pea-soupy character. During the last week there was constipation. It seems certain that in the relapse the large bowel was chiefly affected, and there was much enlargement of the spleen and extensive lung complication. The temperature on the whole ran much higher during the second than during the first attack, and during the first attack was more easily controlled by the cold applications than during the second. The chief interest of the case relates to the purpura, which occurred exclusively during the primary attack. It commenced on the thirteenth day of the illness with oozing from the mouth cavity. This continued for six days, and a few petechiæ remained on the palate for two days longer. It is possible that the thin bloody discharge from the vulva which appeared on the fifteenth day of the illness, and lasted for about twenty-four hours, was allied mucous hæmorrhage. The appearance of small quantities of dark blood in the stools extended from the fifteenth to the twenty-fifth days—that is, it overlapped the bleeding from the mouth by about six days, but the last quantity passed was quite minute. The blood in the urine extended over three days—viz., from the sixteenth to the eighteenth day. The purpuric spots on the arms and the larger extravasations on the lower limbs and on one shoulder appeared on the eighteenth day, but I do not think any fresh petechiæ appeared after the twenty-first day, when all those present were rapidly fading. It is noteworthy that the skin extravasations, unlike hæmorrhagic measles and small-pox, had no relation whatever to the proper typhoid eruption, which did not become purpuric. It seems probable that some of the splenic and renal infarctions and the curious extravasation found post-mortem in the posterior mediastinum and in the retro-peritoneal tissue may have occurred during this purpuric period. Also, in spite of the fact that it was still liquid, it is not impossible that the blood which was found on the surface of the hemispheres may have oozed out about the same time as the above. It appears worth emphasising that the primary attack, during which the purpura exclusively occurred, was a moderate if not a mild one, and that, so far as one can judge, the purpura in no way contributed to the fatal issue. Purpura is no doubt a rather rare complication of typhoid fever, but it is worth investigation whether when it does occur it has necessarily the grave import which has been ascribed to it. Dr. Wickham Legg, in a recent paper in the *St. Bartholomew's reports*, refers to a case of purpura in typhoid recorded by Schneschknow. The patient was a child of six. Purpura appeared on the sixteenth day. Bleedings occurred from the nose, mouth, under the skin, and in the urine and the stools. But recovery set in on the twenty-first day, when the bleedings ceased. Dr. Murchison¹ speaks of having met with purpura spots and vibices in rare cases, several of which recovered. It is probable, therefore, that when purpura occurs in typhoid it is not necessarily a sign of malignancy.

ON

URETHRAL FEVER AND CATHETER FEVER.²

By J. C. OGILVIE WILL, M.D., F.R.S.E.,

SURGEON TO, AND LECTURER ON CLINICAL SURGERY AT, THE ABERDEEN ROYAL INFIRMARY.

FEVER following urethral instrumentation has recently received not a little attention; but although much has been said and written concerning it, it seems to me that a sufficiently distinct line has not been drawn between two conditions which, while they bear a certain resemblance to each other, are really essentially different. I refer to what has been for long known as urethral fever on the one hand, and what Sir Andrew Clark has termed catheter fever on the other. The object of this communication is to define as distinctly as may be the two disorders, and to point out wherein they differ, and I think that it will not be difficult to show that they do so in causes, symptoms, and results.

Urethral Fever.—This term is applied to a pyrexial condition which not infrequently follows the dilatation of a stricture by the passage of a bougie, or a simple urethral

exploration. It assumes different forms, the most common one being the occurrence of a rigor, high temperature, 100° to 106° F., and sweating some hours after instrumentation has taken place. The rigor most commonly, but far from invariably, supervenes on the first urinary act following the operation. The symptoms are of short duration, the patient frequently being perfectly well on the following day, or at most suffering from slight headache and malaise. This is what I regard as a typical case of urethral fever, but many divergences from this type are met with. There is, for example, a form of urethral fever where one or two slight rigors, with slight fever, and no sweating constitute the entire symptoms of the malady, and there are cases—I met with one recently—where the shock is so profound that prolonged and extreme collapse speedily follows the rigor (my patient was in this state for two days, but ultimately recovered), suppression of urine, uræmia, and death ensue. The conditions just referred to are well known, and have for long been described in all standard works, and various explanations of their origin have been offered. Some have stated that urethral fever arises from the absorption of urine through some damaged portion of the urethra where the instrument had abraded the mucous lining of the canal, and this view was founded chiefly on the fact that the symptoms of the disorder did not show themselves until after the first act of micturition subsequent to the operation. This view cannot be upheld for two reasons—viz., first, because urethral fever is met with very frequently where the urine is perfectly normal, and where its absorption would be, as experiment has shown, perfectly harmless; and second, because in many cases no injury to the urethral walls has taken place. Another explanation is founded on the state of the kidneys prior to the commencement of urethral treatment, but one valid objection to this view must at once occur to the minds of all those who have had much experience of operative procedures on the urethra—viz., that while ordinary catheterism or the simple passage of a bougie may be followed by urethral fever, the performance of much more serious operations may in the same subject be unattended by it. This fact is so well known that it seems almost unnecessary to cite any evidence in proof of its correctness, but if such were wanting most surgeons would be able to recall cases similar to one recently under my treatment, where the passage of a bougie through a stricture was followed by rigors, a temperature of 105·4°, and profuse sweating, but where the subsequent performance of internal urethrotomy gave rise to no untoward symptoms. Neither of the two explanations just spoken of seems to me to be worthy of acceptance, and for my own part I cannot but believe that the only tenable hypothesis regarding the causation of urethral fever lies in the reflex theory, and this view, I am glad to observe, seems to be gaining ground among those who have studied this subject attentively.

Catheter Fever.—This form of disorder is chiefly met with in men suffering from retention resulting from enlarged prostate. It frequently commences on the second or third day after catheter life is entered on, and its symptoms are those of irritative fever. The patient is sick and feverish, complains of nausea, has a red tongue, which becomes dry and contracted, the skin is harsh and dry, and sometimes clammy, he becomes gradually weaker, sinking slowly, death occurring two, three, or four weeks afterwards. The urine, which when first drawn off may be comparatively healthy in appearance, soon becomes alkaline, and deposits a large quantity of stringy mucus in the vessel; it frequently contains pus and swarms of micrococci.

The occurrence of the train of phenomena above described attracted my attention many years ago, and I have on frequent occasions directed the attention of our clinical class to the possible dangers of catheterisation in cases of chronic retention; and I may mention here, before entering on the causes of catheter fever, that it may be prevented with a very considerable degree of certainty by attention to two matters of detail—namely, the use of an aseptic catheter, and at first drawing off only part of the retained urine instead of completely emptying the chronically distended viscus.

Regarding the causes of catheter fever, I was for long under the belief that it had a mechanical origin—that the sudden removal of the support afforded to the walls of the bladder by the retained water so affected the lining membrane of the vesical cavity that a low form of inflammation resulted, and subsequently spread to the kidneys. This belief was engendered by the clinical fact to which I have just now alluded—namely, that if the bladder be only partially

¹ Continued Fevers, p. 515.² This paper was written before the publication of Sir Andrew Clark's article on Catheter Fever, which appears in the April number of the *Edinburgh Medical Journal*.

emptied catheter fever does not ensue. But a recent and careful consideration of the subject has led me to adopt a different view, for, while I still believe that the changes in the condition of the circulation arising from the diminution of pressure have a not unimportant bearing, I am now of opinion that the cause of catheter fever is the introduction of putrefactive germs into the vesical cavity, and that such is the most likely explanation is proved by post-mortem examination of the bodies of those who have succumbed to this malady, for the appearances met with are precisely similar to those encountered in other parts of the body when sepsis is the cause. The bladder is generally inflamed and sloughy, the ureters and pelves of the kidneys congested, the kidneys are riddled with small abscesses, and micrococci abound everywhere, the bladder, ureters, and straight and convoluted tubes swarming with them. And here the question arises, How do the germs of putrefaction gain access to the bladder? A few years ago the answer would have been a very easy one, but since the value of an aseptic catheter has been universally acknowledged, we have to look further afield, and in doing so we may inquire what bearing the partial or incomplete emptying of the bladder has on this point. To my way of thinking it has a very important one, and furnishes the required explanation; for if the bladder be completely emptied, there is great probability of regurgitation of air through the catheter taking place, and this is more likely to occur if digital compression of the hypogastrium has been required to cause the urine to flow, or if the patient has used voluntary efforts to expel the last few drops of water, and both of these occurrences are probable enough, for the muscular expulsive apparatus of the bladder is frequently almost, if not completely, useless. If only a partial withdrawal of the vesical contents be practised, the danger of the ingress of germ-laden air is obviated, and thus we are furnished with an explanation of a clinical fact, the existence of which I have for long been familiar with, but which I have been unable until lately to satisfactorily account for. The putrefactive germs having thus gained access to the bladder, find in its lining membrane, which is in a stretched and weakened state, a very suitable nidus for their development, the result being the production of the pathological changes above described.

In conclusion, I would remark that in my desire to make this paper as brief as possible, I have purposely omitted many points of great interest and importance, but I have endeavoured to show that urethral fever and catheter fever are two totally different disorders, and if I have in any measure succeeded, the end I had in view has been attained.

Aberdeen.

ON CYSTS AND CYSTIC TUMOURS OF THE BREAST.

By HENRY T. BUTLIN, F.R.C.S.,

ASSISTANT-SURGEON AND DEMONSTRATOR OF DISEASES OF THE LARYNX,
ST. BARTHOLOMEW'S HOSPITAL.

NO. II.—CYSTS OF THE CONNECTIVE TISSUE.

SOME years ago a breast which had been amputated for carcinoma was sent to me for examination. It was well formed, and there was no retraction of the nipple. It contained a tumour about the size of a racket ball, situated deep down in the substance of the gland. The breast had been removed from a woman between forty and fifty years old, who had noticed the lump about three months, and believed that it was caused by a slight blow some time previously. There was no glandular affection. The smoothness of the tumour made me a little suspicious of its nature, and palpation before cutting into it led me to think that there was very indistinct fluctuation. No sooner was the knife thrust into it than there spurted forth a small quantity of turbid fluid, and the tumour quite collapsed. It proved to be a single, very smooth-walled cyst, about as large as a walnut. The cyst-wall, examined microscopically, was fibrous, and there was no epithelial lining such as one finds in the glandular cysts, although here and there epithelial, or more properly endothelial, cells were visible upon the inner aspect of the wall. There were no intra-cystic growths. Shortly before this I assisted Mr. Savory in an operation on

the breast of a lady supposed to be suffering from cancer. The tumour, as in the last case, lay very deep down in the substance of the gland, and was so surrounded by fat that it was very difficult to convince oneself of the exact consistence of the growth. Under these circumstances the just precaution was taken of making a preliminary incision into the tumour. Fluid spurted forth as if from a syringe, showing how tightly it had been encased. The cyst was removed; it proved to be a simple, smooth-walled cavity, bearing similar general characters to the cyst in the last case.

In August, 1880, a lady, forty-three years old, was sent to me by Mr. Thomas Taylor, for the removal of a tumour of the breast, which she first noticed about two months previously. Since that time it had increased but little, if at all, though it had become slightly painful, especially during the last two or three days. She knew of no cause for its occurrence. It was situated in the upper and outer part of the left breast, was the size of a pigeon's egg, not clearly defined, knotty, very firm, almost hard, not fluctuating at any point. It appeared to be continuous with the mammary gland, with which it moved freely. The skin was dimpled, but not adherent; the nipple was not retracted; there was no enlargement of the axillary glands; the patient seemed to be in excellent health, though, naturally, alarmed at the presence of the tumour. I thought it was a carcinoma, and made all preparations to remove the breast. The absence of retraction of the nipple, of adherence of the skin and of enlargement of the glands, did not materially affect the diagnosis, for these signs of carcinoma are not invariably present, especially in cases presumably so recently developed as this was. However, an incision was first made; a cyst was opened, from which clear fluid escaped, and the error of diagnosis was corrected. A little tumour was removed, consisting of three small cysts in a mass of indurated tissue, the largest of them was about the size of a nut; they were thin-walled and smooth, and neither of them contained any intra-cystic growths. The lining of the cavities was similar to that in the first case; the solid tissue between them was composed of fat, fibrous tissue and glandular structures, with a slight infiltration of leucocytes.

These cases are very good examples of a disease which has not been so carefully described in the later treatises on diseases of the breast as it deserves to be. It is without doubt the same disease which Birkett has called the simple cyst of the breast containing serous fluid.¹ Billroth, Gross, Labbé and Coyne, Follin and Duplay, take so little notice of it that they scarcely seem to recognise its existence. Yet it is not at all uncommon. I have myself notes of five cases, and have seen two or three others of which no notes were taken. I have not, however, been able to collect more than a very few from surgical literature, not because they are not recorded, but because the records are imperfect, especially with regard to the structure of the cyst-wall, and there is not sufficient evidence to prove that the disease is not of glandular origin. The cysts form in the connective tissue of the mamma by gradual accumulation of fluid in the spaces between the fibrous bundles; the dilatation is not necessarily confined to a single space, but may affect several neighbouring spaces, which communicate to form a single cyst. The surrounding structures are condensed, and a more or less thick wall is produced, which is often lined with a layer of endothelial cells. The knowledge which we now possess of the relation of the interfibrillar spaces to the lymphatic system, and the endothelial lining of the cysts, might lead us to infer that many, if not all, of these cysts are lymphatic cysts. Indeed, they have been so described by Mr. Gadsby² in the case of a breast removed by Mr. Marshall at University College Hospital. There were many small cysts scattered through the substance of the breast, but the chief disease consisted of one smooth-walled cavity, containing brownish mucoid liquid. A fragment of the wall of the cavity, stained with nitrate of silver, displayed a perfect endothelial lining, with stomata between the cells; a circumstance which led Messrs. Marshall and Gadsby to term the disease "lymphatic cysts." The researches of the last few years have taught us to believe that many of the cysts which were formerly termed "serous" are in reality lymphatic; the congenital hygromata and the cavities in macroglossia afford notable examples of this. The cause of the accumulation of fluid in the lymphatic spaces is not, in the majority of cases,

¹ Vide THE LANCET, vol. ii., p. 105, 1868.

² Ibid., vol. i., p. 234, 1878.