Address in surgery : delivered at the Annual Meeting of the British Medical Association, held at Birmingham, August 8th, 1872 / by Oliver Pemberton.

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THE ANNUAL MEETING

OF

THE BRITISH MEDICAL ASSOCIATION,

HELD AT BIRMINGHAM, AUGUST 8TH, 1872.

BY

OLIVER PEMBERTON,

SURGEON TO THE BIRMINGHAM GENERAL HOSPITAL;
PROFESSOR OF SURGERY IN QUEEN'S COLLEGE;
AND FOREIGN CORRESPONDING MEMBER OF THE SOCIETY OF SURGERY OF PARIS.

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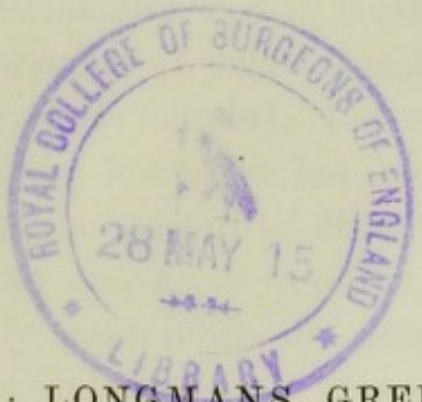

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ADDRESS.

It has been customary to leave to individual selection the subject of this Annual Address. In the exercise of this unfettered discretion, some of my predecessors have devoted themselves to the consideration of a particular subject on which they were entitled—either from the circumstance of original research, or the advantage of practical knowledge—to speak with authority; whilst others have directed their attention to reviewing the progress of surgery with all its bearings on the science and art of their time.

It may be that those who followed the latter course placed themselves more in harmony with the original plan of this undertaking; but it must not be forgotten that during the number of years that have elapsed since this Association was founded, the entire scope and character of medical literature has been changed.

In these days the scientific discoveries of Europe, Asia, and America are, as it were, on our very tables, and within speedy reach of us all: surely it would seem then but a repetition of an old story to arrange them in order before you.

One of the happiest events, however, connected with these meetings provides an escape from any difficulty in the selection, from any sameness in the character of the discourse. They are held, in turn, in England, Scotland, and Ireland. The great cities of these kingdoms are so many centres of surgical study, having features marked and distinctive, created no less by local than by personal circumstances. For I take it, that it will be conceded that whilst the great principles are well nigh everywhere the same, no art differs by its exercise so widely, whether in force, completeness, or success, as ours does, in varying hands, or when practised in places removed by distance from each other. Hence whoever happens to be designated to fulfil this most honourable task, may be expected to possess some special experiences which his opportunities have afforded him in the place of assemblage—some illustrations of vexed questions in surgical treatment that may arouse interest in an audience even as critical and accomplished as that I have before me at this moment.

In this hope, and with a full knowledge of how illustrious are the names that have preceded me in accomplishing this task, I ask your indulgent consideration, whilst I endeavour to justify the confidence reposed in me by the Council of this Association, and by my professional brethren, as I submit to your judgment the fruits of personal observation connected with one or more of the chief maladies that demand the skill of the surgeon.

To solicit your attention to some moot points connected with the treatment of aneurism, is sufficiently appropriate in itself, if we only consider the increasing interest that

at present surrounds it, and the impetus that must be given to its study by the exhaustive nature of the lectures now in course of delivery before the College, by Professor Holmes; but I cannot forget that we are assembled in Birmingham, where Freer and Hodgson lived and wrote—illustrious pair!—well fitted to be teacher and pupil. I cannot forget that George Freer* was the first to tie successfully the external iliac artery, and that Joseph Hodgson† paved the way to a clearer perception of the pathology of arteries and veins than had existed since the days of Hunter. So it is an appropriate tribute to their labours, that here, in this place, I should have the subject of aneurism foremost in my mind.

Professor Lister's improvement in the Hunterian operation, by which the permanent closure of the artery at the spot tied can be insured, without dividing the coats of the vessel, at once effects a complete change in some of the most important conclusions that for long years have guided us in our treatment of aneurism.

One of the greatest dangers attending the Hunterian operation has hitherto been considered to be the application of the ligature immediately beyond any considerable branch of an artery. This impression has deterred surgeons from applying a ligature to that portion of the artery which otherwise would have seemed to them best adapted for the purpose.

That an abiding coagulum will form under certain circumstances in the vicinity of almost any number of branches on the proximal side of a ligature, I am perfectly

* 4th October, 1806.

† *A Treatise on the Diseases of Arteries and Veins.* London, 1815.

satisfied; but the attainment of this success in many cases depends on a fact which it is almost impossible for the surgeon to estimate beforehand; that is, the facility with which the blood will coagulate or deposit its fibrin in any particular instance. In the case I am about to relate, the existence of this tendency, in a much greater degree than usual, was the main cause of the success of the operation.

In June, 1870, I saw F., 60. Strumous from childhood and highly intellectual. There was an aneurism, three months old, of the left superficial femoral, about five inches below Poupart's ligament. There were marked indications of general arterial disease, and, during the past twelve months, of semi-paralytic seizures, evidently due to temporary hindrance to the cerebral circulation. Pressure failed and was abandoned at the end of three months. I then advised ligature—the aneurism rapidly increasing. On October 12th, the aneurism having "leaked" the previous day, in the presence and with the sanction of Sir James Paget, I tied the common femoral with a hempen ligature. Fearing, from the diseased state of his arteries, the worst results, as the ligature might be thrown off, the wound was left completely open, being simply covered for protection. In forty days the ligature came away, and the patient died a week afterwards, from causes wholly removed from the aneurism. There was no hæmorrhage, but gangrene may be said to have commenced in one toe. When I came to dissect the arteries I found the circumflex ilii, epigastric, and the profunda femoris given off together, that is at opposite points of a line drawn round the main trunk, a little above Poupart's ligament, the point of ligature

being five-eighths of an inch below these vessels, the part of artery intervening being firmly plugged.

The profunda was pervious for some distance, and then plugged.

There were plugs of former date in the right and left carotids and in the left axillary arteries.

The aneurismal sac was filled by broken down clot and fluid blood, and would have suppurated.

Now I cannot consider this case as any contribution to the surgery of the common femoral, though I hoped and expected it would be so; for I think hardly any doubt can be entertained but that the patient would have died from secondary hæmorrhage on the separation of the ligature, had it not been for the unusual tendency of the blood to coagulate, for nothing could well be closer than those three vessels were to its site. At the same time I think, with Porter and Macnamara,* that the exclusion of the common femoral, on account of the assumed liability to secondary hæmorrhage and gangrene that follows its ligation, is not founded on any solid basis. It was selected in this instance in preference to the external iliac, and I should so select it again; of course, on the presumption that I was going to place my ligature in the vicinity only of such branches as the epigastric and circumflex ilii; in the presence of which I feel satisfied that an adequate coagulum may under ordinarily favourable circumstances be reasonably expected to form.

It is rather here then I draw your particular attention to this feature of coagulation. For I ask myself this question. If I had tied the artery—complicated as its

* On Ligature of Common Femoral. *British Medical Journal*, ii, 1867, p. 285.

irregularity was in this instance—with a hempen ligature, and there had not existed this tendency, would it have held? Assuredly not.

This same tendency to coagulation, as manifested in the collateral vessels, whilst it preserved the patient's life, would, I may observe, have subsequently caused his death by gangrene.

Further, it appears to me that in proportion as the coats of the arteries deviate from their natural state, and as the general vital conditions are lowered, so is this tendency increased.

Surgically speaking, then, this very circumstance of diseased arteries and tendency to blood coagulation, in some cases, establishes a reason why the surgeon should not refrain from operating where the diseased condition of the coats of the arteries alone would lead him to abstain from interference. And it is clearly of the utmost importance to increase this tendency to the coagulation of the blood as much as possible, after all operations for aneurism, by good diet, and by the absence of all depressing remedial agents.

Apart from this question of coagulation, I feel warranted in expressing my conviction that too much stress has been laid on the disturbing influence of a large branch or branches taking origin close to the part of the vessel tied.

If, however, we are to believe the teaching of Professor Lister,* it will be of little moment in the future whether a plug form on either the proximal or distal side of the ligature at all, so long as the "prepared catgut" insures permanent closure of the vessel at the spot tied, without

* *Observations on Ligature of Arteries.* Edinburgh, 1869.

severance of the coats, and, consequently, without liability to secondary hemorrhage.

From what I have seen since this case came under my notice, I am glad, before such a meeting, to be able to express my unbounded admiration of and confidence in the use of the animal ligature, as placed before us by Professor Lister.

If the so-called "antiseptic system" has effected no more for surgery than to give us the means of effectually closing an artery without cutting it through, and without suppuration, it has in doing this placed the crowning glory on the treatment of aneurism, for which it has waited since the time of Hunter.

For forty days I watched patiently for the detachment of this ligature with an open wound to escape deep-seated suppuration.

I watched twenty-one days in a case last May, in which I tied the external iliac for aneurism at Poupart's ligament; and though the case did perfectly well, the suppuration about the track of the hempen cord gave me the greatest anxiety.

Last August, Mr. Lund,* at Manchester, tied the same artery with a catgut ligature. No pus was secreted—the wound healed on the eighth day!

Professor Lister remarks that ligature of the innominate must yet prove, with these means at command, "a very safe procedure." Yes; but always, I presume, provided that the coats of the artery are healthy where the ligature is applied; and on this we await further evidence: given this, and I feel inclined to echo the assurance.

I shall now endeavour to show that the principles of

* *Lancet*, August 5, 1871.

treatment in the methods of flexion, compression of the sac, and manipulation, are one and the same.

The method of flexion can only be applicable to certain arteries. All that it is needful to do is to keep the limb flexed, not continuously, but to such an extent as to alter the relations between the orifices of ingress and egress, and the fibrinous laminæ of the sac.

Some of these laminæ become, as it were, dislocated, and protrude more or less into the stream when a fresh deposit of fibrin occurs, and so the cure is gradually effected.

The exercise of pressure on the artery above the angle of flexion appears to me useless. What we want is a stream of blood flowing into the aneurism; that it should be more or less retarded there, and that there should be present something in the nature of a foreign body; for example, the fibrinous laminæ, on which blood would coagulate and deposit its fibrin.

This retardation of the blood in the sac can be effected by a gentle compression of the artery on the distal side of the aneurism; as I strongly hold that what we want in these cases is a deposition of fibrin rather than a coagulation of blood.

For, surely, the slow deposition, layer after layer, of solid fibrin in the sac, until the filling-in is complete, is a surer guarantee against subsequent mishaps than if it were closed by a mass of suddenly coagulated blood.

I place before you the case of K., a Lascar, 22, who came under my care May 10, 1859, having an aneurism of the left popliteal artery of four months' duration.

Flexion was maintained for seventy-two hours, with the

result of the aneurism ceasing to pulsate, and becoming solid in eleven.

Compression, during the first forty-eight hours, of moderate character was also made by Weiss's instrument on the artery in Scarpa's space.

I believe that this was the first instance on record in which the combination of compression and flexion was made use of; and the case was published very fully at the time in the *Lancet*.* But I feel satisfied now, in reviewing it, that pressure below the sac would have proved more advantageous than above.

If one wanted convincing of the very slight means whereby important curative changes may be brought about in aneurismal sacs, I have but to ask a consideration of the facts connected with the following cure, in thirty-nine hours, of a popliteal aneurism.

M., 28, a porter, came under my care on the 17th January, 1871, with an aneurism of the right popliteal artery five months old.

On the 22nd I had shown the patient how I proposed to treat him—by flexion—at the same time bending his leg, and arresting the circulation through the sac.

It is very likely that the patient, in the afternoon, subsequent to my visit, imitated my proceeding, as he seemed very much struck by what I had done.

Be this, however, as it may, I only bent the limb once, and fingered the sac lightly, and told him to keep quiet.

At two a.m. on the 23rd he was seized with sudden aching, shooting, gnawing pain in the neighbourhood

* Vol. vii, 1859, p. 232.

of the aneurism, extending down the calf of the leg as far as the ankle, and upwards as far as the crest of the ilium. The pain continued very intense for some hours, during which he had no sleep. He had severe nausea, but no vomiting, and was chilly. The pulsation in the aneurism seemed unchanged, but he remained poorly all the day.

24th.—Slept until three a.m., when he awoke and found pulsation had ceased in the aneurism. The limb was cold below the knee, and œdematous, and though the pain of yesterday was better it was not gone.

On examining the aneurism it felt hard, and was free from pulsation. He was subsequently discharged cured.

I am inclined to think that coagulation commenced here after the first flexion—that the deposit of coagulum, after a few hours, increased rapidly, as indicated by pain which culminated in intensity as the sac was solidified.

There may have been a clot detached which plugged up either the proximal or the distal orifice—if so, it took place at the final exacerbation of pain, when pulsation ceased—for I certainly judged the contents, when I bent the limb, to be fluid.

The verification of the intense pain that occurs, when the blood current is forced suddenly into narrow collaterals, or at the supreme moment when the contents of the sac are solidified, was here very conspicuous.

To ask, for a few minutes, the judgment of a skilled assembly, such as this, on the probability of applying

* *Lancet*; vol. i, 1872, p. 272: Discussion on Aneurism, Clinical Society of London.

pressure with success to the actual sac of an aneurism, would appear to be returning to the pre-Hunterian period; and yet had the theory by which aneurisms are cured now, been fully comprehended then, I can have little doubt but that the necessary deposition of fibrin would have been brought about, in many instances, that otherwise so signally failed.

I entertain the opinion that compression of the sac ought to be used more frequently than it is now. The principle of this proceeding is exactly the same as flexion; we want simply to alter the relations of the laminated fibrin to the cavity of the aneurism, so as to bring about a further deposition of fibrin on the projecting surfaces of any of the displaced laminæ. The pressure need not be continuous. It should be very gentle. It need not, even, be distributed uniformly. But it must ever be born in mind, that if it be carried to such an extent as to empty the sac, and to press one wall against the other, then a cure cannot occur.

The very conditions under which a cure is possible are here ignored.

Blood must pass through the sac. It must not pass through too rapidly; and I now think that this would be facilitated by gentle pressure being made on the artery below the aneurism.

In March, 1857, I visited D., 67, an active sportsman and farmer. He had an aneurism, a month old, of the right external iliac artery, just above Poupart's ligament. It was somewhat fusiform in shape, measuring three inches in length, by one and a half in breadth.

There was no bruit. I failed to arrest the circulation

through the aneurism by making pressure on the artery above, but I found I could greatly limit the current, by making pressures on the sac itself.

The question being raised of an operation, I advised—that as a ligature could hardly be applied to a sound artery, other than in the course of the common iliac, and bearing in mind the vicinity of the bifurcation, no operation should now be performed; but that Dr. Carte's compressor, under careful management, should be applied to the sac itself.

Accordingly under my directions two senior students* remained with the patient. The treatment extended over a period of six weeks. During the first fortnight little good was effected. Then, for eight consecutive days pressure was applied, on an average of seven and a half hours per day. The pressure effectually retarded the circulation—nothing more—and was never applied for more than three or four hours at a time. Consequently, the patient was not wearied, and had undisturbed nights.

There was now a complete interval of nine days, during which no pressure was applied, the aneurism being firmer and with less pulsation, and he was allowed to move about in his room.

Then followed nine days of treatment, averaging six hours and a half per day. From this time the aneurism ceased to pulsate, and the patient gradually resumed his ordinary avocations.

He is yet living, in his eighty-third year, active and well. In December last† the remains of the aneurism

* The late Mr. Dennis Moore, of Walsall, and Dr. Neal, of Birmingham.

† Letter from Mr. Earlam, of Abbotts Bromley.

were represented by an indurated enlargement, about the size of a chestnut.

I venture to think that even Dr. Macnamara will give me credit for having studied O'Bryen Bellingham* to some purpose, who, happily for my patient, published his observations in the January of this very year.

Reduce the force and volume of the blood current by any carefully-considered measures, and we follow out the reasoning of Brasdor and Wardrop in the distal ligature; a reasoning, which is rendering amenable to treatment internal aneurisms hitherto beyond surgery; a reasoning that has the authority of nature's own proceedings to recommend it, from the fact that it is more or less identical with the mode in which the so-called spontaneous cures are brought about.

I own, this case—unique as it is in this situation—has always been in my mind, on the discovery of any fresh instance of aneurism. It serves to confirm the soundness of the remark, that in proportion as the true method of curing aneurisms has been fully understood—that is, the gradual lessening of the blood current to final and complete coagulation in the sac—so have the means whereby this has been brought about, become simpler and more safe.

The occurrence of these cases led me to the attentive consideration of the facts, as they are at present before us, connected with the purposed displacement of the contents of the sac of an aneurism, in the hope of plugging up either outlet: and the case of M. happened when a subclavian aneurism† was under my notice in

* *Observations on Aneurism.* London, 1847.

† Pollock records three cases of direct compression successful to subclavian aneurisms.—*Grey's Reports*, vol. xvi, p. 63.

another ward, about the treatment of which I was—to say the least of it—previously undecided.

It seemed to me so impossible to limit the degree of force, short of extreme hazard, in the method originated by our distinguished *confrére*, Sir William Fergusson;* and I was not reassured by perusing the cases he has recorded, or those of Mr. Pollock.†

I cannot but regard the treatment by manipulation, to be based on exactly similar principles to those on which the methods I have just alluded to are founded. No forcible pressure to detach fibrinous laminæ, in my judgment, ought to be used; as the result would be the almost certain separation of small portions of the clots, which would be carried into the circulation, and would eventually plug the smaller vessels, causing symptoms according to the functions of the parts which the plugged vessels supply. For I must own I have not been able to see how these clots could be located at either outlet to be fixed by arrangement, as it were, at a spot where it is simply impossible to be assured that they could effect a lodgment.

All that is necessary is, that the aneurism should be gently manipulated, so that the laminæ of fibrin in its interior should occupy a different position to that which they had previously held with reference to the two orifices of the sac; and in order that the blood should not be allowed to pass out of the sac too freely, if I have an opportunity, I shall endeavour to compress the distal artery in accordance with the principles I have been advocating.

* *Medico-Chirurgical Transactions*, vol. xl, 1857.

† *Op. Cit.*, p. 45.

B., 32, admitted in Hospital October 20th, 1870, a former Life-guardsman, with aneurism of three months' duration, in second and third parts of right subclavian. It was as large as a hen's egg, and accompanied by bruit, and by dilatation of axillary. He stayed a month, during which time iodide of potassium was freely given; and then, frightened at the idea of an operation, suddenly left.

January 20th, 1871.—Admitted again. The aneurism seemed firmer, giving me the impression that fibrin had been deposited.

From February 10th to 14th. The sac was manipulated, night and morning, for a few minutes. The proceeding was one of the utmost gentleness and regularity, and consisted in making pressure with the thumb and finger, so as to slightly approximate its fibrinous walls, and whilst thus limiting the circulation, probably rendering the clot surface somewhat irregular, and promoting the tendency to deposit already commenced.

On the 11th pulsation much weaker.

On the 14th it ceased altogether. There was no brachial or radial pulse below the aneurism; and he complained of pain, extending downwards from the aneurism to the iliac region. This was constant, severe, and of a numbing character. There was no constitutional disturbance.

During the following days the collateral circulation about the clavicle and shoulder developed itself; and especially noticeable was a large transversalis colli crossing over the aneurismal sac.

April 17th.—Left for the Sanatorium, the sac being small and hard.

Since this time he has pursued his business as a hawker, travelling all over England. I examined him, and so did my colleagues, as recently as the 19th of June, sixteen months after the cure of the aneurism, and I noted the following facts: a little induration marks the site of the aneurism; the axillary artery and upper part of brachial can be felt, cord-like, as far as insertion of coraco-brachialis—here pulsation begins, and can be felt, feebly, in the radial at the wrist.

He is himself excessively thin, but well and hearty.

The absence in this case of any symptoms of paralysis during the four days in which the sac was manipulated, justifies the inference that no clot at least passed into the cerebral circulation, and I entertain myself the conviction that the cure was brought about by a steady process of lamination, rather than by the detachment of a clot, happy enough to fit into either orifice of the aneurism.

I have now to call your attention to what I believe to be a not uncommon result of the cure of aneurism, after it has been effected for some time; I mean the formation of varicose aneurism or aneurismal varix.

I shall first relate two cases.

In 1844, my late colleague, Mr. Amphlett, tied the superficial femoral for an aneurism of the artery as it enters Hunter's canal. The patient was forty-one, and a soldier.

There was nothing unusual at the operation, and the ligature was thrown off on the nineteenth day. Ten days subsequently there was arterial hæmorrhage from the seat of ligature. This recurred again in ten days, and a third time in fourteen. Pressure on the arch was used, and the patient recovered.

He remained well for upwards of three years, when a tumour formed at the seat of operation, which was evidently an arterio-venous aneurism. With this coming under the care of my colleague, Mr. Baker (our President), he died from a drunken pleurisy, just five years from the date of the operation.

I was fortunate in being able to dissect his vessels. The femoral artery had formed an aneurism at the seat of the operation as large as a hen's egg, and the femoral vein communicated with the artery by a large opening.

The former aneurism was cured, and the artery between it and the seat of ligature was impervious.

M., 50, a soldier, syphilitic and intemperate, became the subject of an aneurism of the upper part of the right posterior tibial artery. Pressure was made over the artery on the pubic arch, for three weeks; on it, below, for nine months. The aneurism was cured. Ten months afterwards an arterio-venous communication formed at the chief seat of pressure, and the patient died in about a year and eight months subsequently. I had the opportunity of minutely examining his body, and I came to the conclusion that pressure on the pubes induced varix in the femoral vein, at the situation of the saphenous opening: that this subsequently sustaining pressure, enlarged and became adherent to the artery, and finally, by a succession of changes, readily comprehended between a diseased artery and a diseased vein, adherent to each other, ended in establishing an aneurismal varix.

At the time when I published this case, which was done at considerable length in the *Transactions of the Medico-Chirurgical Society* for the year 1861, I attributed the results merely to the injurious effects of long-con-

tinued instrumental pressure, producing morbid changes around and between the contiguous vein and artery; and I was inclined to quarrel with Bellingham's* remark, "That no morbid change of any kind takes place in the artery or vein at the site of instrumental compression." Subsequent reflection, however, and an attentive study of the preparations, have led me to seek a different explanation. Thus, in consequence of the diseased conditions and diminished elasticity of the arterial walls, which almost constantly exist in all arteries above the seat of an aneurism, the velocity of the blood movement is considerably retarded. It is evident that this retardation must be materially increased by the obstruction at the seat of ligature, or the seat of long-continued instrumental compression. The immediate consequence of this retarded velocity is an increase in lateral pressure.

This dilates the diseased coats of the artery into an aneurism, which—by the usual processes of pressure—absorption and disintegration opens into an adjoining vein.

Sometimes, as in the second case I have adduced, the opening is effected directly into the vein without the previous formation of an aneurism—and the vein subsequently becomes varicose by the passage of arterial blood into it.

It is now with regret that I must leave this subject and pass on to another.

Up to the early part of the year 1868 I had invariably performed the lateral operation of lithotomy in all cases and at all ages, and had encountered that good and ill luck that always, sooner or later, disturbs the statistics

* *Medico-Chirurgical Transactions*, p. 12.

of the most successful or the most unfortunate of operators who take all that come before them. I had reached my sixtieth case. Of this number, thirty-five were under twenty years, and but one had died from the operation—a child of four, a complicated case, with stone both in bladder and urethra.

Of the remaining twenty-five, four had died at 79, 59, 56, and 53. Surgically, there was nothing to be regretted concerning these, save their deaths; for about them all were conditions favouring a bad rather than a good ending—conditions, that happen with stone, at all times, and to all people, and will happen again:—so I need not touch on them further now.

In all this I had used the single knife and the laterally grooved staff, and I did not think then I should use any other instruments as long as I continued to operate. But what happened?

I cut a boy of eleven, and extracted an ordinary-sized stone, without, apparently, complication of any kind. Hæmorrhage followed, of such severity that it was with difficulty controlled, and death from its immediate effects took place within twenty-four hours of the operation. I could find nothing in the *post mortem* to account for the bleeding. It is true he had but one kidney, but that had nothing to do with his death. It was clear the cutting did it somewhere, and I naturally thought I should like in the future to extract such a stone as the one I then removed, with less of the cutting in the dark, if possible.

Accordingly, I turned to median lithotomy. Allarton, who practised near here, had delivered a lecture on the subject of lithotomy simplified, in the Sydenham College

in 1854, and I was much struck by his arguments as to the capability of dilating the neck of the bladder without incision. I felt satisfied that for bladders within reach of the finger, and for small stones, if not for all, here should be escape from the fear of uncontrollable hæmorrhage, though I had never tested his proceeding in the living subject.

Since, I have had an experience of twenty-five cases.

Of these, twenty have been of ages from two to twenty years, with not a single death.

Of the five, of ages from forty-six to sixty, one died of peritonitis.

I perform it in the following way:

I open the urethra, just behind the bulb, on a centrally grooved staff. Along this I pass into the bladder a tapering probe-headed gorget, withdraw the staff, and taking hold of the handle of the gorget, pass the finger gently along its groove into the bladder, letting it dilate as it goes, and so take the place of the receding guide.

Now, I know that many believe that this dilatation means laceration. It has, indeed, not very long since been described as "complete rupture and laceration,"* and as "unsurgical and dangerous."

I answer this statement, which I venture to characterize as eminently reckless, by a positive contradiction as to facts.

I have cut enough cases by the ordinary lateral method, and am still cutting them, to know the difference between laceration and dilatation. I am quite satisfied that my finger passes into the bladder without laceration in the operation I have described. I feel the entrance gradually

* Mr. Tæyan: *Lancet*, vol. ii, 1870, p. 237.

narrow itself into a mere ring, which encircles and equally grasps my finger on all sides. Through this ring pass the forceps; through this ring passes the stone, and any number of stones, or any fragments of stones; and there is no laceration, except the stone is too large for the proceeding, or violence has been used.

I hold that it cannot be necessary to make incisions to the same extent to remove a body the size of a pea, as we should make for one the size of twenty; and, further, my conviction is, that whilst in all ordinary dangers, as compared with the lateral it is simply equal, in two it is immeasurably less.

These are the dangers from hæmorrhage and from prolonged recovery.

The danger from hæmorrhage in the median can only arise from a central wound of the bulb. It is not unlikely to take place, and did occur to me once in the twenty cases, and was readily arrested; for it must be borne in mind the source cannot be far away, and so happily cannot require the use of that most hateful of all appliances, a plug for the wound.

As to recovery, the exception is to find any constitutional disturbance at all. The water is retained and passed naturally. I would like to ask if any one ever knew a patient pass his water naturally with a "lacerated" neck of bladder? and the wound is healed mostly within a fortnight.

I shall be prepared for it to be said of my advocacy of median lithotomy—"The statistics of your own cases are against you." My answer is, "Statistics are not everything." A case may end just as successfully one way as another, though the troubles on the journey

differ widely, and no one will question but that lateral lithotomy in children is eminently successful. But every operator who has sufficiently tried any given two methods of procedure, has a right to say which of the two he prefers; and therefore it is that I say, when I reflect on the anxiety that I endured in watching the threatenings of mischief in children cut by the lateral operation, I rejoice that I have cause for it no longer, notwithstanding the general good fortune that attended my practice with that method.

And now as to the cases where the median operation should not be selected.

In any instance where the finger is not likely to reach the bladder, so that instrumental dilatation would be required, the lateral operation should be preferred. The reason I use my finger is because I have more control over it than over an instrument. I can regulate the one, not the other.

I would sooner cut than lacerate at any time, and I consider the use of instrumental dilatation in this operation means laceration. You may use it, on and off, with impunity, but it is a most destructive instrument—reviving all the dangers of the discarded Marian.

I attribute the peritonitis, which carried off my single fatal case, solely to the laceration of the neck of the bladder that of necessity followed its use.

I repeat, the only dilator must be the finger, and so long as the neck of the bladder can be widened by this sufficiently to allow of the removal of a stone without laceration, I shall deem it a part of my duty to advocate the adoption of this form of median lithotomy.

I hope, however, my observations will not be misunder-

stood. I am second to none in admiring what Cheseldon practised, and what Liston and Fergusson have brought to perfection—the lateral operation for stone. I have been surrounded during the whole of my professional life by teachers and colleagues who have had unusual opportunities for practice, and who have realized brilliant successes in this very operation; but, in my opinion, it is not the most desirable operation to perform for all stones, at any age and under any circumstances, as some would have us believe.

I now desire to say something about stricture of the urethra.

It is to me remarkable, but it is true, that the views entertained by the highest surgical authorities of the day, differ on no subject so widely as on the particular system they adopt and recommend in the treatment of stricture.

Simple dilatation and rest, I am thankful to say, have had a great following, and, if I mistake not, will yet rise into higher position.

The main quarrel is between the advocates of internal as opposed to external division. The late Professor Syme* thought he had effectually put an end to the use of those "dreadful engines," as he termed M. Reybards' instruments; but he was mistaken, for strictures of this day are both cut, split, and torn; and new engines for the purpose multiply, as if the great surgeon had never lived to speak of plunges in the dark with caustic, or of ripping open the urethra by internal section.

Stricture may fairly be defined to be a diminution of the normal diameter of any portion of the urethral canal; and as it must be admitted that the existence of any

* On *Stricture of Urethra*, p. 21, 1855.

stricture, however slight, from whatever cause proceeding, and of whatever nature, may sooner or later give rise to serious consequences in the condition of either the bladder or kidneys, it is needful for the surgeon to discover it and cure it as soon as possible.

But the real question is in reference to this word cure. Have we to deal with a simple stricture that has resulted from inflammation of the lining membrane of the urethral canal, or with a stricture, originally of this kind, which has been aggravated and increased in extent by ill-considered surgical proceedings?

For the first there is a cure by simple dilatation.

For the second there properly is no cure.

Once organic stricture, always organic stricture, is my belief.

Whenever the lining membrane of the urethra has been injured, whether by accident, disease, or by bad surgery, the spot will contract and establish permanent stricture, and I do not believe that the materials constituting such cicatricial narrowings are ever absorbed.

If you endeavour to restore the normal calibre of the urethra under these conditions by ever so well considered a system of dilatation, my opinion is that the contraction will return sooner or later with increased vigour, the natural elasticity of the canal being gone; in other words, dilatation will not effect a cure and never does effect a cure.

But dilatation, if it be well and properly carried out, will protect the patient against the occurrence of those diseases which, dependent on individual health and mode of life, arise either rapidly or slowly in all cases of stricture.

The degree to which it is necessary to carry this may fairly allow of discussion, for I have ever before my mind the conviction that the very means made use of to effect the so-called cure, may become the certain cause of the continuance, and, in many cases, of the increase, of the malady.

I think it will be admitted that the tendency to narrow in cases of stricture, differs very markedly in individuals.

Some may show few signs of change during many years, others, especially those arising from the effects of laceration by direct violence, certainly, surely, and often rapidly increase.

In all cases treatment by dilatation is necessary; but I doubt myself whether it is needful always to endeavour to restore the standard of the canal to the utmost of its original extent.

I believe that there are many cases which admit of being maintained at a standard short of this, depending, however, on the facility with which the contraction yields, and its rate of increase subsequently.

And it must never be forgotten that when once this treatment by dilatation has been commenced—no matter how carefully or how thoroughly it may have been done—it will have to be continued, whether at the hands of the surgeon or of the patient, more or less during life.

For my own part, given time, and I don't believe that there is any stricture through which an instrument cannot be passed by a skilful surgeon.

This being so, treatment by gradual dilatation follows; and in my judgment this should be by the silver catheter, as the safest, simplest, and most certain instrument in

the greatest number of hands yet given to us, *bougie à boule* and *bougie olivaire* notwithstanding.

If the induration be cartilaginous non-dilatable, or if there be fistula, the treatment by external division on a grooved staff should be adopted as speedily as possible.

Entertaining this view of the permanence of the changes established in the urethra by injury or disease, I am not very likely to favour any internal severance of the lining of the canal, whether by Mr. Holt's method of so-called "splitting," or by any form of internal cutting. I believe a wound is produced just as much in the one case as in the other. I regard those methods as artificially inducing the very conditions which I lament should result from almost unavoidable causes; and I further believe that a shut-up wound on the internal face of the lining of the urethra, is attended by dangers, from which an open wound on the outside face is comparatively free.*

I have had occasion to divide the urethra after Professor Syme's method in upwards of thirty cases. In one case only was there a fatal ending, and this from pyæmia.

In no case was there relapse, provided that an instrument was passed from time to time, the frequency of this being determined by individual tendency to re-contraction, once a month to once in three months being about the average, and by this means the calibre of the urethra was without difficulty maintained at its original standard.

All the cases that I have seen, save one, have required this continued resort to dilatation, and will require it, in

* I will, with Sir H. Thompson, admit its use in narrowings at external meatus. *Pathology and Treatment of Stricture.* 3rd edition.

my judgment, more or less, during life. For there is no more a cure by this, than by dilatation or splitting. In the case that did not require it a fistula remained permanently in the perinæum, letting through a little water, the general stream flowing by the urethra, which at the end of twelve years shows no disposition to contract.

If the induration of the urethra, and narrowing, be of such an extent as to preclude the idea of dealing with it by external division, I prefer to tap the bladder by the rectum.

I don't feel inclined, at present, to divide from the bulb to the meatus; and this literally must be the length of an incision in many of these long standing cases, if the entire disease is to be dealt with.

There are numbers of these inveterate cases wholly unsuited to external division; but they are eminently calculated to be dealt with by a method which deviates the course of the water to another channel, in order that rest may heal the fistulæ, and absorb much of that adventitious material blocking up the natural urethra, which can then readily be found, and have a standard established almost without resort to dilatation.

I frankly say that I do not believe that either internal or external division of any urethra will cause the healing of fistulæ in the groin, buttock, and perinæum, where a man makes his urine, as it has been graphically described, like a watering pot.*

Surely relief by the rectum will stand comparison with all the manœuvres that have been suggested from the days of Hunter to Grainger, and from Grainger, who, by

* See Discussion: Medical Society of London: *British Medical Journal*, November, 1879, p. 590.

the bye, belonged to us here, to Gouley and Wheelhouse. I cannot conceive why a patient is to sustain—sometimes for hours together—the distress belonging to hopeless attempts made to trace, in that stage of the disease, an impracticable canal, when the chief cause of the malady—the flow of urine—can be reached and diverted in a moment.

Since Mr. Cock published his views,* now just twenty years ago, I have had many opportunities of seeing the results of this proceeding.

I am able confidently to state that it is wholly free from danger. Indeed, I can scarcely conceive death following as a direct result of the operation. So little fear of the proceeding had one of my patients that he has been tapped at least six times for the relief of fleeting attacks of retention, dependent on a rapidly distended bladder, unable to empty itself in the presence of long standing organic stricture. I have seen him almost within a day or two afterwards as if nothing had occurred. Further no fistula remains, for the opening in the rectum invariably closes after a few weeks.

I have left in the silver canula for three weeks and have not found inconvenience from its presence; indeed, it appears to me that one of the greatest arguments in favour of its adoption exists in the fact of the position of the canula, which, whilst certainly securing the emptying of the bladder, is wholly removed from the urethra. I am strongly myself of opinion that many urinary cases terminate fatally from urethral irritation, set going and kept up by an instrument retained in the canal in its length.

* *Medico-Chirurgical Transactions*, vol. xxxv, p. 153.

Some persons are very tolerant of tied-in catheters, whilst others, dependent on a certain idiosyncrasy, cannot sustain with impunity the simple introduction of an instrument. I saw a case in a young man which all but ended fatally from epileptic convulsions, induced by a first catheter, whilst the single introduction of a lithotrite in a man of seventy-seven to measure a large smooth stone that had been carried with impunity for years, set up such an attack of cystitis that death ensued. I was very much impressed by a case in which a man, suffering from complete paralysis from the bladder downwards, owing to concussion of the spine, had a silver catheter tied in his bladder. He appeared sinking fast, and the most profound irritation of the bladder was established. I directed the water to be drawn every eight hours, and he began from that moment to amend, and ultimately recovered.

Here, doubtless, the true explanation lay not in idiosyncrasy, but in the fact of the existence of disease from the injury.

You may leave an instrument in the bladder for years from the perinuæm, but you cannot do this with impunity and traverse the length of the urethra. Morbid sympathies become excited in connection with the urethra which are not produced by the introduction of instruments into other mucous channels.*

In what I have said I have urged the adoption of tapping by the rectum, as affording assured relief to the most inveterate forms of stricture. And in considering the treatment of this disease, I have hitherto

* *British Medical Journal*, April 2, 1870: Sir J. Paget, Clinical Lecture on Stricture.

limited my observations to cases of stricture of the urethra *per se*, not to those complicated by retention of urine. I must equally urge it, however, as the remedy most reasonable for almost every form of retention.

It is the absolute cure of spasmodic stricture! and if, in any given case arising from this cause, after one good effort has been made to obtain relief by ordinary means, there is no success, it should be carried into effect.

If retention be present with an impermeable urethra from organic stricture, a double necessity supports its selection, whilst I have yet to learn that it is inadmissible in the retention of old people from enlarged prostate.

I know that it can be accomplished in these cases, but of course, not so readily as if the rectum had only its ordinary contents; and I am quite satisfied that far less irritation would be produced in the majority of these diseases, where death so often directly results from the effects of instrumental measures, by the presence, at the most depending part of the bladder, of a harmless tube, calculated to secure the removal of all urine secreted, and thus master that inevitable decomposition which is not overcome by any other method in use, for the simple reason that one and all fail to empty the bladder. If the membranous urethra bulge behind a stricture, or if an abscess opened in the perinæum suggest a ready path to the bladder, by all means let a female catheter effect, through the perinæum, what otherwise, I maintain, can be accomplished by the rectum.

Some years ago I asked the question, "Can the urethral canal be permanently restored whenever any

complete and considerable portion of its length has been entirely destroyed?"* I believe the answer must yet be "No." I had then a boy of sixteen, with at least two inches completely destroyed by burning; and, believing this, I established him with a silver perinæal tube, through which he now (*æt.* 27) passes his water without trouble; but there is nothing in the growth of the parts that tempts me to interfere, for I know the whole circle of the canal must be gone.

I think, however, that if only a streak of mucous membrane lingers about the part, an efficient connection can be re-established even after the lapse of many years.

I saw in the early part of this year a patient, *æt.* 30, who had sustained, eighteen years previously, such a laceration, that two inches below the meatus was a fistulous opening into the urethra, three-quarters of an inch long. The canal was pervious from the meatus to about one inch above the bulb, where was a second small fistula; then, the canal was obliterated for an inch, and a third fistula placed just at the commencement of the membranous division, gave exit to all his water.

For eighteen years, then, the urethra from this spot had been unused.

I first closed the penal fistula, paring the edges, which looked beautifully in apposition for some days, and then came apart as usual.

But by clipping the penis with a self-acting spring, on the principle of the seraphine, the edges were admirably united. Then I laid the lower fistulæ into one, dividing all the hard textures between them, and passed an eight catheter, and for forty-eight hours tied it in the bladder.

* *Lancet*: March 16, 1861, p. 258.

After this I passed nine daily. In a week the patient learned to pass it himself; and at this time the canal is in excellent order, with the introduction once a week.

Now, I don't think the roof of the urethra was destroyed here, and hence the reason of a success which I did not expect.

In reference to the repair of penal and scrotal fistulæ, I would remark that they can alone be treated satisfactorily by the water being drained through the rectum, or, as here, through the perinæum. Even then, union by first intention is a myth; but union can be obtained by keeping the granulating edges together by some artificial means, at the same time that vivacity is maintained in them by the use of the acetum lyttæ.

And now, permit me to indulge the hope that the illustrations I have been enabled to place before you of modes of practice have not been devoid of interest.

The chances are that I shall never again address an audience characterized by qualities that belong to a representative meeting like this.

I ask, therefore, your kindly reading of the motives which have induced me in what I have said to refer mainly to my own experiences.

If I have any regret in having adopted this plan of proceeding, it arises—let me assure you—from the fact that I have been deprived by so doing of the pleasure of referring to the opinions, to the investigations, and to the successes of my colleagues and associates.

The British Medical Association has diffused an experience by these meetings that does away with the idea that the light of Surgery is permitted only to one man, in one place, or to any set of men in any place.

Handed down to us by imperishable traditions—by names that will live as long as the world lasts: greater in its power to save life or limb than at any period of its history: so simple in its teaching—so open in its counsel—that a Tyro may learn and act: Surgery stands proudly with its fellow-sciences in laying claim to having blessed mankind.

And if this be true, can it not claim also to having been the means of diffusing a blessing of a different kind? I mean the engrafting in the hearts of the men who follow its teachings such a spirit of honour and love of truth, that meanness and jealousy cannot live by its side.

I entertain the sentiment—as dear to me as life—that Surgery will cease to be what it is when the dread anxieties that beset its practice are not softened down by the certainty of the knowledge that good fellowship prevails amongst those who may happen for the moment to be judges of what is being done.

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BY THE SAME AUTHOR.

I.

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II.

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