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OBSERVATIONS (CLINICAL AND BACTERIOLOGICAL) ON THE CIRCUMSTANCES OF OPERATIVE INTERFERENCE WITH MUCOUS MEMBRANES, WITH REFERENCE SPECIALLY TO THE URETHRA.

By JAMES H. NICOLL, M.B.,

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The results of certain observations which have occupied much of my spare time during the past few years are here given as so many statements of fact verified as fully as the difficulties surrounding them have permitted. No attempt is made to draw final conclusions on any or all of the points raised—the sources of fallacy are too numerous, and often too obscure; and, as explained below, the observations are incomplete, and are still being carried on. No bibliography of the subject is included. In the course of the investigation I had partially compiled one. Two things led me to abandon as useless labour the effort to complete it. These were, in the first place, the contradictoriness of the statements of the various writers, and, in the second, the obviously second-hand order of many of these statements. The observations detailed, therefore, are to be regarded as the expression of an attempt to reach the facts of the subject by an observer as far as possible unbiassed by previous statements. They are not, however, as already stated, given without the knowledge that fallacies may be present. So many sources of fallacy have become apparent that it is highly improbable that there may not remain still others.

The investigation has followed two lines: (1) Into the

possibility of sterilizing by one or other means the various forms of bougie and catheter in use. (2) Into the question of the presence or absence of bacteria in the urethrae, morbid and normal, into which in practice instruments have to be passed.

The results are given in semi-tabulated form. The cases are those of patients examined in the Western Infirmary, the Central Dispensary, the Children's Hospital, and private practice, and my own notes of the cases are much more full than the brief records produced here.

In the work I have been indebted for invaluable help to Mr. F. H. Riley, who has previously, in another matter, given me much assistance.[1]

The *Clinical Research Association* also have most carefully and exhaustively reported on a number of preparations and cultures submitted to them. To Dr. R. M. Buchanan, of the *West of Scotland Clinical Research Laboratory*, and to Dr. L. R. Sutherland, Assistant Professor of Pathology, Glasgow University, and to two other gentlemen whose names are not published, I owe hearty thanks for much valuable help. Finally, Dr. A. R. Ferguson, Assistant Pathologist, Western Infirmary, and Dr. Leslie Buchanan, of the Eye Infirmary, have lately been giving a large proportion of their time and skill to the completion of that part of the investigation which is still in progress. In the face of the very contradictory statements of various writers, it has been my endeavour, by submitting all doubtful matters to one or more of the gentlemen named—all of whom have a wide and accurate knowledge of practical bacteriology—to arrive at results which might be regarded as reliable. I thus feel a degree of confidence in the accuracy of the results stated, which dependence on my personal efforts alone could not have given.

PART I.

With regard to the first part of the investigation—the sterilization of instruments—the following results were obtained:

[1] "The Aetiology and Treatment of Chronic Enlargements of Lymphatic Glands." *Glasgow Medical Journal*, January, 1896.

BOUGIES.

Metal Bougies may, of course, be sterilized by heat. They may also be sterilized by the same processes as are applicable to soft bougies, and observations on this point are quoted below.

Soft Bougies—red, black, and yellow "gum-elastic"—will not stand heating by any method to a temperature sufficient for sterilization. This statement is founded on numerous tests which I have applied to instruments supplied by different firms in Britain, America, France, and Germany, and applies to dry heat, steam, and boiling in water or oil. Occasionally a bougie is encountered which will survive a single not too prolonged boiling or steaming. Such an instrument is an exception, however, and but proves the rule, for repetition of the process is speedily destructive.

Professor Albarran some time ago introduced soft bougies (and catheters) covered with caoutchouc, which are, or were, stated to stand sterilization by boiling. For these I made inquiry through instrument makers in London and Paris, and was informed that they could be made for me, but were not now stocked, as their power of resisting heat was found to be easily exhausted. I have not therefore tested these, being influenced by the considerations that instruments both troublesome to obtain and costly, by reason of being specially made to order, and of short life, were not, whatever their merits otherwise, likely to be much in use. The following observations were made only on the ordinary red, black, and yellow "gum-elastic" bougies of every-day use:

I. *New gum-elastic bougies*—red, black, and yellow—of various shapes and different makers were tested in solutions of carbolic acid and perchloride of mercury.

Soaking for fifteen minutes in carbolic, 1 to 20, or for half an hour in carbolic, 1-40, or for an hour in perchloride of mercury, 1 to 1000, renders the surface of nearly all of them so sticky that the towel adheres in the process of drying, and the bougie becomes covered with fluff. If the bougie be permitted to lie for an hour or two in the air this fluff dries, and may be readily wiped off. After several soakings, however,

the surface becomes permanently dull and sticky and unfit for use. Occasionally a bougie is found which, for a time, will stand such treatment. That is the exception. The majority are affected as stated, and it is obvious that sterilization in this way is unfit for practice. In the first place it is impossible to wait for an hour or more till the bougie is sufficiently dry to be rubbed smooth for use. In the next place, to employ it while still damp and sticky is to sacrifice a bougie, for if there be germs in the urethra they will become embedded in the sticky surface of the bougie in such a way that it is difficult to conceive of their removal or destruction by any process not destructive of the instrument. Finally, even granting that such brief soaking will sterilize a soft bougie (and on this point I have not made sufficiently accurate tests to speak definitely), and that the average bougie may be used twice or thrice after it, the process is too costly for practice. Some other mode of sterilization is therefore necessary. Dr. Schimmelbusch in his *Aseptic Treatment of Wounds* says: "It is important that it should be known that a smooth" (red gum-elastic) "bougie or catheter can be made externally free from germs by mechanical means, rubbing it with a piece of sterilized gauze and warm water."

To test this I made the following experiments:

II. *Series of six soft gum-elastic bougies* which had been in ordinary use for from six to eighteen months. After use in cases of stricture these were washed with tepid water and soap, rinsed in cold running water, and dried with thorough light friction by sterilized gauze. Each bougie was then rubbed on the surface of slightly acid agar, and on the surface of slightly alkaline agar in tubes. *Results*: In one case, in an alkaline tube, on the fourth day, there appeared two small colonies, which proved to be a large white coccus, which was not definitely identified. In another alkaline tube, on the ninth day, there appeared a patch of penicillium. The other ten tubes remained sterile for over a month, the period during which they were under observation.

III. *Series of six soft gum-elastic bougies*:—soiled with pus from acute abscesses, washed with tepid water and soap, rinsed in cold running water, and carefully dried by friction

with sterilized gauze. Three of these were rubbed on alkaline agar, and three were stabbed into neutral gelatine. *Results*: All six tubes remained sterile for a month.

IV. *Series of six soft bougies*:—used in cases of stricture, washed with tepid water and soap, rinsed in cold running water, and dried with soft white towels which had not been sterilized, but which had come fresh from the laundry, where they had been boiled in the course of cleansing. The process of drying was completed by rubbing the instrument firmly from hilt to point several times with a fresh portion of the towel. Each bougie was then rubbed on slightly alkaline agar and on slightly alkaline glycerine agar. *Results*: All the tubes remained sterile for a month.

V. *Series of six steel, plated bougies*:—used in cases of chronic gleet, washed with soap and water, rinsed in cold running water, and dried with towels fresh from the laundry in the manner above described. They were then rubbed on plates of slightly acid gelatine and on plates of slightly alkaline agar. *Results*: Four of the twelve plates developed penicillium. The others remained sterile for a month.

VI. *Series of six soft bougies*:—used in cases of gleet, washed with soap and water, rinsed in running water, and dried with towels which had been sterilized by steam. Each bougie was then rubbed on neutral glycerine agar and stabbed into neutral gelatine. *Results*: One of the glycerine agar tubes yielded a sparse growth, appearing on the fifth day. This was pronounced by one bacteriologist to be bacillus coli, and by another to be proteus vulgaris. The other tubes remained sterile for a month.

In the foregoing experiments the water employed for washing was the ordinary gravitation water of Glasgow. No means were adopted for sterilizing it. The towels were from the laundry of the Western Infirmary and from two private laundries. The oil employed for lubricating was made up after the formula of Lund. The culture media were prepared in the usual way, and were kept previous to use for a time sufficient to exclude chances of impurity.

Conclusions.—It appears clear that ordinary careful washing of a soiled bougie followed by careful drying with gauze or

towel which has been sterilized by steam or boiled may be relied on to render the instrument sterile. The results of the experiments above detailed are practically uniform, for in series V. the fact that four out of the twelve cultures developed penicillium, a non-pathogenic mould, is probably to be explained by the fact that series V., unlike the others, necessarily consisted of plate cultures instead of tubes, plates being much less readily protected from aerial contamination. It seems, therefore, that it is unnecessary to have recourse to the use of carbolic or perchloride lotions which, as stated, rapidly destroy the instruments.

In practice there is a consideration which is of prime importance. Instruments must not be employed after their surface has become broken. Metal bougies on which the plating has become chipped or scratched, and soft bougies with chipped or cracked surface, offer sites for the lodgment of septic material from which it is not easily dislodged without still further destruction of the surface. Metal bougies may be replated. Soft bougies, however, once cracked cannot be repaired, and have to be discarded, and their life is shorter than that of the metal instrument. As indicated, treatment by carbolic or mercurial lotions is peculiarly destructive of them, and, by rendering the surface rough and broken, defeats its own purpose.

CATHETERS.

Metal Catheters, like metal bougies, may be readily sterilized by heat.

Soft Catheters.—The red "*rubber*" catheters, usually termed "Jacques," which are composed of various compounds containing rubber, may be sterilized by boiling or steaming without in any way suffering. They may also be soaked for months in solution of carbolic acid (1 to 20) and perchloride of mercury (1 to 1000) without damage. An india-rubber catheter may also be used daily, and subsequently washed with hot water and soap, and put to soak in carbolic lotion for the remainder of the twenty-four hours for a period of six months without becoming unfit for use. These statements are based on ample actual and carefully noted tests which I have made, but which

need not be here given in detail. Two things are worthy of note. In the first place these statements refer to rubber catheters of a certain quality only. There are in the market catheters of red rubber composition, which may be of as good quality as, or better quality than, the others, but which rapidly deteriorate under repeated boiling. Certain firms supply these, and these only. In the second place *all* rubber catheters ultimately deteriorate to some extent after repeated boiling, the change in some being in the direction of brittleness, in others of pulpiness. Prolonged and repeated soaking in strong antiseptics has very little effect indeed, even when long continued, in producing deterioration.

By actual experiments I have satisfied myself that rubber catheters treated by boiling, efficient steaming, or soaking for four hours in the lotions above-mentioned, are rendered sterile, both internally and externally. As, however, few, if any, will be inclined to doubt this, detailed records may be omitted.

Gum-elastic Catheters—black, red, and yellow—of various forms and from various makers—have been tested as follows:

I. *Soaking in Antiseptic Solutions* of efficient strength.—Gum-elastic catheters are affected by this in the same way as bougies. In short, and without detailing the tests I have carried out, while nearly all varieties will stand brief immersion, no catheter I have been able to obtain will stand lengthened or repeated soaking such as is necessary for efficient sterilization. This method of sterilization is therefore, as in the case of bougies, impracticable.

II. *Soaking in Carbolized Oil.*—This is in no way harmful to gum-elastic catheters. At present I have lying in carbolized oil (1-40) five gum-elastic catheters which have been in it for over three months. They are in no way damaged. As a means, therefore, of preserving new catheters, or catheters previously sterilized, from aerial or other contamination it may be employed. As a means to be relied on for sterilizing soiled catheters, on the other hand, it fails. This is what might be expected in the light of Koch's experimental demonstration of the fact that antiseptic substances dissolved or suspended in oil are practically inert. To test the matter I made the following experiments:

(1) Black, gum-elastic, No. 6 coudé catheter of inferior make, *i.e.* with rough unfinished interior, saturated with pus from a large perityphlitic abscess, and subsequently wiped externally and allowed to dry in the air for six hours. The catheter was then placed, *end-down*, in carbolized oil (1-20), care having been taken that the oil had thoroughly displaced the air from the interior. It remained there eight days, at the end of which time it was removed on to sterilized gauze, and by means of a sterilized knife and forceps completely bisected longitudinally. The halves were then by means of the forceps thoroughly rinsed in warm water which had been sterilized by boiling. One half was then pressed face-down on the surface of neutral agar in a wide tube, and the other embedded in neutral gelatine. *Result*: The agar medium yielded a growth of bacillus coli and a small coccus. The gelatine yielded a growth of the same coccus. It may be observed that, on washing off the oil with the sterilized warm water, beads of dried pus could be seen plentifully adherent to the interior of the catheter, quite undissolved and unpenetrated by the oil.

(2) Red varnished catheter, No. 10, passed in a case of old prostatic cystitis and tied in for over four days. The instrument was then rinsed in warm water, wiped externally, and placed, end-down, in carbolized oil. After sixteen days it was removed and treated exactly as was the catheter in experiment No. 1. *Result*: Both halves yielded on slightly alkaline agar a mixed growth which was not fully worked out by subsequent culture, but which contained at least two germs—bacillus coli and a diplococcus. Red varnished catheters are made quite unfinished and rough in the interior.

III. *Turpentine, alcohol, and ether.* Nearly all varieties of soft catheter are singularly tolerant of *turpentine*. I have immersed red, yellow, and black catheters for weeks in turpentine without their sustaining any damage. On the other hand, no soft catheter I have been supplied with will stand the action of *alcohol or ether* for more than a few minutes without softening and cracking of the varnish. As, therefore, alcohol or ether is required for the thorough removal of the turpentine before use, cleansing by this means is impracticable.

IV. *Formol vapour* has lately been tried as a means of

sterilization. Through the kindness of Dr. Kay I some time ago obtained from Paris the most approved form of apparatus for the purpose. In it the catheters (or bougies) are placed on wire network shelves over a tray containing formol, and are shut in an atmosphere of formalin vapour by an air-tight cover for twenty-four hours or longer. With this apparatus I conducted the following tests:

(1) Catheter, No. 7, black olivary, of inferior make, *i.e.* with rough unfinished interior, saturated with pus from abscess of acute cellulitis, and dried for twenty-four hours. This was left in the metal case in the formol vapour (material supplied by a Glasgow firm, and employed according to the directions of two published articles) for forty-eight hours. It was then split lengthwise with a sterilized knife and embedded in slightly alkaline gelatine, in which, within three days, appeared a plentiful growth of staphylococci.

(2) Catheter, No. 10, black, coudé, of inferior quality, tied in for over twenty-four hours in a case of old cystitis from prostatic retention. This was rinsed in warm water and at once placed in the apparatus over formol (material obtained from a London firm) for three days. It was then split, and the internal surface impressed in neutral gelatine, where a mixed growth of bacillus coli and a coccus developed.

(3) Catheter, No. 6, yellow, olivary, well coated internally, repeatedly employed by a patient with old cystitis from stricture, and after use held under the tap and laid away in a towel. This was placed in the apparatus over powdered trioxymethylene (obtained from, and used according to directions supplied by, the makers of the apparatus) and kept there for forty-eight hours. It was then split and embedded in gelatine, and yielded a growth of a coccus which at first appeared to be a staphylococcus, but which subsequently showed evidence of fission in the manner of a diplococcus.

It is not necessary to detail certain other tests which I have carried out with the apparatus, and which have given very similar results. Unless there be some serious source of fallacy in my mode of procedure, there can be but little doubt that as a method of sterilizing really dirty catheters the process is quite unreliable.

The process is the most recently introduced method for sterilizing catheters and bougies. Its original claims to efficiency have been quoted and requoted by various enthusiastic writers. My results have been so absolutely disappointing that I should have felt tolerably certain there must be something wrong with my procedure, and should probably not yet have published these results but for one fact. I have recently found in the *Annals of Surgery*, August, 1896, a summary by Dr. Martin of the most recent experiments of the original manufacturer of the apparatus. The results of these experiments by no means bear out the early and very positive claims advanced for the method, and which have been so widely quoted. They correspond very closely with my own. The final results of the manufacturer's tests are summarized thus: "As the result of many experiments, —— comes to the conclusion that formol and trioxymethylene are admirably adapted to the sterilization of bougies, but that in the case of catheters exposed to twenty-four or forty-eight hours' action there may be failure when small quantities of formol are employed, when the temperature of the surrounding atmosphere is low, or when the attempt is made to sterilize small catheters with very fine lumina or irrigating cystoscopes. It is probable that all these instruments could be thoroughly sterilized by a more prolonged exposure. This, however, is not practicable. It is essential, therefore, . . . not to attempt the sterilization excepting with catheters of large calibre exposed for twenty-four hours, or catheters of fine calibre and simple cystoscopes which have been previously washed for forty-eight hours." The results of my tests would seem to indicate that neither for large nor small catheters is the process of any value; and Dr. Martin, in the article quoted, appears to be essentially of this opinion, for the directions given for the process are that the instruments "*should be carefully washed with soap and water within and without*, and dried as nearly as possible before sterilization . . . then subjected to the vapour of formol for at least twenty-four hours . . . when required for use . . . taken out and immediately *submerged in weak antiseptic solutions*, biniodide of mercury, 1·25,000, answering very well." The italics are mine.

V. *Heat.* No variety of gum-elastic catheter, whether black, red, or yellow, will stand boiling, dry heat, or steaming in an ordinary sterilizer. This statement is based on a number of actual experiments which I have carried out with catheters of different varieties and makers. These experiments need not be be here given in detail. Certain conclusions arrived at, however, may be mentioned. (*a*) Soft catheters are less speedily destroyed by heat than are soft bougies. This may be owing to the tubular form permitting of the escape of hot air and gases from the interior, and thus obviating the rapid blistering of the surface which occurs in bougies. (*b*) Not a few soft catheters will stand boiling or steaming once if not too prolonged. A few will even survive a second, and, at times, a third repetition of the process. Of all forms of soft catheter that which is most tolerant of heat is the old red varnished curved catheter. These observations, however, do not invalidate the general statement that soft catheters are more or less rapidly destroyed by heat applied in any of the three ways specified.

Steaming the Interior of Catheters. Various forms of kettle have lately been introduced, designed to drive steam from the spout through a catheter fixed on it, and thus sterilize the interior of the catheter. Various tests which I have carried out have led me to the conclusions that: (1) Under certain conditions the process is reliable as a means of sterilizing the internal surface of soft catheters. (2) Under only exceptional conditions can the process effect the sterilization of the exterior of a catheter. (3) When efficiently carried out the process tends, sooner or later, to be destructive of the catheter subjected to it.

With reference to the first conclusion arrived at I found that the conditions mentioned are two: (*a*) The *apparatus* must be properly designed. Four firms supplied me each with its own particular form of kettle. All four were similarly tested in the following way: A catheter was accurately fitted on the kettle spout, and in the eye of the catheter was placed a delicate thermometer. Two of the kettles raised the mercury to the boiling point of water, and kept it there indefinitely, as tested by three different thermometers. Of the other two

kettles one failed to raise the column to within 2° of boiling point, and the other failed to get the mercury within 3° of boiling point. The results varied imperceptibly with catheters of different calibre, and with tests of varying duration. (*b*) *Sufficient time* must be allowed for the steam to act. On this point I have made the following tests:

(1) *Black Coudé*, No. 8:—saturated with urine containing bacillus coli and diplococci in prostatic cystitis. The catheter was dried in the air for twelve hours. Its exterior was carefully cleansed with warm water and soap, and then with carbolic solution (1-20), care being taken to prevent anything entering the interior. It was then efficiently steamed for thirty minutes, split and embedded in slightly alkaline gelatine. *Result*: No growth occurred.

(2) *Yellow Coudé*, No. 10:—employed in the same case, and afterwards treated in exactly the same way, and finally efficiently steamed for two minutes, split and embedded in neutral gelatine. *Result*: Sparse growth of bacillus coli in both tubes.

(3) *Red Varnished Catheter*, No. 10:—employed in the same case, and subsequently subjected to the same treatment, and finally efficiently steamed for twenty minutes, split and embedded in neutral gelatine. *Result*: No growth occurred.

(4) *Yellow Coudé*, No. 12:—employed in the same case, subjected to the same after-treatment, and finally steamed for twenty minutes, split and embedded in slightly acid gelatine. *Result*: A colony of diplococci formed in the tube, containing one-half of the catheter. From the position of the growth I could not avoid the conclusion that it originated from the external aspect of the catheter. The necessity for preserving the interiors of these catheters from the action of the soap and water and the carbolic solution rendered the process of sterilizing the exteriors somewhat unsatisfactory.

(5) *Yellow Coudé*, No. 11:—employed in the same case, subjected to the same after-treatment and steamed for one minute, split and embedded in neutral gelatine. *Result*: Both halves yielded a growth of bacillus coli, and in the case of one-half it seemed perfectly clear that it first originated from the external surface.

In the face of this difficulty I attempted to test the matter in another way. A large, No. 16, black, olivary, gum-elastic catheter was fixed on the kettle, and in it were placed in succession six No. 1, black, olivary gum-elastic bougies along with a fine thermometer. The results got were as follows:

(1) Bougie smeared with purulent urine from a case of cystitis due to faecal fistula. (This urine swarmed with bacillus coli, diplococci, and staphylococci.) The bougie was not allowed to dry, but was at once placed in the catheter and efficiently steamed for thirty minutes. It was then removed, allowed to cool under glass, and embedded in neutral gelatine. *Result*: No growth occurred.

(2) Bougie treated in the same way, steamed for one minute, and embedded in neutral gelatine. *Result*: No growth occurred.

(3) Bougie treated in the same way, steamed for three minutes, and embedded in neutral gelatine. *Result*: Growth of bacillus coli, originating at two points, one-and-a-half inches apart.

(4) Bougie treated in the same way, steamed for five minutes, and embedded in neutral gelatine. *Result*: No growth occurred.

(5) Bougie treated in the same way, steamed for ten minutes, and embedded in neutral gelatine. *Result*: No growth occurred.

(6) Bougie treated in the same way, steamed for fifteen minutes, and embedded in neutral gelatine. *Result*: No growth occurred.

A second series of tests was made. In this instance the bougies were roughened by friction with sand-paper, and the material used for soiling them was the pus from a large acute carbuncle of the neck, which contained staphylococci only. The results were as follows:

(1) Bougie steamed for one minute, and embedded in slightly alkaline gelatine. *Result*: Active growth of staphylococci.

(2) Bougie steamed for five minutes, and embedded in neutral gelatine. *Result*: Growth of staphylococci.

(3) Bougie steamed for five minutes, and embedded in neutral gelatine. *Result*: No growth occurred.

(4) Bougie steamed for five minutes, and embedded in slightly alkaline gelatine. *Result*: No growth occurred.

(5) Bougie steamed for ten minutes, and embedded in slightly alkaline gelatine. *Result*: No growth occurred.

(6) Bougie steamed for fifteen minutes, and embedded in neutral gelatine. *Result*: No growth occurred.

From such results as these, while it is difficult to speak dogmatically, it would appear that an exposure of ten to fifteen minutes should be sufficient in most cases. It may be well to add, however, that in the case of a catheter with rough, uncoated interior, which catheter has been tied into a bladder containing septic urine for twenty-four hours, and which has, in consequence, been permeated by septic material through, possibly, half its thickness, such steaming will probably not be efficient in sterilizing it.

With reference to the second conclusion arrived at, viz. that under only exceptional conditions can the process effect the sterilization of the exterior of a catheter, the facts observed were:

(1) In a number of instances with steam passing briskly through the catheter, and the thermometer in the eye registering boiling point, it is possible to hold, for an indefinite time, the catheter in the hand.

(2) In the case of five different varieties of catheter I performed the following test: Against the catheter was placed the bulb of a delicate surface thermometer. The catheter and thermometer were then tightly rolled in many folds of lint, with layers of waterproof intervening, the ends being left open. The catheter was then attached to the kettle and steamed. *Results*: The highest temperature attained, even under these conditions was 209 F. (with a black French coudé).

With reference to the third conclusion arrived at, it may suffice to say that in this process, as in others, the resisting powers of catheters vary greatly. The majority are temporarily softened and rendered more or less dull on the exterior by an exposure of fifteen minutes. Some do not recover, or but partially recover, their stiffness and lustre. With an exposure of twenty to forty minutes many become blistered and distorted, and after one or two repetitions of the process a

number are rendered entirely useless. It is a somewhat striking fact that those varieties which most require sterilizing by steaming stand it best, while those which have least need of it are most easily damaged by it. Those well-made catheters, for instance, in which the interior is coated as well as the exterior, are apt to suffer from even a brief steaming, the internal coating cracking and curling up, while catheters of inferior make with rough uncoated interiors suffer no material damage till the process has continued sufficiently long to affect the external coat. Certain catheters appear to be quite unaffected by steaming. I have in my possession a red varnished catheter and a black gum-elastic one, both of which have been steamed for not less than fifteen minutes on upwards of thirty occasions, and which appear in no way the worse. These, however, are exceptions.

VI. *Washing with soap and water.* The following series of six catheters was treated in the following way:—After use each was washed with warm water and soap, rinsed in fresh warm water, and held under the tap for five minutes by the clock while a full stream of water flowed through. It was then dried with a towel fresh from the laundry, and thoroughly shaken while completely enveloped in the towel, and subsequently placed to dry for twenty-four hours under glass, with precautions against any possible contamination. It was then split lengthwise with a sterilized knife, and the two halves were embedded in neutral gelatine. The results obtained were:

(1) Black, No. 10, coudé, which had been tied in for twenty-four hours in a case of old prostatic cystitis. *Result*: Bacillus coli.

(2) Red varnish, No. 8, catheter, which had been used daily for three months by an old prostatic with putrid urine. *Result*: Cocci arranged as staphylococci and as diplococci.

(3) Catheter coudé, No. 12, passed in case of recent gonorrhoea in patient with old prostatic retention. *Result*: No growth.

(4) Yellow, olivary, No. 10, catheter, tied in for twenty-four hours in case of old stricture and cystitis. *Result*: Bacillus coli.

(5) Black coudé, No. 9, which had been used daily for a week in a case of chronic prostatic retention. *Result*: A small coccus, which was not definitely identified.

(6) Black coudé, No. 6, passed in a case of old prostatic retention with recent haemorrhage. *Result*: Diplococci.

These experiments were amongst the earliest I carried out, and at that time I had not become fully impressed with the difficulties in the way of arriving at reliable conclusions by such tests as the foregoing. A number of tests which I subsequently made gave such varying results that it would serve no useful purpose to reproduce them all in full here. What follows therefore has reference to three methods only of cleansing soft catheters.

VII. *Douching with carbonate of soda solution.* The following series of catheters was, after use, treated thus: Each was washed with warm water and soap, thoroughly rinsed in fresh warm water, repeatedly douched through by means of a sterilized six-ounce syringe with 5 per cent. solution of carbonate of soda used as hot as the hands could bear it, and finally again rinsed in warm water and put to dry for twenty-four hours or longer under glass, with precautions against any possible contamination. Each was then split aseptically, and the halves embedded in neutral gelatine. The results were as follows:

(1) Black, No. 8, olivary catheter, which had been several times passed in a case of cystitis due to gonorrhoea. *Result*: Bacillus coli.

(2) Black, No. 12, coudé, which had been employed for a month twice weekly by a patient suffering from prostatic retention. *Result*: No growth occurred.

(3) Yellow conical, No. 6, catheter, tied in for twelve hours in a case of cystitis from stricture. *Result*: A growth of a diplococcus.

(4) Red varnished, No. 10, catheter, taken from the collection of a medical man who had repeatedly used it in cases of retention from various causes. *Result*: A growth resembling bacillus coli, but subsequently found to be proteus vulgaris.

(5) Yellow, No. 10, coudé, employed as a vesical drain in a case of vesico-vaginal fistula and cystitis for twenty-four hours. *Result*: No growth.

(6) Black, No. 8, coudé, employed daily for over two weeks by a patient with prostatic retention. *Result*: A growth of bacillus coli.

VIII. *Douching with warm soda solution and with carbolic solution.* The following series of six catheters was, after use, treated thus: Each was washed with warm water and soap, rinsed in fresh warm water, repeatedly douched in the manner above described with hot 5 per cent. soda solution, and finally repeatedly douched with 5 per cent. carbolic solution. Each catheter was then enveloped in a towel fresh from the laundry and well shaken, and put to dry under glass for twenty-four hours. It was then split aseptically, and each half was embedded in neutral gelatine. The results obtained were:

(1) Black conical, No. 12, catheter, saturated with pus from a case of acute cellulitis. *Result*: No growth occurred.

(2) Yellow, No. 9, coudé, used daily for over a fortnight in a case of prostatic retention with putrid urine. (*N.B.*—This catheter was kept in boracic solution when not in use.) *Result*: No growth occurred.

(3) Harrison's black elastic-gum "whip" catheter, No. 10, F., which had been passed in a case of old stricture with purulent cystitis. *Result*: No growth occurred.

(4) Black coudé, No. 8, tied in for twenty-four hours in a case of acute retention in a patient with old prostatic obstruction and cystitis. *Result*: Growth of diplococci.

(5) Red varnished, No. 10, catheter, tied in for about eight hours in a case of old prostatic retention and cystitis. *Result*: No growth.

(6) Black olivary, No. 10, catheter, which had been used daily for five days in a case of prostatic retention and cystitis. (*N.B.*—This catheter had been simply washed in warm water, held under the tap, and then wrapped in a clean towel when not in use.) *Result*: No growth occurred.

Several of these catheters were distinctly damaged by the treatment. No. 2, which was coated internally, was cracked and roughened both internally and externally. No. 3 was rendered more or less dull and rough, and No. 6 became permanently soft and more or less dull and rough.

IX. *Douching with soda solution and with perchloride solu-*

tion. The following series of six catheters was treated in every way as was the preceding series, except that for the carbolic solution there was substituted perchloride of mercury solution, 1 to 1000. The results obtained were :

(1) Red varnished, No. 10 catheter, taken from a bladder the seat of cystitis of unknown origin, into which it had been tied for five days. *Result* : A vigorous growth which proved to be a mixture of diplococci and staphylococci. (*N.B.*—On splitting this catheter several small phosphatic concretions were found adherent to its internal surface.)

(2) Black, No. 8, conical catheter, which had been passed four times in two days in a case of acute gonorrhoeal cystitis. *Result* : No growth. (*N.B.*—This catheter had been kept in perchloride solution, 1 to 10,000, when not in use.)

(3) Black coudé, No. 12, used in case of acute retention in old prostatic obstruction and cystitis. *Result* : No growth.

(4) Black coudé, No. 12, which was soaked for twelve hours in the fresh pus from a large foetid abdominal abscess, probably perityphlitic. *Result* : Bacillus coli.

(5) Yellow, No. 10 coudé, employed daily for a week by a patient with old prostatic obstruction and cystitis. *Result* : No growth. (*N.B.*—This catheter was simply washed with soap and water, held under the tap, and wrapt in a clean towel when not in use.)

(6) Yellow, No. 8 coudé, which was new and had been douched with water which had been boiled. During an hour it was used in four different cases of partial retention, all of which presented more or less cystitis, and in all of which regular catheterism was a necessity. After each of the first three cases the catheter was washed with soap and water and held under the tap, and finally dried by shaking while fully enveloped in a fresh towel. *Result* : No growth.

Catheters Nos. 2 and 6 of this series were rendered dull and more or less rough on the surface, and No. 2 was also much softened.

In all the foregoing tests applied to catheters the water employed for washing and making solutions was, unless otherwise specified, the gravitation water of Glasgow without the adoption of any means of sterilization. The oil used for lubri-

cating the instruments before use was unmedicated olive oil sterilized by heat, and employed very sparingly. In several of the culture tests penicillium developed. As, however, this is known to be due to aerial or other accidental and secondary contamination of the media detailed notes of its occurrence have been omitted. In making the culture tests less than half the length of each instrument was employed, in order to avoid the use of excessively long tubes and very large quantities of media. The half tested was in all instances that including the eye.

Conclusions regarding the sterilizing of catheters. In the matter of the sterilizing of bougies the results of the various experiments are clear, and the conclusions arrived at would appear therefore so far definite, and have been formulated above with little, if any, hesitation.

With reference to the sterilizing of catheters this is not so. It is obvious that in the results of the experiments detailed above there are anomalies and a lack of uniformity which render any attempt to formulate general conclusions of comparatively little value. Of the different possible causes of this lack of uniformity several are clear. These are mainly three: (*a*) The kind of catheter tested; (*b*) the previous treatment of the catheter tested; (*c*) the medium inoculated.

(*a*) The kind of catheter tested. The various soft catheters in the market present the widest differences in quality and finish, and particularly in the finish of the interior and of the eye. I have split several specimens of the catheters supplied me by each of upwards of twenty different firms. For various reasons it is undesirable that I should here give specific details of this examination. So much may, however, be said. There are firms whose catheters are what they are guaranteed to be, viz. as well coated and as smooth internally as externally. Such firms, however, are in the minority, and are not confined to any one country. Certain of these firms supply these specially well made catheters alone, others supply them only when specifically asked to do so, while many firms do not supply them at all, whatever their assertions may be. As already mentioned, it would appear that it is in many instances, though not invariably, the case that the more rough and unfinished

the catheter the better does it withstand damage from vigorous methods of sterilization. For instance, the old red varnished curved catheter is, as a rule, the most rough and unfinished soft catheter obtainable. At the same time it will stand repeated steaming internally, somewhat prolonged immersion in strong carbolic or perchloride solution, and even fairly prolonged boiling. On the other hand, highly-finished catheters, the interior of which is as accurately coated and as smooth as the exterior, tend to suffer speedily from internal steaming, immersion in antiseptic solutions, and even vigorous syringing with soda or antiseptics, if at all concentrated.

With reference to the bearing of the finish of the interior on the matter of sterilization by various methods as tested by culture experiments, it is obvious that it may introduce serious error into any attempted generalization. If, for example, in the treatment of a case of purulent cystitis there be used two catheters, the one accurately coated, smooth, and glossy internally, and the other quite unfinished internally, it is evident that the first may be possibly rendered aseptic by washing with warm water and soap and douching with some antiseptic, but that such treatment may prove quite inefficient in respect of the second in which the purulent urine in its passage has soaked into the uncovered silk or linen web forming the skeleton of the instrument.

(*b*) The previous treatment of the catheter tested. A certain number of the tests quoted were carried out with catheters which had been in use in septic cystitic cases for days or weeks. It is obvious that the results obtained by a given method of sterilization carried out in the case of a catheter passed nightly by a prostatic cystitic patient, who carefully washes his catheter and lays it aside in a weak antiseptic solution in the intervals, can form no criterion by which to judge of the effect of the same method of sterilization as applied to the same catheter employed nightly by a patient in a similar condition, who merely wipes his catheter after use and carries it in his pocket or stores it in the commode till he next requires its help. To further quote illustrations appears unnecessary.

(*c*) The medium inoculated. How far exactly this may

introduce sources of fallacy is difficult to say. That it may be capable of vitiating the results of a whole series of tests I know from experience. Comparatively slight alterations in reaction of a medium and alterations in its constitution would appear to be capable of preventing or promoting the growth of certain organisms, and thus leading to erroneous conclusions. The subject is one of enormous difficulty, and I would desire to speak with much reserve in the meantime. As indicated below, Drs. A. R. Ferguson and Leslie Buchanan are kindly investigating the matter with me at the present time.

Those three sources of fallacy, with others of minor importance, have been so obvious in the experiments above detailed, as well as in others I have conducted, that anything further in the way of comparison than the comments and conclusions I have indicated under each method of sterilization tested would appear unwise.

The *metal* catheter and the *soft red rubber composition*, or *Jacques'* catheter, may be readily and certainly rendered sterile for an indefinite number of times either by boiling or by washing and immersion in sufficiently powerful antiseptic solutions, and this without sustaining damage.

With regard to the sterilization of all forms of "*gum-elastic*" or "*varnished*" catheter it is perhaps not too much to say that, while there are various methods which are not, when repeated, destructive of the catheter, and which in the way of asepsis of the catheter offer a "reasonable degree of security," there is no method which is entirely reliable. Further, as my experiments seem to indicate, it may also be said that there are reputed methods of sterilizing catheters which, when accurately tested, fail to justify the faith placed in them.

For practice I have formulated for myself certain rules:

(1) Avoid, as far as possible, the employment of catheters. In cases of stricture, for instance, it can only be very exceptionally indeed that the use of a catheter is called for. Bougies, metal or soft, which are readily and certainly sterilized, will do all that is necessary.

(2) When a catheter must be employed use, where possible, a red rubber Jacques' catheter in preference to a gum-elastic or metal one. In such cases as retention due to atony, spinal

paralysis, reflex nervous effects, and other causes, and in many cases of prostatic retention, a red rubber catheter answers quite as well as a gum-elastic or varnished instrument, and is as readily sterilized by boiling or immersion in antiseptic solution as is a metal one.

(3) In cases where the use of a catheter is necessary, but in which a red rubber instrument fails to pass and something stiffer must be employed (as, for instance, in many cases of prostatic retention), the use of metal catheters, especially by the patient, does not commend itself as a measure free from grave risk of injury to the prostate or urethra. One has therefore to fall back on gum-elastic catheters.

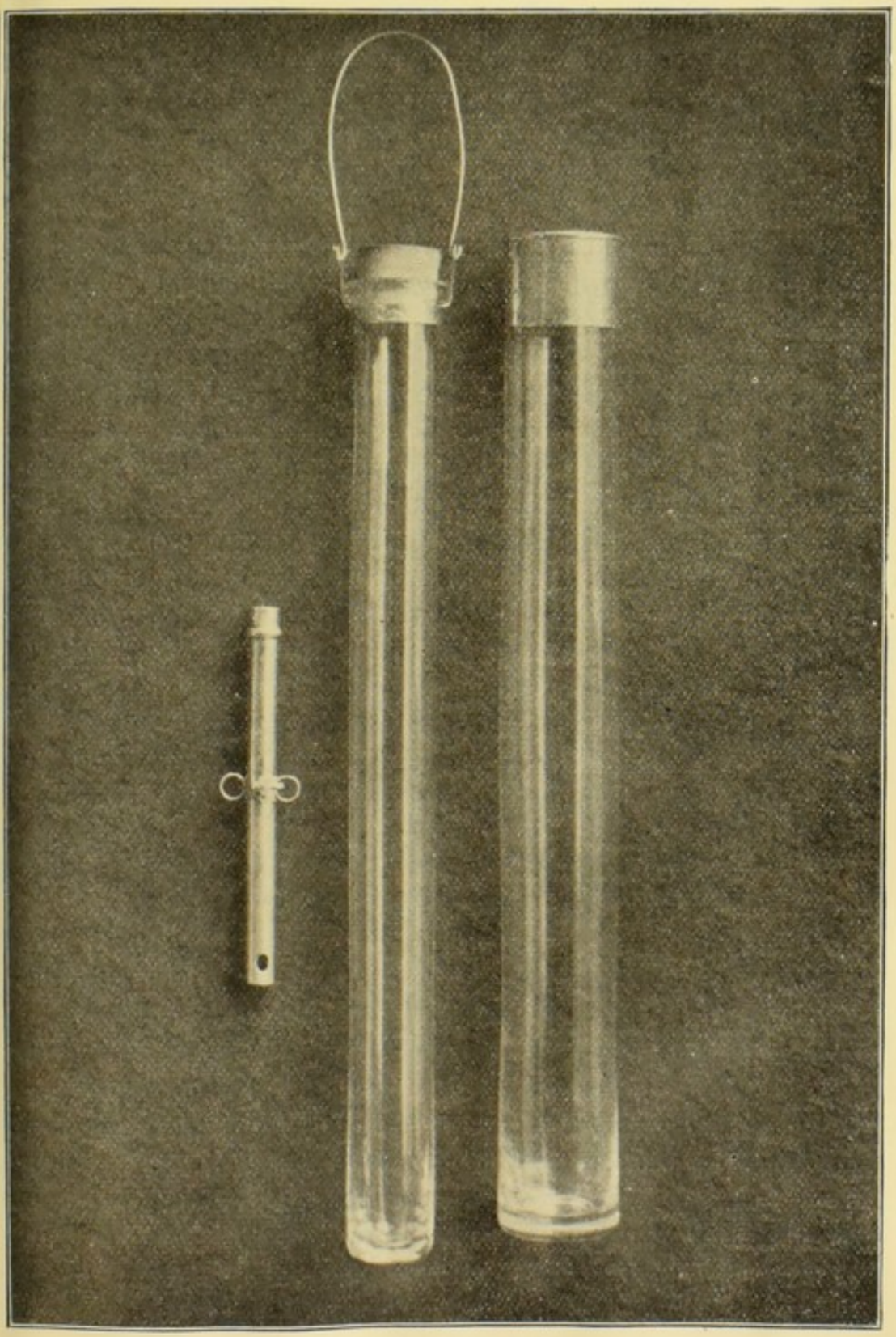
In such cases, if the urine be very septic, I destroy any catheters I have employed. If the urine be not very purulent or offensive I subject the catheters used to external washing with soap and water, and then with antiseptic solutions, followed by internal steaming. Those which survive I retain.

For those cases where the regular use of a gum-elastic catheter is necessary the patient is supplied with a catheter with well-finished interior. After use he thoroughly washes it, holds it under the tap for a few minutes, and then lays it aside in a dish of boracic, weak perchloride, or other weak antiseptic solution. I do not believe that this primitive treatment offers more than a "reasonable chance" of asepsis, but I know, from experience, that it is useless to expect an average patient to carry out anything more elaborate in the way of cleaning.

Moreover, when a prostatic patient has reached the stage of regular catheterism, with all its risks of septic cystitis and vesical atony with their grave sequelae, it comes to be a question for his medical adviser whether his condition warrants an operation for the radical cure of his trouble by means of prostatectomy or castration.

Some years ago I had constructed the glass tubes or bottles figured in the illustration. They are made with either metal or rubber stoppers. In these I store and carry catheters for use. Any catheter used is simply washed and held under the tap and laid aside (or carried home wrapped in a towel or gauze) for sterilization before it is replaced in the glass receptacle.

For prostatic patients who either decline operation or are not fit subjects for it, and who therefore must use a catheter



regularly, I have had these glass receptacles fitted with a metal handle (see illustration). By means of this they may be hung on the bed or in the wardrobe. Filled with an antiseptic fluid they are at hand for the catheter after use, and when emptied of fluid may be used for storing the catheter while travelling. The antiseptic fluid will vary in strength according to the kind of catheter employed, it being necessary to employ weak solutions for gum-elastic catheters, while red rubber catheters will stand anything.

The illustration also shows a metal perineal drainage tube. Surrounding it is a metal ring or collar tightened by a screw or clip. The tube is inserted into a bladder, the tapes fastened to the "eyes" on the collar, and the collar so adjusted by means of the screw that the internal aperture of the tube rests in the *base* of the bladder. I believe the tube has two advantages over the ordinary red varnished perineal tube: (*a*) It may be sterilized in five minutes and replaced fresh; (*b*) by adjustment of the collar the inner end of the tube may be kept at the level of the floor of the bladder, thus acting as a drain of the whole viscus. The old red tube is fixed by a piece of wire in more or less hap-hazard fashion, and frequently in such a way that the internal aperture projects far into the cavity of the bladder, leaving the urine (flowing from the ureters at a lower level) either to accumulate in the lower part of the bladder or find exit alongside the tube.

PART II.

The second part of the investigation, viz. that into the presence or absence of bacteria in the urethrae, morbid and normal, into which in practice instruments have to be passed, I was led to undertake by the discussion which followed a paper which I read before the Glasgow Medico-Chirurgical Society in November, 1893, and which was subsequently published in the *Glasgow Medical Journal*. In that paper and in the discussion which followed, in arguing for the employment of soft bougies (and catheters) in preference to metal ones in cases of urinary retention, more particularly in cases of tight or

tortuous stricture, I expressed the conviction that soft *bougies* could be as readily rendered sterile as could metal ones, that the same might be claimed for *red rubber catheters*, while *gum-elastic catheters*, though not so readily or certainly sterilized, might, when of good quality, by a combination of washing and internal steaming, be rendered sterile for a certain number of times after use. These statements, based on previous practical experience, are, I venture to believe, fully borne out by the more formal experiments I have since then conducted, and which are published in the first portion of the present paper.

In the paper of 1893 I adduced evidence of the change of opinion in favour of soft as against hard instruments, which, originating in France, and led by Sir Henry Thomson in this country, and by American surgeons, has within the last twenty years largely revolutionized practice in cases of stricture and of urinary retention generally; and I pointed out that in Scotland, while soft instruments are now used for prostatic retention and for retention from vesical paralysis and atony, there is a decided tendency to retain the use of metal instruments in cases of stricture. The reason for this somewhat illogical use of metal instruments in stricture, while soft instruments are employed for prostatic and spinal retention cases, appears to lie mainly, if not solely, in the belief that soft instruments are less readily sterilized than are metal ones. That, as regards bougies (the instruments then under discussion) at least, this is certainly not the case the foregoing experiments would seem to indicate, and my opinion to this effect I stated at the meeting and in the paper referred to. I further drew attention to the point that even, for the sake of argument, granting this view to be correct, it represents one side only of the question. In a case of tight stricture the condition of the channel through which the instrument has to be passed is of as much importance as the condition of the instrument, in certain cases probably of more importance; while in all cases demanding instrumental treatment the nature of the fluid in the bladder and the character of any moisture or discharge present in the urethra may constitute features in the case of graver import than the condition as regards asepsis of the instrument employed.

In the paper and discussion referred to, two points were drawn attention to, (*a*) the condition of the gastro-intestinal tract from mouth to anus. This is constantly occupied by the most septic contents without detriment to the health of the patient. The most trivial wound of any part of the mucous membrane, however, is apt to result in the production of septic mischief, frequently of the most rapidly fatal kind. (*b*) The fact that the larger number of strictures are not mere annular constrictions, but long tortuous narrow channels, often with eccentric openings and with sharp angles and curves, of the direction and position of which the surgeon is ignorant. Along such a channel it is manifestly never easy, and is frequently impossible, for the surgeon to pass a rigid metal instrument of a calibre of the necessary smallness without abrading the surface of a curve or ploughing up an angle, even though he may successfully ultimately enter the bladder without the formation of an actual false passage. Through such a channel, on the other hand, fine or small-sized soft instruments, held lightly between finger and thumb, may be wriggled without damage, the pliable point and neck adapting themselves mechanically to the curves and angles of the channel, anything like abrasion or tearing being impossible.

Succeeding the foregoing, the following passage occurs in the paper: "Let it, for the sake of argument, be assumed for the moment that this statement" (viz. that soft instruments are less readily or certainly rendered pure than are metal ones) "is absolutely true in fact; it will not be difficult to prove that the argument based on it, even if its truth be assumed, is specious. . . . Given a case of tight, tortuous, or oblique stricture, with swollen congested surface covered with muco-purulent gleety discharge, and with possibly cystitis and purulent urine in addition. Take to treat it a soft instrument, filthy and septic. In the majority of cases you can gently wriggle this through, and that without abrading any part of the mucous membrane, thus dilating the stricture and giving free exit with the urine over the intact mucous membrane, not only to anything septic you may have introduced, but to the discharge in the urethra and the pus in the urine. Take, on the other hand, a small No. 1, 2, or 3 steel bougie or catheter,

carefully sterilized, and, in passing that through, make but the slightest abrasion, and you establish a condition of affairs in which it is not your fault if septic mischief does not ensue. . . . What I have for the moment admitted for the sake of argument, however, I do not in any sense admit as an actual fact, namely, that soft instruments, when intelligently managed, tend more to be septic than metal ones."

How far these statements of opinion, based at the time when they were made on general observations, are borne out by specific investigation may be gathered as regards the instruments from the records of the experiments detailed in the first portion of the present paper. How far they may be accepted as regards the urethra may be to a great extent gathered from what follows, though, as previously mentioned, the investigation is not yet completed.

Series of twenty-four cases of stricture examined in the dispensary and wards of the Western Infirmary, in the Glasgow Central (formerly Andersonian) Dispensary, and in private practice. Reproduction of detailed notes of these cases would serve no useful purpose. They were cases of ordinary urethral stricture coming for treatment either on account of acute retention or in the course of treatment by gradual dilatation. All had had instruments passed by various people on occasions previous to that on which the examination was made. The media inoculated were gelatine, agar, and glycerine agar. The procedure in all cases was the same. Before passing an instrument the meatus was cleansed with carbolic lotion (1-40), and dried with sterilized gauze. A pair of fine sinus forceps, sterilized in the flame of a spirit lamp, was then passed into the urethra to the extent of about an inch, and opened. Between the blades, and therefore out of contact with the urethral orifice, a stout platinum wire with a looped end, carefully sterilized in the flame, was passed into the urethra. The distance to which this was passed varied from two to eight inches in different cases. In withdrawing it care was taken to keep it between the blades of the forceps, and therefore free from contact with the meatus, and inoculation of the medium was at once made.

The results were as follows:

Case 1. Bacillus coli.

Case 2. Bacillus coli.

Case 3. No growth. The patient was not seen again.

Case 4. Diplococcus and bacillus coli.

Case 5. No growth. Three weeks later a second inoculation test yielded a growth of diplococcus.

Case 6. Diplococcus.

Case 7. Diplococcus and a staphylococcus.

Case 8. Bacillus coli.

Case 9. Bacillus pyogenes foetidus and staphylococcus aureus.

Case 10. No growth. Patient not seen again.

Case 11. Bacillus coli.

Case 12. Diplococcus and staphylococcus aureus.

Case 13. No growth. Eight days later a second inoculation test yielded also a negative result.

Case 14. Diplococcus and bacillus coli.

Case 15. Large white coccus growing freely. Several secondary cultures were grown, and the growths submitted to three bacteriologists, but no definite decision was arrived at.

Case 16. Diplococcus and a staphylococcus.

Case 17. Growth of cocci. Culture accidentally destroyed before the germ had been identified.

Case 18. Bacillus coli.

Case 19. Bacillus, investigated by secondary cultures, and variously pronounced to be bacillus pyocyaneus, bacillus subtilis, and proteus vulgaris.

Case 20. No growth. A second inoculation test a week later yielded a staphylococcus.

Case 21. Large white coccus not definitely identified.

Case 22. Diplococcus and streptococcus pyogenes.

Case 23. Diplococcus and bacillus coli.

Case 24. Coccus arranged as a streptococcus.

In the foregoing series of tests the media were prepared in different places and by different people. No accurate record of the reaction was kept. The composition and reaction of the media may have, as further research has taught me, a very decided effect on the results of inoculation tests. I have in my possession records of inoculation tests recently made which

clearly demonstrate this. Two instances may be given: (1) T. H., aet. 2 years, operated on at the Children's Hospital for tubercular disease of the metacarpus. While under chloroform the urinary meatus was cleansed and inoculations made from the urethra on acid agar and on alkaline agar. The alkaline tube yielded a luxuriant growth of a large diplococcus, while the acid tube remained sterile. Secondary inoculations were made from the growth in the alkaline tube into (*a*) the original acid tube, (*b*) a fresh acid tube, (*c*) a fresh alkaline tube. Tube *c* yielded a vigorous growth of the same diplococcus, while *a* and *b* remained sterile. (2) M. A., aet. 37, operated on for varicocele. While under chloroform the meatus was cleansed and inoculations made in neutral glycerine agar, which yielded a mixed growth of a diplococcus and a large coccus. Attempts to produce secondary cultures on neutral glycerine agar failed, but both germs grew well on alkaline agar.

These are not the only difficulties of the investigation. The whole subject bristles with them. For instance, in investigating a series of normal urethrae, I have several times failed to obtain cultures by the method above described, viz. that of passing a loop of platinum wire into the urethra, and subsequently succeeded by gently scraping the urethral mucous membrane with a fine curette, and making inoculations with that. Again, I have on several occasions obtained films on coverglasses, in which germs were readily demonstrated by staining, by scraping urethrae which were normal, or by scooping out discharge from urethrae, the seat of gonorrhoea or gleet, in cases in which careful inoculation yielded no result.

Nor does this end the list of difficulties. There is one of probably greater magnitude than any of the foregoing. In the series of tests above detailed, the term "diplococcus" is largely employed, and I have been driven to the use of this collective term by two considerations. In the first place, and here I would speak at present with very definite reservations, I am not certain that there is any way of distinguishing between the "gonococcus" and the other diplococci. There may or may not be. At present I do not know. Of one thing, however, I am convinced, and believe I already have the means of demon-

strating, viz. that much of what is published in the various text-books on this matter must be received with caution, if not ultimately entirely discarded as erroneous. In the second place, and to a large extent proving the preceding statement, I have repeatedly received contradictory reports from different competent bacteriologists on the same germ. In my possession are a large number of detailed reports on such cases. Repeatedly the same germ has been termed "gonococcus" by one authority, "micrococcus urea" by another, and some one or other of the various diplococci by a third. And in reading the reasons detailed in these reports for the decisions come to, one ceases to wonder at the differences of opinion contained in them, and, as already stated, giving such contradictoriness to the various published statements. The personal equation, for one thing, must be reckoned with. Particularly in the matter of Gram's test is this so. What means decolourization to one eye is not so in the view of another of equal experience. Further, in making a series of examinations, as cases occur, germs which did not decolourize by Gram's method at the time of staining may gradually lose colour on being kept till, in the course of weeks or months, the series is completed, the result being that the same germ may be at one time pronounced "micrococcus ureae," and at another, "gonococcus" by different observers, or even by the same observer.

Again, there is the frequently quoted statement that the relative size of the germs grown forms a distinctive point as between the gonococcus and the other diplococci of the urethra. My experience over a large series of cultures has been such as to raise, though not by any means to settle, the question whether it may not be the case that a given diplococcus varies in its size at different periods of its growth in the same culture, and whether it may not be distinctly modified in size in successive secondary cultures. This question opens the entire field of mixed cultures (whether in artificial media or urethra), and of the effect of alterations in composition and reaction of media, and is manifestly one not capable of ready solution.

One other point remains. The appearances of a number of cultures (and I have before me now a series of growths, highly suggestive in this connection) raise the further question of the

relation of various morphological forms of germ found in the same culture. Take a tube in which, within a few days of inoculation, there are found patches of growth, which, on microscopic examination, are noted as "Staphylococci of a size distinctly larger than those obtained from a recent case of facial boil. Staphylococci of very much smaller size. Diplococci of large size. Diplococci of smaller size and less defined outline." (This is the note of an actual culture obtained from a perfectly normal urethra). There are two possible explanations. (*a*) The tube contains a mixed growth; (*b*) the tube contains a culture of one germ only, which is in process of actively reproducing itself by fission. The solution of this question may or may not be possible in a given case. In any event, it demands weeks of work in the production and observation of secondary cultures in media of various compositions and reactions.

During the past four years I have carried out carefully made and noted bacteriological tests in the following, amongst other, series of cases:

Cases of prostatic retention requiring instrumental aid.

Cases of tubercular disease of the prostate.

Cases of acute gonorrhoea.

Cases of chronic gleet.

Cases of adults with normal urethrae.

Cases of children with normal urethrae. (This investigation, carried out in cases under operation in the Children's Hospital during 1896, yielded some puzzling and suggestive results.)

The records (with, in many cases, the actual stained specimens, and, in a number, with microphotographs of the germs) of all the results are in my possession, and it had been my intention to publish them as part of this paper.

In the face of the difficulties and sources of fallacy above indicated, and of the secondary or subsidiary lines of investigation which have opened out in the attempt to eliminate these sources of fallacy, it seems preferable to delay publication in the meantime.

As already stated, Dr. A. R. Ferguson, Assistant Pathologist, Western Infirmary, and Dr. Leslie Buchanan, Pathologist, Eye Infirmary, have kindly consented to aid me in further investi-

gation, and have, indeed, already done much work in the matter, the records of which it is intended to embody in a joint paper. That the result of the investigation going on will be to finally settle all doubtful points in the subject no one practically acquainted with the nature of the difficulties will venture to predict. On the other hand, looking to the special fitness for such work of my collaborators, by reason of their special knowledge and great facilities, and having regard to the great amount of time and skill being devoted to the production of the records and specimens accumulating in Dr. Ferguson's hands, and of the microphotographs of these specimens accumulating in Dr. Leslie Buchanan's hands, it is possible that the publication of these records and microphotographs, with particulars of the cases to which they relate, may provide material to form a basis on which certain conclusions may be founded, and from which the investigation of possible difficulties may take origin.