay the complete closure of the

wound. My house-surgeon, Mr Wilson, has devised a tube of plaited catgut, which I think will meet our requirements. It can be made of any thickness or thinness, of material which will quickly disappear or of lasting character. If made with a large lumen it permits the escape of fluids as easily as an indiarubber tube of the same calibre, and for the same reason, and because of its loose texture, the granulations eat it up more easily and quickly than an ordinary skene of catgut or a decalcified bone. It will not last sufficiently to serve in a septic case. Then we must use indiarubber or glass.

I have wished to make these observations because I formerly left too much out of account the rare cases in which evil arises from want of drainage, and because, of course, the result of any method of wound treatment must be thereby modified.

But to return to our subject. The place to be held by the various germicides in the treatment of septic wounds is a question of great interest, and which has been by no means decided. Which is best for this purpose? In what form is it to be applied? Is the application to be constant or intermittent? Are they to be used with a curative or palliative object in view? Is the possibility of further contamination to be prevented by dressings? These and a thousand other questions are constantly pressing, and have not yet been adequately considered. I hope to return to them. Meantime I confine myself in this paper to the treatment of recent wounds.

I need not recapitulate here the arguments which led me to prefer a dry absorbent and elastic dressing, which retains its antiseptic properties for an indefinite period. Nor need I detain you with a *résumé* of laboratory experiments, which went to show that the spray, as spray, was not germicide. I take these for granted.

Fortunately, during an operation a wound becomes contaminated far more frequently by contact than through the air. The surgeon's fingers, instruments, coat-sleeves, blankets, towels, sponges, such are the sources of putrefactive mischief.

First, then, it is essential that whatever may come in

contact with the wound should be absolutely pure. With the necessary precautions for this purpose surgeons are familiar, as well as with the importance of attention to the minutest detail. What is needed in the laboratory to keep a beaker of fluid or a gelatine dish aseptic is needed also for a wound. A little matter, but not without its importance, is the surgeon's comfort in this respect. Strong solutions of antiseptics injure the skin of the hands and interfere with the delicacy of touch. I have, on the whole, found it most satisfactory to wash first with a solution of carbonate of soda, and then smear the hands with a solution of carbolic acid in glycerine. The glycerine preserves the skin soft and pliable.

But given the well-known precautions as to hands, instruments, patient's skin, etc., we have also to consider how best to protect the wound from atmospheric contamination.

This resolves itself into two periods, viz., during and after the operation.

There is reason to believe that during the operation the chances of contamination are comparatively small. On a surface three inches in diameter there will sometimes no germ light in fifteen minutes, and to be certain of a successful cultivation about twenty minutes' exposure of such a surface is necessary. Now, healthy tissue forms, in some respects, an unsuitable soil for the growth of organisms. I have often thought that we might regard the constituents of the human body much in the same light as Darwin regarded living things in general. There is a survival of the fittest, a choking off of the weakest. Every tissue is always trying to enlarge its boundaries, and their territories are settled by a true balance of power. Disturb the balance by strengthening one, or weakening another, and the territorial limit is altered accordingly. The history of tumour growths, of grafting, whether of skin or bone or malignant disease, is an illustration of the law. The eating up of dead bone, of blood, of catgut, of sponge, is yet another example. And so, whether or not, when microbes are brought in contact with the interior of the human body, they will increase or die, must depend on their relative vital power. If, on the one hand,

they are very numerous or powerfully vitalised, or if, on the other, the tissues with which they struggle are naturally weak or artificially devitalised, the microzymes will propagate themselves. Otherwise they will not. Were it not for this, there would in the old days have been no possibility of healing by the first intention, and yet it was common enough when the wound was small, and not rare even when it was large.

The tissues, then, are germicide. Is it possible to trust to this in our ordinary surgical practice? It is proved by the experience of many surgeons that we may do so in abdominal surgery. A woman, æt. 21, was sent to me by Dr Angus Macdonald, suffering from pyo-nephritis. The tumour had begun eight months before, inflammation spreading up from the bladder during the last weeks of a pregnancy. It was now of large size, reaching past the middle line of the abdomen. I operated by the median incision, and had no difficulty in removing the enlarged kidney. No antiseptics whatever were used to protect the wound during the operation, although, of course, the most scrupulous care in dressing and in all other respects was observed. It is certain that sepsis did not occur, because a glass drainage-tube was kept in for three days, and the teaspoonful of serum, which was twice a day sucked up through it, was always carefully examined. The wound was healed in a fortnight, and the patient was out of bed in three weeks. The sequel of the case is so interesting that it seems worthy of a digression. At the time of the operation I tied the greatly thickened ureter in two places, and divided it low down in the abdomen, between the ligatures. A few drops of pus exuded, which were carefully mopped up, and the cut end touched with carbolic glycerine. Had I correctly estimated the meaning of these drops of pus, viz., that there was communication between the ureter and the pelvis of the kidney, and had I borne in mind that for weeks the urine had been perfectly free from pus, although in the early days of the disease it had been highly purulent, I should have understood that obstruction of the ureter existed below the spot at which I had divided it. The result was that in the

fourth week an abscess slowly enlarged the stump of the ureter, and, although we succeeded in opening it from the vagina, it subsequently formed a communication with the bowel, and the patient died exhausted seven weeks after the operation.

But to return, the surgery of the abdomen shows that although it is certain, from the long continuance of the operation, that germs from the atmosphere must obtain access to the wound, they will not propagate themselves unless they be of peculiar virulence and in unusual numbers. I suppose that no one proposes that in the abdomen we should wash the parts with any antiseptic whatever. The certainty of producing inflammation and the risk of poisoning the patient forbid it. Some surgeons still maintain a fond belief in the spray, and cover the abdomen with a mist of carbolic solution. But their results are not better than those of others who do not. Here, then, in abdominal surgery we have facts which inevitably lead to the conclusion that the use of antiseptics, for the purpose of destroying germs in the atmosphere, or even sometimes after they have lit in the wound, is unnecessary, ineffectual, and even dangerous.

May we then argue from this to ordinary surgical wounds? Is there a difference so great between the surgery of the abdomen and that of the rest of the body that a diverse practice should be followed in the one and in the other? It appears to me that this point is still undecided. I have abandoned the spray in all cases, with what results this paper is designed to show. It is useless and inconvenient. May we also abandon the washing of the wound with an antiseptic. I believe that in the great majority of cases no evil would follow. But I have no doubt also that a certain percentage would become septic which would not otherwise do so. That is the case in the abdomen, spray or no spray. Every now and again the ovariologist has to fight septic inflammation and septic poisoning by drainage, and suction, and ice-caps, and other things. And so it is in general surgery, with a certain additional disadvantage in the fact that the whole surface of the wound is an oozing surface, and that the soil is therefore more suitable for the development of germs.

I cannot, therefore, as yet abandon altogether the application of germicides to the wound. I try to do so now and again in the more simple wounds, and I have not had reason to regret it. I think it may come that we shall operate in an atmosphere rendered sufficiently pure to enable us to do so safely. Meantime, not yet venturing altogether to abandon, we try to moderate the irritating qualities of the antiseptic. With this object in view, I have elected to smear my wounds with a solution of carbolic acid in glycerine. There are two solvents of carbolic acid which mitigate its caustic action—oil and glycerine. After trying the mixture of oil and carbolic acid, I came to the conclusion that the glycerine was in many respects to be preferred. As every one knows, glycerine is itself little prone to putrefaction, and although there are rare organisms which find it a suitable soil, animal substances may be kept in it without decomposing. Oil, on the other hand, parting quickly with the acid, may easily become rancid. Again, glycerine is mixable with water, an advantage of no slight practical importance. Its adhesive property, moreover, at once maintains it on the surface of the wound, and is calculated to catch and retain any organisms which may alight, and so prevent them from easily finding their way into the recesses. I have found that glycerine alone is sufficient in minor wounds, but as a rule, and to make assurance doubly sure, I mix it with carbolic acid. I have generally used one part of carbolic acid to twelve of glycerine, but a solution of one to twenty is, at the same time, perfectly antiseptic, and even more free from caustic and irritating properties. I expected that the attraction of glycerine for water would produce a very copious flow of serum from the wound, but it has turned out that there is not so great an exudation as when the watery solution of carbolic acid has been used. For ordinary wounds it is quite sufficient, the skin having been rendered aseptic, to smear the solution over the surface at the end of the operation; but in wounds in which cavities are laid open, in which there are many recesses, or in which the depth is great as compared with the breadth, it is better to pour on a little from time to time during the operation. If the

operation be rendered bloodless, the cavity of the wound may be sometimes filled with glycerine, and the operation be yet carried on with perfect ease under the translucent fluid. With all antiseptics it is essential that they be not introduced under pressure, especially if the tissues be already bruised. I shall presently show that even with glycerine this may prove injurious. If the case remain aseptic it causes a painful cellulitis; if it become septic it inevitably leads to sloughing. It is less injurious, however, than either chloride of zinc or the watery solution of carbolic acid under similar circumstances.

Let me enumerate then, shortly, the modifications of the Listerian precautions which I am in the habit of using. Those which I do not mention I adopt as they are usually carried out. It is well to wash the hands and the skin of the part to be operated on with a solution of carbonate of soda, and then smear thoroughly with the glycerine solution. After the operation, and during it if it be prolonged or complicated, the wound is also covered with the glycerine. Oiled silk, simple or dextrinised (it matters little which), is soaked in carbolic glycerine, and laid on the closed wound. The object of this is chiefly to prevent adhesion of the outer dressings. The whole is then covered with a large quantity of salicylic wool, a freshly torn surface being placed next the skin. If on account of oozing the dressing requires renewal next day, the drainage-tube is then removed; if not, its removal may be delayed for ten days or a fortnight.

I have now pursued this method for nearly a year. It has been modified at different times by using other antiseptics, as bichloride of mercury, thymol, eucalyptol, and others, but I have not been able to assure myself that any one is less irritating or more efficacious as a germicide.

Statistics have, no doubt, a certain value in determining the usefulness of different methods of dressing. But the conditions are so complicated that the record of individual cases is undoubtedly of greater importance.

In the first place, then, I propose placing before you evidence that the method is effective in the various classes of

cases which antiseptic surgery has more especially rendered feasible and safe.

Take, first, examples of simple osteotomy. Peter Horner, æt. 25, came to the Infirmary on 26th June 1883, on account of knock-knee. Eighteen months before admission he had been operated upon in an English provincial hospital, by removal of a wedge of bone from the inner condyle of the right femur. There was now a sinus which had not healed since the operation, and which led to carious bone. The limb operated on was inverted and bowed, the knee and ankle joints were stiff, and the foot rigidly pointed. An advantage of the carbolic glycerine to which I have not yet referred is its power to correct septicity. I look upon it in this respect as superior to chloride of zinc, and much less irritating if it fail to render the part at once aseptic. In this case the carious bone having been freely exposed was gouged out, and the part being drenched with glycerine at each dressing speedily became aseptic, and after a few weeks healed. The left leg which had been left untouched was now operated upon. It appears to me certain that Mr Macewen's plan of operation for knock-knee is greatly to be preferred to all its numerous rivals in respect of simplicity, safety, and the subsequent comeliness and usefulness of the limb. The only modification of it which is an improvement is the placing of the incision on the outer instead of the inner side of the limb. If the wound be over the outer condyle, pressure may be applied on the inner side to keep the limb straight, and much less chiselling and force is required to divide the bone. It may be objected that a gap is left at the outer edge of the divided bone. The answer is, that such is the case to a certain extent with both methods, that the union of the fracture never fails, and that what slight lengthening there may be of the limb is not inconvenient. By this plan, then, I operated on Peter Horner. The wound was dressed as has been described, and the limb was placed at rest. I am in the habit of using in knock-knee an apparatus which I find useful both for rectifying the deformity without operation, and also after section of the bone. The double long splint is used with the tie rod at the foot, and

extension is applied to the limb by weight and pulley. This last aids in diminishing the pressure on the outer condyle, and so allowing its more rapid growth in the child. In the adult after section it tends to steady, wonderfully mitigates the pain of any slight movement, and helps to maintain the proper straightness. But the chief difficulty in the treatment of knock-knee by apparatus is to keep the leg in apposition to the splint. By the slightest eversion the limb eludes the straightening force whether that be rigid or elastic. A little experiment will at once show how this may be avoided. If a long splint be applied to the outer side of a knock-knee, and pressure made to draw the knee to the splint, it will be found that this is easily accomplished, but only because the knee has become slightly everted and flexed, while the heel has separated from the splint. If the heel be held firmly applied to the splint it is impossible to make the knee touch it. I therefore fix the heel by attaching to the lower part of the long splint a little suitably padded wooden trough.

In Peter Horner's case it was impossible to use the double splint, because of the distortion of the right leg. In other respects the apparatus was applied as described. At the end of a fortnight the wound, which had been left open without a drainage-tube, was found to be healed when looked at for the first time. A jointed support was subsequently fitted to his left leg, while a high-heeled boot was adjusted to his right, and he went out after fourteen weeks, walking well with the aid of a stick, a feat which had long been impossible to him.

I have selected this case for narration simply because of its intrinsic interest, not because it presented any peculiarity in regard to its successful result. All the cases of a similar character have done equally well, viz., two other operations for knock-knee, two of section of the tibia for deformed union of old fracture, one of resection for overgrowth of bone in a stump of the upper arm. They were all healed when the first dressing was removed.

Wounds of joints are important from a septic point of view. If they go wrong, the joint is spoiled and the patient in great risk. I have operated once for badly united

fracture of the patella of old standing since I abandoned the spray. Considering the good results that are easily to be obtained by other treatment, and the evil effects that may follow the slightest error on the part of the surgeon or carelessness on the part of the patient, I think it unjustifiable to suture in recent fracture. When the limb is rendered useless by wide separation of the fragments it is far otherwise. The operation, however, is then difficult. In this case, by section of the quadriceps extensor, I was able to reduce the gap from $2\frac{1}{2}$ inches to half an inch. The wound was dressed the next day on account of serous oozing, when the drainage-tube (inserted in front only) was removed. It was not again seen till it was healed. Of the same character is a case in which I incised the elbow on account of synovitis just become purulent. The temperature immediately fell, the pain disappeared, and the only dressing required was for removal of the drainage-tube a fortnight after the operation.

A very good illustration of another class, that of compound fracture, is found in one of the fibula opening into the ankle joint. The tibial malleolus was also broken. My house-surgeon, Mr Wilson, therefore made an opening on the inner side, passed a drainage-tube through the ankle, washed out with carbolic glycerine, and dressed with salicylic wool. I removed the tube next day, the retentive apparatus was kept on for three weeks, the dressings for a week longer, and the patient left in six weeks with a perfectly movable joint.

A class of case which tests the value of any proceeding for maintaining a part aseptic is the opening of chronic abscesses. A psoas abscess, from caries of the spine, was recently brought to a successful termination after about six or seven dressings at long intervals, and the efficacy of the plan is all the better illustrated that the treatment was carried out by Mr Hodsdon during my absence from town in the autumn. There are at present in the Infirmary two cases of empyema. In both the treatment has been, so far as it has gone, absolutely successful. The wounds are still open, and dressed at intervals, but the temperature is

normal, and the pus scanty, pure, and sweet. In such cases the patient is directed to hold the breath while the dressing is changed. Formerly, while I had faith in the spray, I used often to allow the air to whistle in and out of the opening for considerable periods, while examining the patient or demonstrating to students. Most of my cases became septic, with increase of temperature and pus formation, as one would now anticipate, considering the inefficacy of carbolic spray as a germicide.

I cannot conclude this list of cases without referring to one of which, doubtless, the fatal termination is not far off, but which illustrates the safety of the method. The patient, a young woman of 26, had suffered for a year from sarcoma of the skull. It was nearly two inches in diameter, and so round and well defined that by some who saw it, it was regarded as a cyst. It had produced double optic neuritis and intense headache, but no other cerebral symptom whatever. I determined to expose it, and ascertain if it were removable. I found that at isolated spots, over an area of five inches, little portions of the skull were softened by sarcomatous tissue emerging from the *dipl e*. I closed the wound, and five days afterwards, because the patient was anxious to return home as soon as possible, we removed the dressing, and found that healing was practically accomplished.

But lest I become tedious, I close this history of successful cases. They are sufficient to establish the proposition that the method is effective in maintaining parts aseptic if properly carried out.

I have still a word to say as to failures. The comparative frequency of these often means simply the idiosyncrasy of individuals concerned in the management of the cases, and, indeed, there are very different records in this respect throughout my Infirmary experience. I lay little stress, therefore, on their number as the test of a method of treatment, but when analysed they may yet afford information.

I have first to record four deaths among patients treated by this plan. These were cases of trephining, of nephrectomy, of double amputation, and of amputation at the elbow. The

compound fracture of the skull died in six days of extensive cerebral injury, as discovered on post-mortem examination, when, also, the wound was found almost healed and perfectly aseptic. The nephrectomy has already been referred to. The amputation of thigh and leg died the same night from his internal injuries.

The amputation of the elbow is the only one which can be placed to the discredit of the treatment. It was performed in a woman, æt. 72, for sarcoma of the ulna. The wound immediately became septic, a considerable amount of cellulitis followed, and the patient died on the twenty-ninth day, worn out by the pain, inflammation, and discharge. The lesson to be drawn from it is one which may be enforced by two other cases in which, although recovery ensued, the local accidents were identical. One was a double amputation, tarso-metatarsal on the one side, below the knee on the other. The foot healed with one dressing, but the leg, although making an admirable stump in the end, was attacked by sloughing cellulitis. At the time of the operation I observed, too late, that a serious error had been committed by one of the assistants, which of necessity conveyed septic material to the wound. This I endeavoured to remedy afterwards by injecting it forcibly with carbolic glycerine after the sutures had been inserted. It is curious how slowly a lesson of this sort is learned. I had seen the same result in other hands with other antiseptics, and I had injected with a like effect in the amputation of the elbow for a precisely similar reason. Of course I had done it in others without mischance. The sloughing occurs if the injection be unsuccessful. The combined effect of septicity and antiseptics is too much for any tissue. The third case was similar. A compound Pott's fracture of the fibula with opening of the ankle joint was injected with carbolic glycerine. In this case it was not the force used, but the absence of a drainage-tube, which led to cellulitis. It has recovered well, though slowly, but the antiseptics, too long kept in contact with the texture, did harm when the part became septic. This case contrasted strongly with the other compound fracture into the ankle joint, which I have already

related, and which happened to be under treatment at the same time.

A certain number of other cases became septic, as judged by the odour, the discoloration of the protective, and other signs. These were an excision of the testicle and a hernia from displacement of dressings by the patients, one excision of the mamma, and one excision of the knee. Altogether seven cases became evidently septic,—a proportion in my experience of Infirmary work as small as by other methods.

If we judge by microscopic examination of the discharges, the proportion in which bacteria or micrococci could be found was very much smaller than under the gauze and mackintosh. I do not attribute this to any more powerful germicide properties in the dressing; I believe it to be due to the fact that a far larger number of the cases are absolutely dry, and thus afford no possible soil for development. It appears to me, however, a fact of great significance, that, notwithstanding the absence of the spray, fewer cases are scientifically septic.

I append a table of all the major operations, in cases with unbroken skin, which I have personally performed in the Infirmary, and treated by this plan, since I finally abandoned the spray. I find that, including these and all others, during the past twelve months I have performed forty-one major amputations and excisions, and seventy-nine other operations of a like magnitude, on patients in the wards of the Infirmary, besides many other minor amputations and operations involving a breach of surface. Of these six have died,—

- 1 excision of the tongue—from septicæmia.
- 1 lithotomy—from erysipelas of the head, to which he was very liable.
- 1 nephrectomy.
- 1 colotomy—of diarrhœa, nine weeks afterwards.
- 1 amputation of leg and thigh—the same night.
- 1 amputation of elbow—of sloughing cellulitis.

I mention these figures because they imply a not unhealthy condition of the wards generally during the period involved in the tables. I have already said that no great reliance is to be placed on statistics in a matter of treatment; but I have

endeavoured to give such particulars of the individual cases as may, to a certain extent, enable one to judge of their value, without making them unnecessarily bulky.

There are some points which I should have liked to bring out more clearly. Thus, instead of the date of dismissal I should have preferred to give the day on which the wound was found healed. This is, of course, much earlier, as a patient is rarely sent out the moment a wound has closed. Again, I should have liked in each case to give the precise number of dressings; but these matters have not always been accurately recorded in the case-books.

It will be seen, however, that of the fourteen amputations six, of the seven excisions two, and of the thirty-two other operations fifteen, were only dressed once. When two, three, or four dressings are recorded, they were performed for the removal of the drainage-tube, for early oozing, sometimes simply from curiosity. It is impossible to exaggerate the importance of this infrequent dressing as adding to the safety, comfort, and rapidity of healing.

Four deaths have occurred. One only has connection with the mode of dressing, and I have already commented on it. The deaths from trephining, nephrectomy, and double amputation must have happened, whatever the mode of dressing. I might legitimately have added to the tables six cases of compound fracture, all of whom recovered.

I claim that these cases confirm the statements made in a former paper as deductions from experiments with the spray, and show that, from an antiseptic point of view, there is no appreciable difference whether an operation be performed with or without it; and that, from every other point of view, these alterations in wound treatment are greatly for the better.

[TABLE OF MAJOR OPERATIONS.]

TABLE OF MAJOR OPERATIONS.

No.	Name.	Nature of Operation.	Age.	Date of Operation.	Date of Dismissal.	Cured.	Died.	Remarks.
1	R. L.	Myxoma of abdominal wall	8	15th Nov. 1883	1st Dec. 1883	1	0	One dressing. Tumour size of hen's egg. Wound healed without pus, but phlebitis of other leg delayed dismissal. A blood cyst under the hamstrings.
2	J. S.	Cyst of thigh	37	13th Nov. 1883	18th Dec. 1883	1	0	One dressing.
3	J. R.	Large cyst of scalp	50	22nd Oct. 1883	15th Nov. 1883	1	0	One dressing.
4	M. M. K.	Excision of patellar bursa	22	22nd Oct. 1883	15th Nov. 1883	1	0	One dressing.
5	A. S.	Sarcoma of skull	26	29th Jan. 1884	5th Feb. 1884	1	0	One dressing. Tumour not removed.
6	M. M.	Fatty tumour in each groin	47	12th Feb. 1884	Still in Ward	1	0	One dressing on right side. Aseptic slough on left.
7	M. S.	Large fatty tumour of buttock	30	21st Feb. 1884	Still in Ward	1	0	Six dressings. Now healed. Aseptic. Drainage difficult from decubitus.
8	H. D.	Sarcoma under sterno-mastoid	56	19th Feb. 1884	5th March 1884	1	0	Two dressings. Size of goose egg. Drainage-tube of plaited catgut absorbed by second dressing. Fourteen days.
9	G. H.	Lymphoma of neck	24	1st May 1883	19th May 1883	1	0	One dressing.
10	J. R.	Adenoma of mamma	28	15th Jan. 1884	31st Jan. 1884	1	0	One dressing.
11	A. S.	Scirrhus of mamma	47	6th April 1883	19th April 1883	1	0	Two dressings.
12	M. T.	Scirrhus of mamma	73	17th May 1883	7th June 1883	1	0	Aseptic.
13	E. S.	Scirrhus of mamma	45	15th May 1883	2nd June 1883	1	0	Aseptic.
14	B. M.	Scirrhus of mamma	42	22nd May 1883	14th July 1883	1	0	Aseptic. Extensive operations. Glands in axilla.
15	H. H.	Scirrhus of mamma	56	12th June 1883	18th July 1883	1	0	Became septic, but healed well.
16	A. H.	Scirrhus of mamma	53	10th July 1883	1st Sept. 1883	1	0	Became septic. Skin deficient.
17	J. D. S.	Trephining, for compound fracture	66	5th Dec. 1883	10th Dec. 1883	0	1	One dressing. Died of cerebral injuries.
18	P. H.	Trephining, for compound fracture	52	2nd Jan. 1884	24th Jan. 1884	1	0	One dressing.
19	G. B.	Resection of humerus in stump	17	17th Jan. 1884	29th Jan. 1884	1	0	One dressing.
20	P. T.	Section of tibia, for deformed union	46	7th Jan. 1884	31st Jan. 1884	1	0	One dressing.
21	M. M.	Incision of tibia, for osteitis	33	14th Jan. 1884	22nd Feb. 1884	1	0	Two dressings. No pus found, but inflammation cured.
22	J. N.	Section of tibia, for deformed union	36	28th Feb. 1884	Still in Ward	1	0	Not yet dressed. Is perfectly well.
23	J. T.	Section of radius, for osteoma	18	4th Jan. 1884	11th Feb. 1884	1	0	Three dressings.
24	A. C.	Incision of elbow	18	19th Feb. 1884	Still in Ward	1	0	Has not been dressed. Temp. normal. Free from pain.
25	M. S.	Femoral hernia	45	9th May 1883	9th June 1883	1	0	Aseptic.
26	M. M.	Femoral hernia	65	13th Feb. 1884	3rd March 1884	1	0	Became septic from displacement of dressings, but healed well.
27	J. D.	Femoral hernia	57	6th June 1883	1st July 1883	1	0	Aseptic course.

28	J. B.	Nephrectomy	22	14th June 1883	31st July 1883	0	1	Case related in paper.
29	S. R.	Excision of testicle	15	26th Feb. 1884	Still in Ward	1	0	Dressings were displaced by patient, but healing nearly accomplished.
30	P. H.	Knock-knee	27	26th June 1883	16th Oct. 1883	1	0	Referred to in paper. One dressing.
31	M. T.	Knock-knee	14	27th Feb. 1883	26th Mar. 1883	1	0	One dressing, one side; other treated by apparatus.
32	W. M.	Knock-knee	16	18th Mar. 1883	16th April 1883	1	0	One dressing, double.

MAJOR AMPUTATIONS.								
1	J. H.	Thigh, for dislocation of knee	28	22nd May 1883	7th July 1883	1	0	Four dressings.
2	P. M.	Thigh, for dislocation of knee	21	29th May 1883	5th July 1883	1	0	Two dressings.
3	A. C.	Thigh, for gangrene	62	29th Nov. 1883	23rd Feb. 1884	1	0	One dressing. Healed in fourteen days.
4	M. M.	Thigh, for dislocation of knee	17	31st May 1883	10th July 1883	1	0	Arteries were like pipe stems.
5	J. M.	Thigh and leg. Railway injury	50	13th June 1883	..	0	1	Aseptic. Healed in three weeks.
6	J. S.	Leg, for ununited fracture	4	19th June 1883	11th July 1883	1	0	Died the same night. Shock and internal injuries.
7	M. S.	Ankle, for strumous disease	15	6th April 1883	9th May 1883	1	0	Aseptic. Recurrent disease after excision three years before.
8	M. M.	Ankle, for strumous disease	32	12th June 1883	11th July 1883	1	0	Aseptic. Eight dressings. Surface slow of healing.
9	A. D.	Ankle, from gunshot wound	17	17th Nov. 1883	9th Feb. 1883	1	0	Small portion of injured flap sloughed.
10	R. W.	Leg and foot	42	9th Jan. 1884	Still in Ward.	1	0	Quite healed. Referred to in paper.
11	J. J.	Arm, for burn	23	3rd July 1883	8th Aug. 1883	1	0	One dressing.
12	A. S.	Elbow, for sarcoma	72	17th July 1883	14th Aug. 1883	0	1	Died from diffuse cellulitis.
13	J. H.	Forearm, for injury	36	24th Oct. 1883	17th Nov. 1883	1	0	One dressing. Found healed 9th Nov.
14	A. D.	Forearm, for injury	22	6th Feb. 1883	Still in Ward.	1	0	Found healed on 16th Feb. First dressing. No drainage.

EXCISIONS.

1	M. W.	Excision of knee	16	31st May 1883	11th Sept. 1883	1	0	Became septic. Amputation by Dr Maclaren, 11th Sept., for recurrence of bone disease, sinus never having healed.
2	J. B.	Excision of knee	17	24th May 1883	6th July 1883	1	0	Four dressings. Pressure ulcer from splint delayed.
3	J. M.	Excision of knee	18	18th Dec. 1883	Still in Ward	1	0	Wound healed in seventeen days. Now encased in plaster.
4	G. L.	Excision of elbow	17	8th Jan. 1883	7th Feb. 1883	1	0	Five dressings.
5	R. W.	Excision of elbow	9	29th Jan. 1884	Still in Ward	1	0	Healed by first intention.
6	W. M.	Excision of wrist	12	22nd Jan. 1884	29th Feb. 1884	1	0	Dressed only 6th, 15th, and 29th Feb.
7	E. D.	Excision of wrist	15	12th Feb. 1884	Still in Ward	1	0	Doing well. Referred to in paper.

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