III. Original Communication.

ON THE TREATMENT OF INOPERABLE CASES OF CARCINOMA OF THE MAMMA: SUGGESTIONS FOR A NEW METHOD OF TREATMENT. WITH ILLUSTRATIVE CASES.

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Mr President and Gentlemen,—I have no doubt it has fallen to the lot of nearly every one here present to have been consulted from time to time by patients suffering from carcinoma so widely spread or so situated that it has been quite apparent that nothing in the way of operative measures could be recommended. Such cases naturally excite our sympathy, but they also bring home to us the fact that once a case of cancer has passed beyond the reach of the surgeon's knife, our curative measures are practically *nil*, and "that, whether the case advance with giant strides or with slow and measured steps, the result is equally sure and fatal." Of late, owing to my taking up the work of surgeon to the Glasgow Cancer Hospital, I have seen a considerable number of such cases, and an opportunity has been furnished me of working out a line of treatment which I am not aware has been as yet tried by others, and which is founded on a view of the etiology and nature of cancer which is entirely opposed to the local parasitic theory of the disease, and which seems to me to offer a more reasonable explanation of it. As these inoperable cases of cancer may be arranged into two groups—first, those which have been operated on, but in which, sooner or later, there has been a recurrence, or, as it should perhaps be better expressed, a reappearance of the disease; and, secondly, those in which no operation has been attempted, but in which, when they first present themselves, the disease has progressed so far that no local removal could be attempted—I shall bring forward three cases, one of which is illustrative of the first group, and the other two of the second.

The first case, then, that I wish to bring under your notice is that of Elizabeth B., who consulted me on 11th May 1895, at the Glasgow Cancer Hospital, bringing me the following letter:—

"37 Apsley Place, *6th May* 1895.

"Dear Dr Beatson,—The bearer, Mrs B., is, and has been suffering, I fear, from a malignant breast. She has been in the Royal Infirmary before she came to me. My own opinion is that nothing can be done for her; but as she is a woman of great courage, you might have a look at it for my sake, and perhaps you can order her something in the way of dressing. Even this little will be accepted by her as a great deal.—With kindest regards, yours very truly,

"James W. Wallace."

The history she gave me was that she was 33 years of age, married, and the mother of two children, the oldest 3 years of age

and the youngest 15 months. She nursed both of her children for ten to twelve months, chiefly on the left breast, the first child entirely so, as the right breast suppurated for two or three weeks. While nursing her first baby she observed a small hard lump at the outside of her left breast, and as it was painless and did not increase in size she took no further notice of it. It was only when her second baby was born twenty months later that she became aware it was increasing. She nursed the child on both breasts notwithstanding, and it was not for ten months, by which time the tumour had grown a good deal, that she weaned the child and sought advice at the Glasgow Royal Infirmary. In January of 1895 she was admitted to that Institution, and the journal report states that an examination shows the left mammary gland to be a little more swollen than the right one, and to present a hard and nodular appearance. In its centre is felt a large mass, measuring 5 in. across and 3½ in. in vertical diameter, while small nodules from this infiltrate the skin around. About 2 in. upwards and to the left of the nipple is seen an ulcer 1 in. in size, two nodules about the size of beans bordering on the extreme left of this ulcer. Patient appears to be strong, healthy, active, and robust.

On 25th January 1895 she was operated on. The Hospital journal says that the left breast was excised, a large area of skin free of tumour being taken away. The axillary glands were removed, also a considerable part of the pectoral muscle which appeared to be implicated. A plastic incision was made parallel to the trunk to allow of the edges of the wound being approximated. Patient seems to have made a good recovery, and to have left the Infirmary towards the beginning of March with the wound almost healed. About a month after she had gone home, that is, within three months of the operation, she noticed that the wound had opened, that a little discharge was coming away, and that pain of a shooting character had developed. She observed, also, that a hardness was growing at the side of the scar, and so she returned to the Infirmary for advice. She was there told that she should come into the Hospital again. She was readmitted for a few days and then discharged, as it was thought that an operation would be useless. The journal report is as follows:—

"28*th April* 1895.—Dismissed. General involvement of whole scar by large tumours, cancerous in nature, to remove which entirely was thought impossible. Adherent axilla and chest walls. One of the wounds from the recurring secondary tumours has given way, and there is now an ulcerated surface."

Such, briefly, was the outline of her personal history as detailed to me. On questioning her, nothing could be elicited in her family history that showed any hereditary tendency to cancerous disease. On 11th May, at the time she presented herself to me, the local condition for which she sought advice was as follows:—

On the left side of the thorax there is seen a very extensive

cicatrix in the situation of the left mamma, which has apparently been entirely removed. The scar extends from the middle of the axilla to within $1\frac{1}{2}$ in. of the xiphi-sternum. It is irregularly curved in aspect. Above the centre of scar is a cicatrizing area, which had broken out after the operation in January last. This is now granulating and seems healthy, but immediately below, and arching over centre of the long scar, is a mass of recurrent tumour, hard and nodular, with much thinning and discoloration of skin. This mass is curved in shape, about $2\frac{1}{2}$ in. broad at its broadest part, and about $3\frac{1}{2}$ in. in length. There are other nodules in the cicatrix as far back as the axilla. Four inches lower down there is the linear cicatrix of a plastic operation, made apparently to allow of the sliding together of the edges of the operation wound. No enlarged glands felt in axilla or above clavicle, but there is a distinct tumour of the left lobe of the thyroid gland, with some enlargement of the isthmus. This, however, she said had been present as long as she can remember. Right breast and axilla were free of any disease. Patient's weight was 9 st. 9 lbs. She looked pale and careworn, and when questioned admitted she felt ill, and was quite unable for her household duties.

From the clinical history she had given me, and from the local condition present, I had no doubt that the case was one of carcinoma, a diagnosis that was subsequently confirmed by our pathologist, Dr R. M. Buchanan, who reported as follows on a portion of tissue taken from the ulcerated surface above the line of the cicatrix:—

"The portion of tissue from the case of Mrs B. is typically cancerous. The cellular elements predominate over the stroma very largely."

The question that had to be decided was whether anything further could be done for the case. As regards local removal, I was quite at one with the opinion already expressed at the Royal Infirmary that it was unjustifiable, because the prospects of complete eradication of the cancerous material were not good, and previous experience had shown me that in young patients, such as the present, the attempt is seldom successful, and, indeed, sometimes seems to hasten the progress of the disease, which assumes an acute and fulminating form, most disappointing and disastrous. Failing, then, local measures, could the disease be attacked in any other way, and by any other channels? To answer this last, it is necessary, Mr President, that I should put before you views that I have for some time held as to the etiology or cause of cancer generally, but more particularly of that of the female mamma. Before, however, doing so, I think it will be advantageous that I should very briefly lay down what I consider is the present state of our knowledge of carcinoma or cancer, so that I may make it quite clear what I mean by that term, and that there may be no difference of opinion as to what it is we are discussing. Well, I

think I put the case fairly when I say that there are certain points in carcinoma on which we are all agreed, and others on which there is great diversity of opinion. I think we are all at one on the following:—

1. That carcinoma is a tumour taking origin in epithelium, and having an epithelial structure.

2. That the essential feature of the disease is the continuous and excessive growth of this epithelium, which invades the surrounding tissues, spreads along the lymphatic vessels, passes from one set of glands to another, and eventually forms deposits in distant organs and parts of the body.

3. That once this proliferation of epithelium has begun, nothing that we know of has the power of arresting it.

4. That if a microscopic section of a carcinoma is made sufficiently thin and stained, certain special cells are observed, which cells, although not fulfilling the *rôle* of Lebert's specific cancer-cell, are yet sufficiently characteristic of the disease, and are now known as "cancer-bodies."

5. That clinically it is a matter of common observation that the younger the patient the more rapid the cell proliferation, and the more quickly fatal the disease; while in many old persons cancer assumes the atrophic or withering form from fatty degeneration and absorption of the epithelial cells, little more being left than a mass of fibrous tissue, with here and there a few cells surrounded by granular debris.

6. That cancer kills either by septicæmia from absorption of unhealthy products, or by hæmorrhage, or by interference with the function of some important organ.

7. That in our present state of knowledge of the nature and etiology of cancer, that the best treatment we can offer our patients is the complete removal of the disease by the surgeon's knife, and that the aseptic surgery of the present day allows this to be done more freely than heretofore, so that very extensive operations are performed nowadays.

There is, however, not the same unanimity of opinion on the two following points in connexion with cancer:—

1. As to the purely local origin of the disease.

2. As to the interpretation to be put upon the structures known as cancer-bodies.

Taking the first point, we find that some hold that the carcinomatous growth has a purely *local* origin—starts, in fact, from an irritation developed locally, and that, if that irritation and its effects are freely removed, the patient is cured (Hutchison). Others, again, teach that carcinoma, though an affection of the solid tissues, as shown in the local cell proliferation it causes, is really a blood disease, and that the tumour is only a local manifestation of a blood affection (De Morgan). Lastly, there is what I may term a third school, who hold that there is a certain state of the system or

of the tissues in which a local injury, such as a blow, will start a carcinoma of the part, and without this local irritation a cancer will not develop (Paget).

Coming next to the interpretation to be put upon the cancer-bodies, a large number of observers, and amongst them men of the highest standing, look upon them as inter-cellular organisms of the nature of coccidia, or psorosperms, as French authors call them, and they regard them as the cause of the cell activity and proliferation characteristic of cancer. One distinguished member of this Society, Dr Russell, has brought out the fact that these cancer-bodies can be particularly well displayed by fuchsin staining, but, if I remember correctly, he looks on them as closely related to the yeasts. Others, however, are not satisfied as to the parasitic nature of these cancer-bodies. They explain them as arising from the embedding of leucocytes within certain of the cells, or, as Klebs puts it, from the fructifying influence of the leucocytes upon them; while others, again, think that they are simply epithelial cells undergoing vacuolation in the course of what is evidently a mucoid degeneration. I confess that of late this latter has been my own feeling.

I must now ask you to allow me briefly to mention to you what has led me to modify still further my views about these cancer-bodies, and also to lean to an explanation of the exciting cause of cancer that is quite opposed to the parasitic theory of the disease. I shall do so as shortly as I can.

It is just twenty years ago that I was asked to take medical charge of a gentleman whose mind was affected, and I went to reside with him at one of his estates in the West of Scotland. My duties were at times exciting, but never onerous, and I had a good deal of leisure to myself. I thought it would be a good opportunity of writing my M.D. thesis, and, after consideration, I decided I would take up the subject of lactation. What suggested it to me was the weaning of the lambs on a large adjoining sheep farm soon after I went down to my patient. Accordingly, I commenced to work at it, getting all the practical information I could about it from the farmers and shepherds round. At that time, however (1876), cerebral localization was being much talked about, and I took up the disease sturdy in sheep instead, as there were a good many cases of it just then. I yet, however, elicited the following points in connexion with lactation that struck me as of great interest:—

1. I found that the secretion of milk, though undoubtedly affected by the general nervous system, had no special nerve supply of its own to control it. Neither section of the sympathetic nor of the spinal nerves seem to influence it. The erectility of the nipple is affected by cutting the latter, but nothing more.

2. It was clear to me that the changes that take place in the mammary gland in the process of lactation are almost identical, up

to a certain point, with what takes place in a cancerous mamma. We have, under both these conditions, the same proliferation of generations of epithelial cells which block the ducts and fill the acini of the gland, but in the case of lactation they rapidly vacuolate, undergo fatty degeneration, and form milk, while in the carcinoma they stop short of that process, and, to make room for themselves, they penetrate the walls of the ducts and the acini, and invade the surrounding tissues. In short, lactation is at one point perilously near becoming a cancerous process if it is at all arrested.

3. I learnt this very remarkable fact, that it is the custom in certain countries to remove the ovaries of the cow after calving if it is wished to keep up the supply of milk, and that if this is done the cow will go on giving milk indefinitely. This fact seemed to me of great interest, for it pointed to one organ holding the control over the secretion of another and separate organ, and thus explained the absence of that distinct nervous control that I pointed out as characteristic of the mamma. Of course, the close intimacy between the ovary and the mamma is well known to all of us, as seen in the absence, as a rule, of the menstrual function during lactation, but I certainly was not aware until then that it was of the nature that it would seem to be, and almost of a distinct control. In our country farmers have not gone the length of spaying cows, as in Australia, but they attain the same end of having a continuous supply of milk by getting rid of all ovarian influence in another way. We know that during pregnancy the ovary is, as a rule, functionless,—that is to say, we have not the indications of its activity in the shape of the menses, and it would seem to be in its turn brought under the control of the pregnant uterus. Farmers knew that their cows after calving usually begin to menstruate every three weeks, and that with the establishment of this function the mammary secretion gradually lessened. They also knew that during the nine months the cow carries her calf she did not menstruate, so to prevent menstruation and lessened milk they put the bull to the cow usually two or three months after the calf is born and when the milk secretion is becoming lessened, the result being that with pregnancy the secretion ceases to lessen and remains copious.

I need hardly say that, though I temporarily abandoned the subject of lactation for my thesis, I did not lose sight of the facts above mentioned, for they seemed to me to point to influences at work in the human system that had not as yet been generally reckoned with or recognised. Above all, I was struck with the local proliferation of epithelium seen in lactation. Here was the very thing characteristic of carcinoma of the breast, and, indeed, of the cancerous process everywhere, but differing from it in that it was held in control by another organ, and could either be arrested by that organ altogether or continued to a further stage, where

the cells became fatty, and passed out of the system not only in an innocuous but nourishing fluid—milk.

Now, gentlemen, I think I am correct in saying that the spirit of modern pathology is this,—that all pathological changes are merely modified physiological ones, that there is no essential difference between the two, and that a knowledge of the forces controlling the one may sometimes give us a clue to the other. I often asked myself,—Is cancer of the mamma due to some ovarian irritation, as from some defective steps in the cycle of ovarian changes; and if so, would the cell proliferation be brought to a standstill, or would the cells go on to the fatty degeneration seen in lactation were the ovaries to be removed? For an answer to these questions I felt I must wait; but, on settling in practice in Glasgow in 1878, I determined to look further into this point of the control the ovaries seemed to have over the function of lactation. Accordingly, I obtained at the end of 1878 a licence for performing the experiment of removing the ovaries from suckling rabbits. Through the kindness of Prof. M'Kendrick, I was able to carry my experiments out at the University laboratory. Time will not allow me to go into them in detail, but I may say that the three cases I tried all confirmed the fact. As long as the young ones were at the breast the milk supply continued, and when eventually they were taken away the milk supply ceased; but the creatures increased very much in size, and post-mortem examination revealed that this was due to large deposits of fat around the various organs, and above all in the lumbar region, where there were masses of pure adipose tissue, showing that the secretion of milk was still going on, but, not being discharged by the usual channels, was deposited in the various tissues of the body as fat.

In the year 1882 a case of uterine cancer, unsuitable for local removal, came under my care, and I thought I would try on it the effect of removal of the tubes and ovaries, as she was willing to submit to any operation. I found, however, on performing abdominal section that the disease had extended so much into the broad ligaments that a satisfactory removal of the appendages could not be accomplished, and I abandoned the operation. She made a good recovery from the laparotomy, so that no harm was done by it; and she died some months later, I was told. With this single attempt to put my views to the test I was for a time content, as I was very unwilling to do anything of the nature of experiments on my fellow-creatures. Further, with the rise and progress of bacteriology, I began to share in the hope that in this quarter a solution of the true nature of cancer would be found; and, with the announcement of the so-called cancer-bodies now generally recognised, I began to think less and less of my ovarian theory of the origin of cancer.

On taking up my work at the Glasgow Cancer Hospital, which I may say has been established not only for the treatment of

cancer in all its stages, but also for the pathological study of the disease, I felt that the position of matters was that our present state of knowledge has nothing better to offer than the surgeon's knife for the cases where the tumour was limited and could be thoroughly removed; but that in inoperable cases, if the so-called cancer-bodies were not parasites at all, but merely cells undergoing mucoid degeneration, it was possible a free administration of thyroid extract might influence the growth, and work through time a cure. Failing this, I thought I might follow up my old line of reasoning, and in cases of advanced carcinoma of the breast in young patients see what effects the removal of the tubes and ovaries would have on the progress of the cancerous growth in the way of arresting the cell proliferation and converting the cells into fatty matter.

Although the breast had been removed, this was the line of procedure I decided to adopt with Mrs B., and accordingly on 11th May she was put upon the thyroid tabloids. They were pushed until their physiological action was made apparent; but, as no appreciable change was seen in the diseased condition at the end of a month, I put it to her husband and herself as to whether she should have done the operation of removal of the tubes and ovaries. Its nature was fully explained to them both, and also that it was a purely experimental one, but that it could be done without risk to life; and that, if it should have no effect on the cancerous process, it would cause her no increase of suffering. She readily consented that I should do anything that held out any prospect of cure, as she knew and felt her case was hopeless. On 15th June I operated, and removed the tubes and ovaries on both sides. The right ovary seemed healthy; the left one was somewhat cystic. Subsequently there was some little trouble with the action of her bowels; but she made a good recovery, and on 28th June was sitting up. *No local application was made to the diseased areas on the thorax. They were simply kept clean with boric lotion, and dressed with protective and boric lint.*

On 12th July the administration of the thyroid tabloids, three daily, was resumed, as I felt that, though I hoped by my oöphorectomy to arrest the cell proliferation and favour perhaps fatty degeneration of the cells, there was present such a large amount of cancerous material that a powerful lymphatic stimulant such as thyroid extract might be useful.

On 19th July, about five weeks after operation, an examination of the diseased areas on the left side of thorax showed, as the Hospital report states, undoubtedly a marked change compared with their condition some weeks ago. The larger mass of disease is much less vascular. It is also smaller, flatter, and altogether less prominent, and the same may be said of all the other secondary foci of disease. The tissues around are also softer and more pliable.

On 1st August it is noted that the local improvement continues, and that the measurements of the largest area of disease are—length, $2\frac{3}{4}$ in.; breadth, $1\frac{1}{4}$ in.; while the depth is hardly appreciable. The colour is a dull yellowish-white, and the vascularity slight. There are five small nodules in the axillary region, which are also diminishing in size and vascularity, though perhaps not so much as the larger growth. Patient's general health and nourishment are satisfactory; and, as she is an intelligent and reliable woman, and interested in her own case, I allowed her to go to Bridge of Allan for a change, and asked her to report herself from time to time. This she did, and, without going into a detailed account of her condition on each visit that she made, I may say that the local improvement continued, and my note on 12th October, just four months after the operation of oöphorectomy, was as follows:—"On examination of the left breast the condition of the tissues is favourable. The most remarkable feature of the case is the yellow fatty look that the former thick bar of cancerous tissue above the scar of the incision for removal of the breast presents. It is to my mind the most striking feature of the case. The cancerous tissue has been reduced to a very thin layer, and is in no way raised above the surrounding skin. In fact, the whole surface is smooth and level, and to the naked eye it seems as if the skin at this part had a yellow look. So distinct is this that one could easily trace out the outline of this yellow-coloured tissue. At places the surrounding skin seems pushing its way into the yellow mass, and the processes of bluish cicatricial tissue are to be noted. The yellowish nodules at the axillary end of the incision are still apparent from their colour, but they seem thinning out. The whole of the tissues on the chest wall are more movable, and the surrounding skin has a clear and healthy look. The scar of the former ulcer above the mammary excision cicatrix is sound, and no new nodules are at present observable. Patient expresses herself as feeling very well, and looks so. She is taking four 5-gr. tabloids of thyroid extract daily."

I need not trouble you with any further detailed account of this patient than to say that eight months after my operation all vestiges of her previous cancerous disease had disappeared, and that I am able to show her to you to-night with a sound cicatrix and healthy thoracic tissues, and that she is apparently in excellent health.

The next case that I wish to bring under your notice is that of Margaret R., æt. 40, married, no family, who was admitted to the Glasgow Cancer Hospital on 2nd September 1895, suffering from a large tumour of right mamma. It had existed to patient's knowledge for five and a half years, and had not been operated on. Her family history was satisfactory, and personally she had

been a very healthy woman all her life. She began to menstruate at 13 years of age, and the menses still continue. At present they come every three weeks.

The account she gave of the appearance of the tumour was that it followed an injury five and a half years ago, that at first it grew very slowly, but that it has increased much more rapidly since she sustained a blow on it from a door nine months ago. Further, she has had continual pain in it as well as in the neck, the pain passing through to the back. It comes on more severely at times, and is of a shooting nature, and at nights keeps her from sleeping by its severity. She has got thinner, but has not felt specially ill or weak.

Her condition on admission, as noted in the Hospital journal, is as follows:—She is pale and worn-looking, and gives the impression of being a nervous and emotional woman. Locally the right breast is occupied by a large tumour, which involves the whole organ, save its extreme lower margin. The tumour is densely hard, but uniformly so, and more or less rounded and smooth. It is adherent to the skin over a large area, this skin being infiltrated and reddened. The mass is somewhat tender. At its upper part there is a distinct cutaneous nodule about half an inch long and one-fifth of an inch broad. The tumour is not fixed below, though there is no great freedom of movement. The skin around the breast, especially at its lower part, shows congested vessels, and is apparently hyperæmic. The nipple is retracted and fixed. In the axilla there are a number of enlarged hard glands rather deep and fixed. The pectoral fold running up to axilla is slightly thickened and indurated. In first interspace some small shot-like nodules are felt under the skin. Also over the clavicle under the skin there are quite a number of them, and they give a rough feeling to the surface of the bone. In the supra-clavicular space, and in the posterior triangle of the neck, there is a wide infection of the lymphatic glands—one especially hard and fixed mass lying slightly above the clavicle. Numerous enlarged glands can be traced as high as the lobe of the right ear. On left side, in anterior triangle of neck, just above inner end of clavicle, there is a small, hard, enlarged gland. There is no œdema of arm, but patient complains of almost constant pain down each side of the arm, and of pain in the neck. Left breast and axilla seem normal. No secondary deposits can be detected in any of the organs of the body. The only suspicious point is that the percussion-note over and just below the right clavicle is impaired, while, anteriorly and posteriorly, the R.M. is somewhat prolonged and hard at apex, and a slight creaking and grating can be heard. Otherwise the lungs are normal.

I learnt from her that she had recently applied for admission to one of our local infirmaries, but had been refused, as the surgeons who saw her considered that nothing in the way of

operation could be done. I was of the same opinion, for the disease was evidently carcinoma, an opinion subsequently verified by Dr Buchanan microscopically, and any attempt to locally remove the disease would be futile, I considered; but after what I had observed in the previous case related to you, I was not without hope that something might be done to retard the progress of the disease, although, from its great extent, as compared with Mrs B.'s case, I was not so sanguine of success. However, I admitted her to the Hospital. What I looked on as the unfavourable feature of the case was the implication of the glands, not only in the neck, where there was a hard and irregular chain of them, matted together and quite fixed, running up the posterior border of the right sterno-mastoid, but also in the axilla, where there was a deep and immovable mass of them. After admission she had to have half-grain pills of opium for the pain, which seemed severe. I put before her the question of the removal of the tubes and ovaries in her case, but she could not bring herself to submit to an operation which I could advise her to undergo as regards the risk involved in it, though not able to promise her with certainty a successful result in the matter of cure. Another point that made me not very keen in urging the operation was that I found she was a woman of highly nervous temperament, and rather addicted to alcoholic excess. After her admission, quite a month elapsed without anything being done. During that time it was clear that the disease was extending, as the journal of 30th September says—"Extension of disease is seemingly rapid"; and again on 2nd October it is noted that "the skin over the prominent nodule above the centre of the upper margin of the tumour is reddened, and that downwards and inwards, diagonally from the nipple, there is another nodule of smaller size; while on outer side of breast, on and beyond the margin of the tumour, lying in an area of congested skin, there are nine separate and distinct small nodules in and just under the skin, the size of peas or small shot. In the right axilla were three cutaneous nodules, and the deep mass of hard fixed glands; while below the centre of the right clavicle was an irregular mass of enlarged glands evidently growing quickly. Above the clavicle in the anterior and posterior triangles of the neck are the glands much enlarged and matted together, and extending in front of and behind the sterno-mastoid as high as the angle of the jaw. On the left side of neck there is present a small adherent nodule between the tendons of insertion of the left sterno-mastoid and an enlarged left supra-clavicular gland."

At this time she felt she was not improving, and consented to my doing anything that I thought would benefit her condition, so, on 3rd October, I removed her tubes and ovaries. The operation was not an easy one, as the uterine annexa were very adherent, thickened, and altered in appearance as well as in situation, the ordinary anatomical arrangements of the parts being quite

obscured. The after-progress of the case was, however, uninterrupted, and at the end of three weeks she has completely recovered. The journal note of 12th October says that it is evident that the cutaneous surface of the tumour is less vascular, the red blush which covered it entirely being very noticeably diminished, while on 14th October it is remarked that to-day patient states very emphatically, of her own accord, that "she feels in a different world," the pain in her breast being so much easier as to be almost gone. She says, too, that she moves her neck more freely and with less pain. In confirmation of the diminished pain, the matron stated that she slept well at night, and had not required any opium since the operation. On 14th October she commenced to take two 5-gr. thyroid tabloids twice daily, as I decided to follow strictly the mode of procedure in Mrs B.'s case. On 9th November, five weeks after the operation, it is noted that the mass above the clavicle certainly seems smaller and less fixed, the neck being now moved freely in any direction. Diminished size and vascularity in the breast tumour are admitted by all who have watched the case to be apparent, though slightly marked, while the nodule at upper zone of breast is superficially distinctly less, and has a yellowish dull colour, quite different from its former glazed and red appearance. On 3rd December, two months after operation, the above nodule, as it continued to show decided atrophic changes, and to shrink, was removed under cocaine, so that it might be examined by the pathologist to the Hospital, Dr R. M. Buchanan. The journal report continues:—"There is now a striking difference in the size of the tumour of the right mamma as compared with its former state, and over the inner and upper parts of the tumour the skin is becoming freer, and can now be pinched up, though it is still distinctly thickened and infiltrated. Formerly it was quite impossible to grasp it at all, so firmly was it adherent to the tumour." The patient's progress was unfortunately interrupted by a septic suppuration which followed the excision of the nodule under the cocaine, and on 9th December I had to open an abscess over the shoulder, the scar of which can still be seen anteriorly. After this she got on well, and the spot where the nodule was excised cicatrized soundly, and can now be seen quite healed. This I thought interesting, as cutting out a nodule from a cancerous breast is very apt to leave a sore that refuses to heal, and even fungates. The report, from the pathologist, of the microscopic appearances of the excised nodule was that there was a great increase in the stroma, and that the epithelial cells were undergoing marked fatty degeneration—in other words, that there was a more cicatricial condition of the tissue, such as we see in the cases of atrophic or withering scirrhus mammæ.

On 10th January Mrs R. was allowed to leave the Hospital to go to the coast for a change. I am afraid that while absent

she did not take that care of her health that I should like, and was remiss in taking her tonic and her thyroid tabloids, and on her return on 25th February, although looking well in herself, I did not consider the local condition so satisfactory, the glands in the neck being more attached to the skin, larger, redder, and more fixed. Over clavicle an increase of infiltration is occurring, while in axilla some fresh small nodules have arisen. The breast itself seems more adherent again to the skin, and redder, and the outlying nodules larger.

On 2nd March I readmitted her to the Hospital, as I was satisfied she was not doing justice to herself outside, and was not allowing the remedial measures that had been employed to have a fair chance. She was kept in bed, and three thyroid tabloids administered daily. Under these measures she improved gradually and seemed to be free of pain, sleeping well without any opiates.

On 7th April there is noted the increased size of the nipple of the right breast. It is of a pale pinkish colour, somewhat glossy, is like a raspberry in shape, and about four times the size of the left nipple. It now stands out prominently from the centre of the tumour, while formerly it was sunken and retracted.

I have brought this patient before you to-night, not putting her forward as a case that is cured, but simply as illustrative of the changes that were noted in Mrs B.'s case, but have now entirely disappeared in her. Thus, in the enlarged supra-clavicular glands you will, if you examine them closely, note the areas of yellowish-white coloration, which indicate, I consider, the presence of fatty degeneration, a condition present in the cancerous masses in Mrs B.'s case before they eventually disappeared. The same can be seen in several of the nodules in the vicinity of the breast, and recently one of them, just above and to the right of the nipple, broke, and has left a yellowish scab with a subjacent ulcer. A comparison of the two casts of the right mamma taken at intervals of five months shows the difference in the size of the mammary tumour at the two periods. Altogether, I am inclined to think that the disease is in a more quiescent stage, and gives some indications of a possible cure.

The last case I wish to bring under your notice is that of Margaret M., æt. 49, unmarried, who was admitted to the Glasgow Cancer Hospital on 2nd December 1895. She sought advice for a large sore of the left breast, but, as you will see, the left breast has been entirely eaten away, almost as if it had been removed by operation, but she assures me that nothing was ever done to it, and that she had had no advice about it until she came to the Hospital. The disease seems to have existed for six or seven years, and to me one of the remarkable features about it is that it has remained so localized, none of the adjoining glands being apparently as yet involved, and no evidence of any secondary

deposits in any organs of the body. I was at first inclined to think it was possibly a tubercular ulcer we had to deal with, but the removal of a portion of tissue from the margin and its examination by the pathologist showed it to be "typically cancerous." The report adds, "There is no evidence of tuberculosis."

As the menopause had occurred with her two years previously, I was not prepared to at once remove the tubes and ovaries, and thought it might be a case where the thyroid tabloids might be alone administered with the view of seeing what effect they would produce, and whether under their administration any of the vascular diminution and atrophic changes seen in the other cases would be seen. Accordingly, the sore was got into as healthy a condition as possible, and on 7th February, when it was clean in its deeper parts, and even showed at places some attempts at cicatrization, the administration of the thyroid tabloids was commenced, and they have been continued since, being pushed to their full physiological effect. During the three months they have been taken I cannot say that I observe any marked effect on the sore. The indurated and infiltrated borders are apparently unaltered, and show no diminished vascularity, while the ulcerated area has increased below and seems extending. By itself thyroid extract seems to have little effect on the cancerous process, and the question I consider that is opened up by this case is, whether in the light of what has been observed in the other cases I should see whether the removal of the tubes and ovaries in this case would be of any service. I am satisfied that it would be a mistake to attempt the local removal of the sore. I am not so sure, if my view that ovarian irritation may be the exciting cause of cancer is correct, whether it would not be right, even though the menopause has set in, to see what effect the removal of the uterine appendages would have. If this is done, and the Society desires it, I will gladly read a note of the case at some future meeting and show the patient.

Gentlemen, although I have already trespassed very considerably on your time, I am going to ask you to bear with me while I place before you what I believe to be the interpretation of the changes seen in the two cases I have brought under your notice this evening. The conclusion I draw from them is this, *that we must look in the female to the ovaries as the seat of the exciting cause of carcinoma*, certainly of the mamma, in all probability of the female generative organs generally, and possibly of the rest of the body. I have felt for some time that the parasitic theory of cancer is an unsatisfactory one in many ways, and that in directing all our energies to working it out we are losing time and searching for what will never be found, simply because it does not exist.

Further, and bearing on this point, I think we are perhaps in error in assigning to the nervous system the entire regulation of

the metabolic changes in the tissues of the body. I am satisfied that in the ovary of the female and the testicle of the male we have organs that send out influences, more subtle it may be and more mysterious than those emanating from the nervous system, but possibly much more potent than the latter for good or ill as regards the nutrition of the body, and, undoubtedly, whatever cancer may be, it is very generally admitted that it is a disease of the nutrition of the part affected.

To make clear to you how I think the ovaries may be the exciting cause of carcinoma in the female, I must ask you for a moment or two to consider reproduction as it takes place in the lowest animals and in the human body. In the former we have no special cells or organs for reproduction. If a hydra is cut into a number of portions, each portion will develop into a separate and complete hydra, as Trembley showed more than a hundred years ago. This would seem to indicate that all the cells of the creature possess equally the reproductive power. As we get higher in the scale of life, the principle of *division of labour* comes into play, and in the human body there is seen at an early stage of development the arrangement of the cells of the body into separate layers, and subsequently the differentiation or specialization of these cells into separate organs and tissues. In other words, these cells become of a higher type. But there is one very definitely marked-off group of cells which retain their primitive condition, and do not specialize into a higher kind of cell—I refer to those whose function it is to reproduce the species. In no way dissimilar at first, they are separated off to form either ovary or testicle as the case may be, and through their agency the continuation of the race is assured. In the ovary we have well demonstrated the active proliferating power of these cells, for from it are derived several epithelial formations—viz., the surface epithelium of the ovary, the epithelium of the Graafian follicles, and the cells of the ovary that we speak of as ova.

Now, I consider, comes in this very important question—When these reproductive cells are set aside for this one function, do they take away from all the other cells of the body their reproductive power, leaving them only the ability to form the special tissue for which they are intended, or do these cells still retain their reproductive power, but have it kept in check and control by the ovaries as long as these organs are healthy? I am inclined to the latter view, though I am aware that it is not in harmony with Weismann's teaching on heredity, which would have us believe that there is a very definite distinction between the *somatic* and *germ* cells, the latter being handed down in unbroken continuity from parents to offspring, and having nothing whatever in common with the somatic cells, they simply having, as it has been very well put, only board and lodging in the human body. I have never felt sure of Weismann's theory. It is no doubt ingenious

and attractive; at the same time it is quite possible that this doctrine of absolute continuity of germ plasma may be incorrect, and, if at variance with what is observed in disease, it shows it to be faulty in a very important respect.

Going, then, on the assumption that the ordinary cells of the body have not lost their reproductive force, but that this latter is held under control by the healthy ovaries, which are simply masses of germinal epithelium, I can conceive it quite possible that any altered secretion of these organs, or any morbid condition of them, might so affect the other cells of the body as to allow their latent reproductive power to come into play, and thus confer on these cells the active proliferating powers of the germinal epithelium. And this is what at present I am inclined to think does take place, and that cancer consists in the epithelium of the part affected taking on the active proliferation which is the marked characteristic of the *germinal epithelium;* and, though I am not yet in a position to assert it as demonstrated absolutely, I have the belief that the special cells seen in sections of cancer, and known as cancer-bodies, will eventually be shown to be special germinal cells corresponding to the ovum cells elaborated by the ovary.

After this idea had occurred to me, I was interested to find that Klebs has come to the conclusion that in cancer epithelial cells become transformed into ovum cells. He attributes this to the leucocytes exerting a fructifying influence upon the cancer cells and causing them to multiply. I, on the other hand, while agreeing with him as to this fact, think that in the female there is some ovarian influence which works the change. It may be an altered secretion, or it may be the migration of cells, it might even be a parasite in the ovarian cells, for it should be borne in mind, in regard to the secretions of the reproductive glands, "that, unlike other secretions, their essential constituents are living cells" (Stewart), but in whatever way brought about, there seems to me a reasonable ground for thinking that the active processes seen in a cancerous tumour are best explained by regarding the epithelium of the part as having taken on the properties and powers of the *germinal epithelium.*

It may be said that in many cases of cancerous disease no changes are perceptible in the ovaries. This may be so, because they may not have been looked for and because they may be of a delicate nature and requiring special investigation. We know that there are certain coarse lesions of the ovary that certainly are of a malignant and infective kind, and are typical of what we meet with in cancer. I refer to those cystomata of the ovary which sometimes burst and infect the whole peritoneum, and even the abdominal organs themselves. I have operated on such cases and had to close the abdomen, as it would have been useless to attempt removal of all the infected parts. I was struck by a

paper published by Professor Alex. Simpson, in one of the recent volumes of the *Edinburgh Hospital Reports*, where such a case had occurred to him, and in the remarks he made upon it he formulates the view that removal of both ovaries seems to predispose to cancer. If this were so, then my theory to explain my success in Mrs B.'s case must fall to the ground, but the point that Professor Simpson has overlooked is that in all the cases he refers to the ovaries were removed *because they were diseased*, and thus these cases in reality support my view that cancer in the female is in all probability due to some altered ovarian condition, which may be either a coarse lesion of the organ or something more hidden or obscure, such as an altered condition of the cells and their secretion. If cases can be shown that have had both healthy ovaries removed and have subsequently been the subject of cancer, then my theory falls to the ground.

Further, if the view I put forward is a correct one, then cancer in the male should be due to some altered condition or secretion of the testicle, for this organ is built up of cells that, at one stage of their existence in early fœtal life, are so identical with those that form the ovary, that for some weeks they are not distinguishable one from the other. Well, I have been looking into this matter, and one or two facts of considerable interest have come to my knowledge, showing that the testicle seems to have the same control over local proliferation of epithelial cells, such as is seen in the ovary and lactation. Thus, in stags the yearly growth of the horns, which is a local cell proliferation, is under testicular influence, for if a deer is castrated its horns do not grow; and what is more remarkable still, if only one testicle is taken away it is only the horn on that side that does not grow. I have arranged with a friend of mine to carry out this season some experiments on lambs, by castrating them only on one side, and to watch if the horn on that side is absent. Also, I am making inquiries as to the existence of cancer amongst eunuchs, for if my view is correct they should not suffer from it.

The only other explanation that might be urged in favour of the removal of the tubes and ovaries affecting the progress of carcinomatous growths, by those who hold the parasitic theory of the disease, is that the operation so profoundly alters the nutrition of the tissues that the parasite no longer finds a suitable nidus for its existence, and so perishes. I confess I think this is not likely, though it does not call in question the utility of the operation.

To recapitulate, I would urge the following points:—

1. That there seems evidence of the ovaries and testicle having control in the human body over local proliferations of epithelium.

2. That the removal of the tubes and ovaries has an effect on the local proliferation of epithelium which occurs in carcinoma of the mamma, and helps on the tendency carcinoma naturally has to fatty degeneration.

3. That this effect is best seen in cases of carcinoma in young people, a class of case where local removal of the disease is often unsatisfactory.

In conclusion, gentlemen, I would say that my paper has been written without the desire of being in any way dogmatic in the matter of the etiology of cancer. I desire it to be if anything suggestive. I am sure that many of you, like myself, while very desirous that a parasite should be found in cancer, as it would thereby render our chances of dealing with the disease more hopeful, have felt that this view does not satisfy the case, and leaves much to be explained. On the other hand, facts such as I have observed indicate, I think, that it is possibly in the direction of an altered condition of the ovary and testicle that we are to look for the real exciting cause of cancer, and if so, the sooner we direct our energies into that channel the better. I know that I have had nothing in the way of great results to show you to-night, but you must remember that I have worked with most unpromising cases, and that when you have present large masses of cancer it is not easy to bring healing influences to bear upon them. They have got beyond control, whereas in an early stage they might have been amenable to treatment. I am not standing here to-night advocating a wholesale removal of tubes and ovaries for carcinoma. My paper is headed "The Treatment of Inoperable Cases," and I am not in a position to ask you to replace the old plan of local removal—which, no doubt, brings us many disappointments, but has had of late years many encouraging results—by a new and untried method. All I feel is that there are grounds for the belief that the etiology of cancer lies, not in the parasitic view, but in an ovarian or testicular stimulus, and that the whole subject requires careful working out. I need hardly say that if that view is found correct it must materially modify our present lines of treatment.

I know it may be said that I am premature in bringing these cases before you, but I am well aware that in the field in which I am a humble worker there are others labouring as equally desirous as myself for the solution of this problem of the cause of cancer, and I am only too glad that they should know any facts that I have observed, and that may give a clue to the direction in which to work. I am also well aware that the views I have put forward to-night are to a large extent problematical; but whether you accept them or not, I am sure you will acquit me of having acted thoughtlessly or recklessly, and will believe that in all I have done I have had some reason for the faith that is in me, and that I have been actuated solely by the motives that guide all of us in the exercise of our profession,—primarily, the interests of those who place themselves under our care; and, secondarily, the progress and advancement of the healing art.

The President said he was sure the Society was deeply indebted to Dr Beatson for having brought before them these very interesting cases, and for his admirable exposition both of the actual cases and of the theory he had advanced in explanation of the grounds on which he had treated, and proposed to treat cancer, more particularly cancer of the mamma. There was no question that the operative treatment had had a marked influence on the progress of the diseased condition of the mamma. Whatever the nature of the change might be, the tumour had diminished most materially, and the patients' health improved. Whether these cases would command their belief in the great doctrine which he had brought forward, that cancerous affections might be due to some irritation from the sexual organs either in the female or in the male,—that cancerous conditions might in all cases be attributable to such a cause was perhaps, to say the least of it, doubtful. There were many irritative conditions of the generative organs which were not associated with cancerous affections of any part of the body, and the association with such conditions would require a large amount of further observation in order to be established. At the same time, he thought Dr Beatson was so far warranted, in that his idea was corroborated by recent investigations, which had shown that the nutritive conditions of the prostate, for example, were very materially influenced by the removal of the testes. That seemed to have been thoroughly proven; but it was a very wide and extensive theory which he had promulgated. He (the President) would be very glad to hear the observations of those able to speak with more authority than he could do on the nature of the theory and the facts which had enabled Dr Beatson to evolve it.

Prof. Simpson said that he agreed with the President that the Society was greatly indebted to Dr Beatson for the admirable paper he had brought before them to-night. They could all heartily endorse his closing sentences that they should regard it as a contribution made in good faith for the purpose of advancing science, and it deserved quite in an unusual way the expression of their thanks to-night. That was not to say, however, that he was prepared to adopt right off Dr Beatson's theory. Various points required explanation; *e.g.*, he told them he made experiments on rabbits, removing their ovaries, and finding that they got fat afterwards. Well, sometimes pigs when spayed might be fattened apart from lactation altogether. In the history of the female, when the ovaries began to lose their function a certain group of women had a tendency to *embonpoint*. That a deposit of fat took place had long been recognised, and that tendency asserted itself on the disappearance of the sexual function. Further, Dr Beatson had referred to his (Prof. Simpson's) paper, which had not quite regard to the point brought before them. His point was this:—That the patient who came under observation in the ward was judged to be subject to ovarian tumour. On opening the

abdomen they found two tumours. Both ovaries had undergone cystic degeneration. There was an unusually large flaccid wall in the case of the left. The liver had quite distinct cancerous nodules in it. The patient recovered from the operation, but died a few months later from the cancer that they had seen in the liver. He did not know if cancer had ever been seen before in that particular way. On looking into the relation of ovarian disease to cancer, one found that a considerable proportion of patients operated on for ovarian tumour had died afterwards from cancer. Spencer Wells had called attention to it. On looking into the matter lately more particularly, he found that, when both ovaries were removed, patients died in a considerable proportion of cases from cancer which developed sometimes in abdominal organs, sometimes in thoracic and other viscera. He did not remember the exact figures at present. But if, say, 4½ per cent. deaths of women in any year were from cancer, then some 28 per cent. of those from whom ovaries had been removed died of cancer. Therefore when the ovaries had been removed, especially both, there was a special proclivity to cancer. Olshausen and others supposed it due to engraftment of peritoneum with some epithelial cells left there from operation; or that perhaps the tumours themselves, supposed to be innocent at the time, really contained some malignant element or malignant proclivity. His (Prof. Simpson's) case seemed to show that the tumours might be perfectly innocent, because the diseased ovaries were carefully examined, and the degenerations were of the simple cystomatous character, but associated with carcinomatous degeneration in the liver, and that led him to make the suggestion that abeyance of function in the ovaries, still more the removal of them, gave proclivity to the development of cancer. Dr Beatson's paper to-night did not affect Sir Spencer Wells' statistics. He (Prof. Simpson) quite well believed that the ovarian function modified in quite a distinct fashion the tendency to change in the mamma, and the cases of Dr Beatson were of the highest interest, especially the first case; but they had to keep in view that the patient had had other treatment as well; and while the thyroid extract had value in many directions, it was quite possible that it might exert an influence in this case, perhaps all the more when the ovary had been removed. There were various elements in the system that required to be taken into consideration. The ovaries, he had no doubt, exerted part of their function by their influence on the nervous system, also partly by their chemistry, whatever that might be. In the *Centralblatt für Gynäkologie* a writer had recently drawn attention to cases where women had suffered from giddiness, flushings, and distresses of the climacteric. He first administered ovarian extract, ovarian tabloids in some form, and had got one of his assistants to make experiments in transplantation of ovaries from one side to the other in rabbits, engrafting in

peritoneum or in cellular tissues. The ovaries not only lived, but continued their functional activity. Their attention was thus called to the circumstance that the ovarian influence on the system—in this he hardly agreed with Dr Beatson—was not purely nervous, but was largely chemical, whatever the secretion or product might be. But that was a long way from saying that a change in ovarian function should start epithelial cells in other organs, even in an organ so closely allied to the ovary as the mamma, on a career of ovulation or development of ova. It was a kind of exaggeration, to say the least of it, to speak of cancer cells, although there was the high authority of Klebs for it, as being ova. He did not think any one would suggest that if conjunction could come about with spermatozoa from another individual that these would have any true ovular value whatever. At the same time they had a great deal to learn yet in regard to the development of cancer, and possibly also in regard to the development of embryonic structures generally. A very interesting discussion was in progress in a German Gynecological Association in regard to a matter discussed in London last week, of which they had seen no example in Edinburgh, viz., a deciduoma malignum, first described as a sarcomatous growth developed in decidual tissue; but some of his countrymen had alleged that this was a misnomer, and that it was not the decidua that underwent degeneration, but that it depended on some fragments of ova,—some villi of the chorion that remained in the uterus, which had gone on growing with a kind of independent growth, sometimes spreading into the vagina and going as far as liver and lungs; so that the kind of cells found in cancerous tissues ought not to be spoken of in such terms. At the same time the paper was one of great interest and would set other minds working, and they hoped that Dr Beatson would carry out his experiments. In that last case, *e.g.*, supposing the thyroid extract were mainly to receive the credit of it, it seemed to him a different variety of cancer. It was quite possible that thyroid extract might operate differently in different patients. In this case the woman was beyond the climacteric, a point to be carefully considered, because if it was a healthy ovarian action that was in some way stimulating the cancer growth—he understood that was the idea in Dr Beatson's paper—the fact remained that there was more tendency to the development of cancer after the ovaries had ceased functional activity.

Dr W. T. Black said that having once been a sanitary officer in the army he took a different view of cancer. From its extreme rarity in the army and navy, and among the Jews, who all have their butcher meat inspected before cooking, he deduced its prevalence in the civil population to the accidental use of infected meat. On inspecting the veterinary journals for some time back, he had found numerous cases related of malignant diseases in the domestic animals. In the human subject its

prevalence primarily along the digestive tube from mouth to rectum should lead to the same suggestion of its origin. Dr Beatson's view was highly interesting, being founded on physiological and pathological grounds. It was a hopeful paper.

Prof. Chiene remarked that he need not say he was very pleased indeed to rise and corroborate all that Prof. Simpson had said and what the President had said with reference to the value of this paper. It was valuable from the fact that Dr Beatson had been looking into this for twenty years before publishing anything upon it. He had taken care to go very slowly and gradually into it. As regards the theory, he (Prof. Chiene) was not going to enter into it. He could not follow it, could not understand it; but he would have them look at that preparation there, which was undoubtedly a carcinoma of the mamma, and he would say that every practical surgeon in the room, as he looked at that preparation and that woman there, was bound in inoperable cases of cancer to give a trial to removal of the ovaries along with the administration of thyroid extract, explaining to the patient what he was doing. There was a patient in his ward at present who had come from a distance wanting an operation. There was great pain and discomfort. The arm was swollen. The axillary glands were pressing on the vessels. The tumour was firmly attached to the chest wall. The difficulty in the case was that she was 40 years of age, still menstruating, but near the end of the menstrual period. Now, was he not justified in telling that woman what he had seen, and giving her an opportunity of judging for herself? Otherwise they knew what the result would be. She would die a painful death. The arm would swell, and the poor woman would be miserable for the rest of her life. He certainly thought, from what he had heard to-night, that he, as a practical surgeon, was justified in recommending the operation which Dr Beatson had recommended for cases inoperable by other means, and by which patients had been saved. He intended to try it when occasion arose; if this woman wished it he intended to try it on her. With regard to the second case, they had only to look at the cast taken before treatment and the cast recently taken. No one could say there was a cure; but there was such an improvement that he thought it could very well be put side by side with the first case.

Mr Joseph Bell said that since he saw so many inoperable cases at the Longmore Hospital, this paper had given him the deepest possible interest. The first case he thought one of the most remarkable cases he had seen in his life. He knew Dr Beatson well. His diagnosis of cancer was thoroughly good. Other people had seen it. And yet that woman was cured. He did not say it would not come back. He quite agreed with Prof. Chiene that such a case ought to have a chance either of thyroid treatment or excision of the ovaries, or both. Of course,

they all knew that the younger the patient the more inoperable the cancer was likely to be. The worst case he ever saw was that of a girl of 23. There was one operation. She recovered from it, but had a recurrence, and died in twelve weeks. She was a patient of Mr Syme's. He (Mr Bell) had her breast in his collection. The cases that came under his notice were generally inoperable and in old people, and his friend Dr Beatson would excuse him if he said that he had seen a good many very like No. 2, in which in the gradual process of shrinkage of the whole woman the glands diminished and the tumour diminished without any special treatment. No. 3 was very remarkable, but he had seen one or two even more remarkable. He watched one old lady in private for nearly fifteen years. She ulcerated, but did not lose flesh much, and not a gland was affected. There was no doubt of its being cancer. She died eventually of bronchitis. He was not so much impressed, therefore, with Nos. 2 or 3. But No. 1 was a remarkable bit of information, and he would like to add his own little quota of thanks for the admirable manner in which Dr Beatson could teach. He was able to follow every word, and that was not the case with all the papers one heard in that room.

Dr James Ritchie said he did not think Dr Beatson ought to be discouraged from operating on a case that had passed the climacteric. For some months past he (Dr Ritchie) had been studying a disease in which the ovaries seemed to have a trophic effect on bones and muscles, viz., osteomalacia. Fehling believed that the disease was a reflex trophoneurosis, having its source in the ovaries. Dr Ritchie did not believe that this expressed the whole cause, but the ovaries played a very important part. Many cases were cured by removal of these organs. Several cases had occurred after the climacteric in which the disease had made continual progress, but was cured by removal of the ovaries; one was cured about the age of 70. They would watch with very great interest the development of Dr Beatson's work. Dr Beatson had given them further proof that the generative organs had a profound influence on nutritional changes in the system generally. He did not think that they were warranted in going further than that.

Mr Harold J. Stiles said he need not say with what extreme interest he had listened to Dr Beatson's remarks on the treatment of cancer. He should like to congratulate him on the very clear way in which he had put his case, and also the very modest and cautious way. If he had betrayed any incaution, he (Mr Stiles) thought it was only in the field of hypothesis. One could not dispute the facts after seeing the first case. Dr Beatson was kind enough to show him the case about Christmas time, and even at that time he failed to discover any cancer. He did not believe that at present there was any cancer there. There were one or two little nodules, but these he had often seen. A little keloid

tumour often developed at the seat of the wound. It was only the other day that Mr Chiene removed a carcinoma of the mamma, one or two nodules in which were undoubtedly carcinomatous, but others consisted of thickened cicatrix. He examined all the nodules with much interest. Some were undoubtedly cancer, but others were doubtful. He did not mean that the first case was not a case of cancer, but only to show that the little nodules were not cancer. He had not an opportunity of seeing the specimen removed at the time of removal of the ovaries until to-night, and from a cursory examination he had no hesitation in saying that it was a case of carcinoma. To come to Dr Beatson's theory, he (Mr Stiles) agreed with him in not regarding the so-called cancer organism as a parasite. During the last six months he had had some cases illustrating this in a very remarkable degree. They were not to be found in every case, but if one examined a large number of tumours, now and then they got hold of a specimen which showed them exceedingly well. He had photos. which he hoped to show either to this or to some other Society, with the hope, he would not say of proving, but of substantiating, his disbelief in these so-called cancer bodies. If, then, they went back to the foundation and asked what the cause was, he was afraid he did not feel inclined to attribute its outbreak to any influence on the part of the generative organs. He believed the disease broke out independently altogether of the genital organs. He was not surprised to hear Prof. Simpson say that cancer was very common in women whose ovaries had been removed. If the healthy organs had an influence like this, why should the disease in the mamma be so localized? Why should it not break out over the whole mammary area? He did not think Dr Beatson should use the word ova. It was simply that the cells referred to were more embryonic in type, and went on proliferating. Each tumour had more or less its own kind of malignant cell. All malignant cells were not exactly the same, but varied much in different cases. There was a birth-rate and death-rate of cells. They went on together. In the more slowly growing, in the atrophic tumours, and more especially in the ulcerating tumours, the death-rate was almost as rapid as the birth-rate, so that the tumour growth was very slow. He thought the influence of the removal of the ovary was simply to turn the scale between birth-rate and death-rate. It might check the former, and at the same time encourage degenerative changes or death-rate. It was much more likely to do that in the case of the mamma. He considered the first case a cure; but he believed the reason was that she was a young woman and had recently borne children. They knew that the ovaries had a decided influence on the functional mamma. He did not think they could expect that removal of the ovaries or testicles would have any curative effect on malignant tumours growing from other than the generative organs. He thought the treatment was well

worthy of trial, more especially in younger women, and he thought also it should be tried on elderly women. It might diminish the activity of the proliferation and increase the death-rate, and so prolong the life of the patient.

Dr R. A. Lundie said that, in common with all who had spoken, he had listened with extreme interest to Dr Beatson's paper. He thought they were indebted to him for taking them so fully into his confidence, for giving them the whole history of his thought and work on this subject. He felt that that gave to his paper very much greater vividness and interest than it would otherwise have possessed to him (Dr Lundie). The position in which they stood with regard to all cases of cancer, except those which could be completely removed with the knife, was a very distressing one. They had to say to the patient that, according to the current view, cancer was incurable. They took away all glimmer of hope. He had noticed in recent years occasional cases in medical journals, apparently undoubtedly cancer, that had recovered. He hailed these cases with pleasure, as enabling him to say that such cases occasionally did recover. They had all seen to-night one case of undoubted cancer that had recovered, and he thought that would help them to try not to take away all hope from cancerous patients. In the treatment of cancer and all other diseases the medicine of most potency of all was, in his opinion, the medicine of hope; and Dr Beatson attacked these cases of his, of which they had seen the results to-night, not only with new treatment, but above all, with the feeling of hope in the success of his attempt. He (Dr Lundie) did not think they were at all in a position to say yet whether removal of the ovaries or the administration of thyroid had a decided influence on the cases. He thought it quite conceivable, at least, that the hope which from his hope Dr Beatson had succeeded in infusing into his patients had, at all events, a most important influence on the result; and he thought they were especially indebted to him for showing that there were cases of cancer which did recover, so that they might try all in their power, by his or other methods, to induce hope in their patients.

Dr Norman Walker said he would make just a few brief remarks. One was to express satisfaction that it was only in inoperable cancers that Dr Beatson recommended this treatment. One saw so many cases in which other treatment had been tried before the surgeon had his opportunity that it was very desirable to point out clearly that this method should be tried only in cases beyond reach of the surgeon's knife. The second point was in connexion with the third case shown to-night. That case seemed to him to be one of rodent ulcer; as did also Mr Bell's, and not ordinary cancer. (Mr Bell—"Mine wasn't.") Mr Bell's case had, at any rate, the clinical history of rodent ulcer. Dr Beatson's had the features of it, both clinically and microscopically. He thought

that in that case removal might have been quite well done with the knife.

Dr Beatson, in reply, said he thanked them very much for the very kind reception they had given to his paper. They had entered into the spirit in which he hoped it was written. He need not go into detail, because the different speakers had gone over much the same ground. First of all, as to Prof. Simpson's reference to Spencer Wells' case of carcinoma following removal of both ovaries. He thought if these cases were looked into they would find that the carcinoma had appeared within a few weeks of the operation, at any rate in several cases. That afforded ground for saying that in all probability the disease was there at the time the operation was done. Cancer did grow rapidly, but not, like a mushroom, in a single night. He had looked into these cases. One died five years after operation, but they were not told how long the cancer had existed before she died, which was a very important point. The disease ran a slow course in some, in others a more rapid one. He thought that in the majority of those cases of Spencer Wells' it had been early and rapid, and also in the abdomen. He thought there were one or two in which mention was made of secondary deposits in bone and other organs, but it was not said whether they were of the same kind as those in the abdomen. He quite admitted with Prof. Simpson that his view about carcinoma was problematical and theoretical. But it was a point which might be worked out. At the present day much was being done on the subject of development. An ovary that was to all appearance healthy might have changes, especially chemical, going on that were not apparent. They must have a standard to which the ovary, when examined microscopically and chemically, must come up before they could say it was distinctly healthy. In regard to Mr Bell's remarks on the second case, in which he said that shrinkage of the tumour accompanied shrinkage of the patient generally, he watched that point. The patient on admission weighed 7 st. 8 lbs. Her weight at present was 7 st. 7 lbs., so that practically it had been a local shrinkage. Whether the shrinkage was entirely due to the atrophy of glandular substance alone he could not say. Each mamma was undoubtedly atrophied, but the general diminution in the size of the tumour, as apparent from the two casts, was very striking. He quite agreed with Prof. Chiene that the practical point was the one they had to look to. At the same time, he would not have it go forth that he stood there to advocate removal of the tubes and ovaries or testicles for cancer. It was a dangerous thing. But he hoped that cases on which it might be tried would be carefully noted, and the results carefully published. He had done the operation in some cases of carcinoma of the uterus. It took months before these cases showed appreciable change. They were all cases in which local removal was out of the question. To one he was giving pure

ovarian extract. She was brought in very much exhausted from hæmorrhage last October. Since removal of the ovaries there had been a very remarkable condition of the tissues. The hæmorrhage had ceased, and the tissues had assumed a dried-up, almost shrivelled appearance, with freedom from discharge. Her weight was going down, and she had undoubtedly that yellow cachexia that they saw in those cases. Yet she had been keeping remarkably free from offensive fœtor and sacral pain. Altogether he was watching the case with great interest. He had not felt he had sufficient grounds in the case third shown for doing anything local. There were cases in which cutting into the tissues did undoubtedly favour the spread of the disease. If there were other cases he would, if the Society were willing, be very pleased to bring them before it.

Meeting X.—June 3, 1896.

Dr Argyll Robertson, *President, in the Chair.*

I. Exhibition of Patient.

Mr C. W. Cathcart showed a patient who, he said, illustrated a point which was unknown to him until he had seen one or two examples of it this winter. In certain cases of tertiary syphilis the sore presented characters which one was always accustomed to consider tubercular—viz., marked undermining of edges of sore and irregularity of the scar. Of the other two he had seen, one was on the arm and the other on the neck. If one had not really been on the look-out for it in the ward, one might have taken it for tuberculosis. They healed up without any trouble under antisyphilitic remedies. The patient had only been in the ward two days, and there was a great difference in the sore. The history was that three years ago he had syphilis; and now he had got, besides this curious undermining of the skin, a gumma forming on the clavicle, and another on the scalp.

II. Demonstration of Instruments.

1. *Dr Logan Turner* explained and demonstrated Dr Kirstein's method of autoscopy of the larynx and trachea, or direct examination without the aid of the laryngeal mirror, and read the following notes:—

The principle of this method of examination consists in bringing the interior of the larynx and trachea *directly* under the eye of the observer without the use of the laryngeal mirror, and consequently