

changes which produce an alteration in the configuration or position of the uterus let me discuss, first of all, the following propositions, which, says Thomas,<sup>1</sup> "present the views upon the subject of versions and flexions which will be found to bear the test of experience." 1. "Versions and flexions of the womb may, but very rarely do, exist without causing any symptoms, for in themselves they do not constitute disease." The logic of this precept is strange. On authority, but not by reason, we are asked to believe that versions and flexions do not constitute disease and that consequently, although they may not produce symptoms, they are nevertheless much more likely to give rise to discomfort and prove troublesome. Are we to infer that the structural changes which result in the production of these displacements do not constitute disease? If not, they must then be normal variations. No process of reasoning will, I fear, lead us to this conclusion. They are most assuredly the result of some derangement, physical or chemical. Judging alone from the manner in which we are recommended to treat these displacements we are compelled to admit that the alteration, whether in configuration or in position, is in reality the disease. By the application of what "they" term a well-adjusted pessary the uterus will be restored to its natural shape and situation. Now everyone will admit that a system which is reasoned is of some value even although it may not be true, whereas haphazard statements are absolutely worthless, and, worse than this, they are misleading. 2. "By interfering with the escape of menstrual blood, by disordering uterine circulation and keeping up hyperæmia, by causing pressure and friction from contact with surrounding parts, and by creating a barrier to the entrance of seminal fluid, they become as a rule of great importance and require special attention." We have no evidence that flexions or versions interfere with the escape of the menstrual blood. Many women suffer more or less during menstruation, and although we may occasionally in such detect some alteration in the configuration or position of the uterus, it does not necessarily follow that the pain is due to an interference with the escape of blood. Frequently we find women with decided flexions and versions who suffer no discomfort whatever during menstruation. We are too apt to jump at conclusions regarding cause and effect, and for this reason statistics are apt to prove fallacious. We have no evidence either that these variations disorder the circulation. Those who assume that the circulation is disturbed forget apparently that they are dealing with organic structures, with tissues which are endowed with a large amount of power of adaptation to circumstances. In the female dugong we have a uterus which presents a peculiar corkscrew shape, but it is perfectly evident that the circulation of the organ is not thereby disordered. If flexion is due to want of tone in the uterus, then the vessels will not receive that amount of support from the tissues and the blood will consequently pass more sluggishly. In this case, however, the circulation is disturbed not by the flexion, but by the cause of the flexion; the disturbance is nevertheless augmented by the configuration or position of the uterus. In the animal and vegetable kingdoms we have abundant proof that mere shape does not interfere with the passage of that fluid which nourishes the tissues. The tendrils and branches of plants perform many gyrations and assume a variety of shapes without the vascular system in any part of its course being necessarily disturbed. Again, the influence of pressure and friction is undoubtedly exaggerated, for everyday we find new growths developing in the pelvis, arising, it may be, from the uterus or ovaries and attaining a great size, and yet the pressure exercised by these produces little or no discomfort unless they become confined, when the increased tension necessarily produces pressure symptoms. It is alleged, too, that flexions and versions are a cause of sterility, as they create a barrier to the entrance of the seminal fluid. If we examine the genital organs of the females of many of the lower animals we shall observe that greater barriers to the entrance of seminal fluid exist naturally in many of them. The spermatozoa appear to experience no difficulty whatever in effecting an entrance into the corkscrew-shaped uterus of the dugong. In the cervix and body of the uterus of the sheep and goat we find groups of laminae presenting the appearance of a number of successive ora tincae, and many no doubt would be disposed to affirm that these irregularities in the canal would hinder the progression of the spermatozoa. A careful study of the various methods whereby

the process of fecundation is carried on in plants and animals and a close examination of the genital tract in mammalia compel us to admit that the spermatozoa and ova are attracted towards each other and that they do not meet and coalesce simply because they happen to be traversing the same passage. It sometimes happens that this elective affinity is absent and fecundation is then impossible. In the case of fishes, for example, it is this elective affinity—this attraction between the ova and spermatozoa—which keeps the species pure; without this influence hybrids would abound. In the case of the ornithorhynchus we find a cervix uteri located on each side of the roof of the uro-genital canal. Each cervix presents two orifices, one—the lower—communicates with the ureter, whilst the other—the upper—leads to the body of the uterus. If, therefore, the process of fertilisation depended simply upon chance we would be justified in asserting because of the structural arrangement that the female ornithorhynchus would seldom become pregnant, for the spermatozoa would tend to enter the first opening which leads to the ureter, and would thus fail in their mission. It is evident, however, that the spermatozoa seldom, if ever, enter this opening, but are attracted towards that opening which leads to the uterus and ovary. In the macropus major—a marsupial animal—we find also a structural arrangement of such a character that if the meeting of the spermatozoa and ova depended purely upon chance the species would run a fair prospect of being speedily extinguished. 3. "Often being the results, as they are sometimes the causes, of uterine and peri-uterine diseases, their treatment should be combined with efforts at the alleviation of these states." There can be no doubt that the displacements of the uterus are the result of intrinsic or extrinsic changes, but I am fully convinced that if greater caution were exercised in treating these malpositions and altered configurations of the uterus many women would be freed from a great deal of suffering, and many who have become invalided in consequence of a too vigorous treatment would pass through life more comfortably.

Gordon-square, W.C.

#### THE

### RADICAL CURE OF URETHRAL STRICTURE.<sup>1</sup>

By W. BRUCE CLARKE, F.R.C.S. ENG.,

SURGEON TO THE WEST LONDON HOSPITAL; ASSISTANT SURGEON TO ST. BARTHOLOMEW'S HOSPITAL; EXAMINER IN SURGERY TO THE UNIVERSITY OF OXFORD.

THOUGH it is universally recognised that strictures of the urethra differ in their situation, cause and arrangement, no attempt has, so far as I am aware, been made to lay down any principles which should guide the surgeon in dealing with these separate varieties. Excluding the traumatic form which, as is so well known, differs from the organic stricture in its extreme intractability, we seem hardly to have advanced much further in our ideas of the causation and pathology of the disease than had John Hunter. He recognised three varieties—*true spasmodic*, *permanent* and *mixed*, and states further and with equal truth that spasm plays an important part in most strictures. Subsequent writers on the question have little to add to this view and the less we stray from these simple statements the greater chance of our getting a clear conception of what urethral stricture is and how to treat it. One point alone we may add to this description to make it complete: we must recognise that congestion plays a very considerable part in many a stricture that comes before us. There is another factor of equal if not of greater importance which remains to be determined before a stricture can be successfully treated. Its exact locality must be determined, and this is doubly necessary, because, for reasons that are not altogether explainable, the locality affected influences both the course which the disease follows and the means which are to be taken for its relief. It is often assumed that the appearances of diseased structures which are so familiar to the student of museums form an all-sufficient basis to enable us to carry out a successful line of treatment. Nothing, I am persuaded, is more untrue and nothing more likely to bring surgery into unmerited disrepute than the acceptance of such a dictum, as some writers have accepted it without any reservation whatsoever. So far as it goes it

<sup>1</sup> Practical Treatise of Diseases of Women, edition 1880, p. 365.

<sup>1</sup> A paper read before the West London Medico-Chirurgical Society, Jan. 8th, 1892.

attempts made by his most dextrous hand at introducing an instrument into the bladder; but no sooner was the patient put on the table and rendered insensible than the No. 8 silver catheter, which had been passed down as far as the stricture, and the point of which was to serve as a guide for the knife, slipped into the bladder and thus rendered a dangerous operation unnecessary." Since my attention has been specially directed to the connexion between spasm and stricture I have, almost as a routine treatment, employed an anæsthetic before beginning to undertake the treatment of a case of urethral stricture, and I have been very much struck by the large number of cases in which the stricture has largely increased in calibre under the influence of an anæsthetic. I think I may say it is almost an invariable rule for strictures to yield somewhat, and many yield to a very considerable extent. Whenever a stricture yields largely to an anæsthetic, it is almost certain, when examined with the urethroscope, to display a considerable amount of congestion, and often patches of granulation tissue are seen in its immediate neighbourhood. Much of this spasmodic irritability will disappear by rest in bed and other well-known appropriate means, but there usually remains a small amount of true spasm which yields only to the anæsthetic. If this residual spasm is to be seen in perfection, it must be looked for in an early and recent case, and not in one in which the cicatricial contraction has reached its practical maximum, and where the muscular tissue surrounding the urethra is incapable of producing further constriction. It is needless to add that such conditions are incapable of demonstration by museum specimens, but they can be readily detected by the methods just referred to. These are the cases which, if early recognised and judiciously treated, will I believe yield a considerable number of real cures. The very earliest of all may possibly be cured by the passage of instruments, whilst the more severe cases will yield to electrolysis, or should if this plan fails be subjected to urethrotomy, and well dilated afterwards. Probably one reason why one seldom sees a real cure after urethrotomy is that dilatation is not practised sufficiently, nor with large enough instruments, but of this more later. It is worth while recording that there is no longer the same rooted objection to the word "cure" in connexion with urethral stricture that there used to be. Mr. Mansell Moullin has published an account of some cases in THE LANCET, May 7th, 1892, in which patients had been treated by various surgeons for stricture and in which a cure in its truest sense resulted. It is not so long ago that such a statement would have almost been hailed as evidence of quackery. Once a stricture always a stricture represented the views of twenty years ago. I have myself come across a fair number of cases in which an undoubted cure has resulted, one, I remember, in which a patient had been originally under treatment for stricture by the elder Coulson, and thirty-five years afterwards I saw him. He assured me he had diligently passed a catheter or bougie (I forget which) most religiously twice a week ever since. He had absolutely no trace of a stricture, and I advised the discontinuance of his instrument. I saw him a year or more later, when I passed a catheter for him in order to be able to assure him that there was no chance of a recurrence, and he was still quite free from constriction of any sort. Now what is the best system to pursue for strictures which are not complicated by spasm, or in which at any rate spasm plays a very insignificant part? Can we hold out any hope of cure? It must be admitted the expectation of cure is not often realised, but it certainly does occur sometimes.

Sir Henry Thompson records a case in which, after a stricture had been three times divided by internal urethrotomy, a cure which remained permanent twelve years later occurred, and other instances of undoubted cure may be found. One thing is certain, and that is if once a stricture is completely divided the question of its ultimate cure depends on the subsequent dilatation, on the mode employed and on the thoroughness with which it is carried out. It has been too long assumed that so inveterate is the tendency in a cicatrix to contract that no human power can prevent it. Such a doctrine has done little but retard our knowledge. That cicatricial tissue has a strong tendency to contract may be readily admitted, but it is a tendency which can be overcome in some instances very readily. Why, we might ask, does not the cicatrix which is formed after a radical cure for hernia go on contracting for ever? If only it did do so the hernia would have no chance of requiring a second operation and patients would be more easily cured than they are at present.

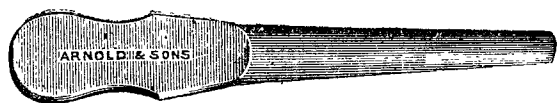
Why do not the scars of old strumous abscesses in the neck, after they have been relieved by subcutaneous section, return to their former condition, and why do not burn cicatrices, the most inveterate of all forms of scar tissue, go on contracting for ever? Simply and solely because treatment is directed towards preventing such a result. Movement of one kind or another is kept up for a sufficient length of time and eventually most of the tendency to recontraction disappears. In the case of the neck this is the more remarkable, because the only possible treatment is rubbing and moving the scar, presumably with the object of preventing the cut bands of scar tissue from rejoining, and there is, as we shall see, some reason for believing that an analogous form of treatment in the case of the urethra will produce in some instances an equally happy result. Rectal strictures of a fibrous nature certainly yield to free division accompanied by persistent dilatation subsequently, and if large bougies are used for about a year, at first daily for ten minutes and after a few months less often, the stricture disappears and shows no tendency to return—a fact which I have been able to verify on several occasions. In some instances I have tried a precisely similar plan with urethral stricture, and with the best results. For this purpose the best instrument that can be employed is a conical steel bougie of as large a size as the urethra will admit. There are very few urethra that will not take with ease a No. 16 English catheter, when the meatus is divided, and in a very large number of patients a No. 18—occasionally No. 20—can be passed without difficulty. Such instruments have the supreme merit of being readily kept clean; they take a high polish and thus pass easily into the bladder, and though at first sight it may sound strange to say so, patients pass them more readily for themselves than almost any other kind of instrument. The reason for this is on reflection quite clear; their weight is such that when passed an inch or two into the penis they glide imperceptibly down the inclined plane of the urethra into the bladder.

I have several cases under observation in which no trace of stricture remains three years after all treatment by dilatation has ceased. But those who have had any experience with hospital patients know how futile it is to expect treatment of this kind to be carried on long and continuously. A few may carry out directions, but the vast majority do not, and in this fact lies the difficulty of the cure. Such experience, however, as I have had of the passage of steel sounds daily, and allowing them to remain in for ten minutes, has led me to believe that dilatation so practised has a far more potent effect even on the very worst strictures than any other plan with which I am acquainted. One man I at present have under observation who was the subject of a very bad ruptured urethra, has been for some months quite free from trouble, while he was so under no other plan of treatment, and at the present time he is beginning to be able to slack off dilatation, because the tendency to recontraction is no longer so marked as it was.

There are classes of cases in which probably there will be no difference of opinion amongst surgeons as to treatment—viz., those cases in which no instrument can be introduced into the bladder. One of two operations must be performed—either that which is associated with the name of Mr. Cock or that which is known as Wheelhouse's operation. The latter is unquestionably tedious, and often attended with considerable difficulty; but the results, both immediate and ultimate, are excellent. Cock's operation, though one of the most rapid methods for the immediate relief of a distended bladder, is by no means altogether free from risk, and often leaves the patient in a worse position almost than he was before the operation, for no attempt is made to promote a continuity between the back and front parts of the urethra; and unless a second operation, somewhat of the nature of a Wheelhouse, is undertaken at a later period, the patient may be compelled to urinate through his perineum for the rest of his days, quite apart from the danger of having his urethra cut completely across, as a result of the first operation for his relief. For my own part I have been inclined to employ more largely than some surgeons the various methods of external urethrotomy from a belief that has been gradually forced upon me that the dangers and evil consequences of relieving a strictured urethra with small instruments, especially when metal ones are made use of, are even greater than they are ordinarily supposed to be. Moreover, though the immediate consequence of an external urethrotomy is often a prolonged and tedious convalescence, yet its ultimate result is with ordinary care both certain and permanent. As an instance of the difficulty which is sometimes met with when

the shorter but more dangerous plan of attempting to pass a small metal instrument by the urethra is carried out, there is a preparation in the Norwich Museum of the bladder and urethra of a surgeon, who had suffered for some years from stricture and for whom a small metal instrument was passed by a well-known London surgeon fifteen years before the patient's death. For fifteen years he never voided his urine except by means of a catheter and after his death it was discovered that the reason for this state of affairs was that a false passage existed, which had left the urethra in front of the stricture and re-entered it in the middle of the prostate. Through this false passage all the urine had been drawn off with a catheter for fifteen years. Nothing probably has tended so much to the better treatment of urethral disease of recent years as the more general employment of rubber instruments in the treatment of small strictures, and it may certainly be laid down as an axiom that whenever a soft guide bougie will pass through a stricture it is capable of treatment by internal urethrotomy, whilst such an occurrence as the one just related will serve to show that however brilliant the passage of a metal instrument may seem to the bystander, the state of the patient afterwards may not be one to be exactly envied. Even if it be granted that success would follow on nine occasions out of ten the surgeon should rather content himself with the more tedious method than court occasional disaster for the sake of a brilliant temporary success.

It has long been recognised and it is now pretty generally admitted that penile strictures demand incision, whilst those more deeply situated are quite amenable to dilatation. It is certainly a curious fact that anterior strictures will not often yield to dilatation, and this is probably due to two facts: (1) The septum pectiniform of the penis is unusually firm and unyielding at its anterior portion; (2) the causes—such as syphilitic sores and very acute inflammations usually of gonorrhoeal origin, which tend to produce anterior strictures, are usually attended with a great deal of inflammation, and thus give rise to strictures of a very unyielding nature. Still the anterior strictures are interesting in other ways. Some of those which are situated within the first inch of the urethra are certainly capable of real cure. In the first place they are visible to the eye, so there can be no dispute about their existence; in the second they can be kept in view whilst they are being divided; and in the third place they can be kept dilated with very large instruments for any period that may be considered to be necessary. If cure be desired in such cases the stricture should be kept dilated for a few minutes daily with a short conical steel bougie for at least six months, after which time the dilatation should be carried on once or twice a week for four or five months more, at the end of which time there will be found to be in most cases (probably in all) but little tendency to recontraction. The accompanying engraving will show the character and shape of the



instrument. Surely if such a result can be obtained with such severe strictures, it should encourage us to hope for cure in those more deeply placed.

With regard to the strictures which are more deeply situated there will probably be much difference of opinion as to how they should best be handled. Many of the slighter ones are so readily amenable to occasional dilatation that few patients are disposed to submit to more prolonged treatment. With the more severe types whenever there is much thickening to be felt from the outside, if there is tendency to congestion, as is shown by their readiness to bleed, or by their admitting instruments of variable size within a short period, no plan has yielded in my hands such good results as electrolysis. I have on more than one occasion published an account of cases with their results several years afterwards, and further experience leads me to assert confidently that in suitable cases excellent results will with care be obtained.

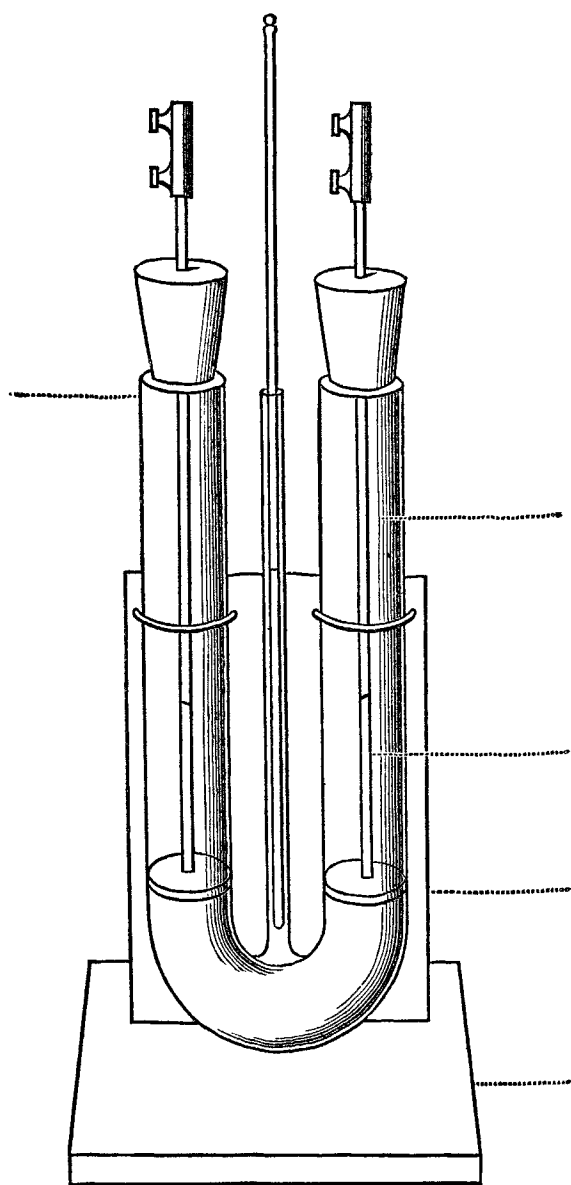
If I were asked to formulate in a few words the most essential points in the treatment of stricture of the urethra I should be inclined to say: 1. Make certain you have a true stricture to deal with. 2. Take the greatest care to keep it dilated for a long time after it has been incised. 3. Where there is much thickening employ electrolysis.

Harley-street, W.

## "THE ELECTRICAL RESISTANCE OF THE URINE AS AN AID IN DIAGNOSIS."

By DAWSON TURNER, B.A., M.D., F.R.C.P. EDIN., &c.,  
PHYSICIAN TO THE LIVINGSTONE MEMORIAL DISPENSARY; LECTURER  
ON MEDICAL ELECTRICITY AT SURGEONS' HALL.

A PRELIMINARY ACCOUNT of part of this research was given in the Proceedings of the Royal Society of Edinburgh on Dec. 21st, 1891, and a further account is being published in the author's "Practical Manual of Medical Electricity." I desire here to give a brief summary of the work and of its chief results. The object of the inquiry was to ascertain the electrical resistances of various kinds of urine, both in states of health and of disease. The measurements were made by means of a Wheatstone's bridge with alternating currents and a telephone according to Kohlrausch's method and at a temperature of 65° F. The engraving is a view of the electrolysis tube used.



It would appear from the observations—some 500 in number—that the specific resistance of a normal urine amount on the average to about 45 ohms, and that it varies as a rule more or less inversely with the specific gravity. When the specific gravity is high and when the urine holds in solution much salts, its electrical resistance is low, and *vice-versa* where the specific gravity is low, the resistance is high. The amount of urea has but little to do with the resistance in ordinary urines. A number of experiments were made with artificial urines (see tables), and from these it is clearly apparent that the electrical resistance depends almost wholly upon the salts, and that it is only when these are quite absent, or very much diminished that the influence of the