THE LETTSOMIAN LECTURES

BEING

PRACTICAL OBSERVATIONS ON CANCER OF THE BREAST.

[WITH SPECIAL ILLUSTRATIONS.]

*Delivered before the Medical Society of London, April 2nd, 1900.*BY SIR WILLIAM BANKS, LL.D., F.R.C.S.,
Liverpool.

LECTURE III.

THE PARASITIC AND INFECTIVE VIEWS OF CANCER.

It would be out of place here to enter into a detailed history of the early searchings after truth in this matter, for, however praiseworthy, nothing definite came of them. The first notable step in advance was made by Dr. Russell, of Edinburgh, in 1890. In the *BRITISH MEDICAL JOURNAL* for December of that year will be found a paper by him modestly entitled "A Characteristic Organism of Cancer." Seeing that they were well displayed by staining with fuchsin, he termed them "fuchsin bodies." The attaching of men's names to all sorts of silly little discoveries and operations is much to be deprecated, but so important a move in the right direction might well be commemorated by designating them Russell's bodies. He described them both as intracellular and extracellular, and, what is of much importance, he held them to be blastomycetes, a variety of the saccharomycetes or yeasts.

The general opinion was distinctly against him, but in the *Annals de l'Institut Pasteur* for 1890 appeared a paper by Professor Soudakewitch of Kieff, "Parasitisme Intracellulaire des Neoplasies Cancéreuses," describing and figuring bodies obviously very similar to Russell's bodies, and in the same year (*Journal of Pathology*) a very important contribution by Armand Ruffer containing confirmatory evidence. Preparations illustrating both these papers were shown to Metchnikoff, and that distinguished pathologist gave it as his opinion that the bodies were "parasitic protozoa." This at once altered the *venue* from the vegetable to the animal kingdom. In this country the fuller investigation of the parasite has been very ably carried on by Mr. Plimmer, of St. Mary's Hospital. In the so-called Cancer Number of the *Practitioner*, already referred to, will be found a review by him of the whole question up to April, 1899. It is a model of clear unbiased writing, and the description of the general structure of cancer and methods of examination most admirable.

For a considerable time, my friend and old pupil, Dr. Buchanan, formerly Assistant Demonstrator of Physiology in University College, Liverpool, and now Physician to the Liverpool Consumption Hospital, has been investigating these parasites, and he recently (January, 1900) contributed a paper upon them to the Pathological Section of the Liverpool Medical Institution. I have had the pleasure of going over all his work with him, and I cannot help coming to the conclusion that the bodies described as parasites are bodies *sui generis*—special organisms—and are not degenerations of cell protoplasm or of non-parasitic cell inclusions. It seems as if the almost daily increasing evidence is now too strong to permit of our simply listening to those sceptical persons who merely deny their existence without having ever hunted for them or even seen them. Where these bodies come from, where they go, or what they do, is nothing to the point for the moment. The question is, Do they exist? I have been convinced that they do.

A very reasonable question on the part of the doubter is this: Cannot these said parasites be found in structures other than carcinomata and sarcomata? After Russell's pronouncement, many investigators said that this was so, but, as time has gone on, they have failed so far to prove their case. On the other hand Plimmer's most extensive inquiries have gone all the other way. During the six years preceding 1899 he had examined microscopically 1,278 cancers, taken as they came from many parts and organs and of all possible varieties; out of this number he has found the parasite in 1,130 instances; of the cases in which he failed to find them,

63 were densely fibrous and atrophic, actually in process of spontaneous cure, and 23 were so degenerated that they would not stain. He has examined by his method gummata, typical histoid tumours of all kinds, tuberculous growths, glanders, actinomycotic growths, epithelium which had been artificially irritated, and normal tissues; and never in man, in any instance, has he seen any intracellular or extracellular bodies like the parasitic bodies he describes or which had their reactions.

The opinion of Metchnikoff that the parasitic bodies were protozoa was for awhile the dominant one with probably all investigators. But Russell's view that they were blastomycetes has been revived by two eminent Italian *savants*, whose names are at present in the mouths of all who are interested in the question. One of these is Professor Sanfelice, Director of the Institute of Hygiene in the Royal University of Cagliari, and the other is Professor Roncalli, of Rome—the former working largely from the experimental point of view and the latter from the histological. Now, their opinion is distinctly that the parasites are blastomycetes. Plimmer, however, still holds to the protozoal view as the better for provisional acceptance until more is known. It is clear that the question is not a vital one, and that, at all events, it does not in any way hinder the experimental work now going on, and having for its object to prove that these bodies, when separated from the other constituents of a cancerous growth when cultivated and the cultivated products implanted in animals, will be reproduced in these animals and originate cancer in them.

What, then, is the appearance of these parasites? As I have seen them in Dr. Buchanan's sections they are round cells, possessing what would correspond to a central nucleus, a surrounding mass of indefinite protoplasm, and an enveloping cell wall or enclosing capsule of some kind. These general features have been brought out by different investigators in different ways, but the plates published by them show a great uniformity in the general appearance of the parasite.

The method of display pursued by Dr. Buchanan is this. As fixing reagents he used (1) a saturated solution of perchloride of mercury, in 0.75 salt solution, in which the specimen was placed for 24 hours, and then transferred to methylated spirit to which had been added a few drops of saturated solution of iodine in alcohol; (2) bichromate of ammonia; (3) Fleming's solution. As regards staining processes he found the Ehrlich-Biondi triple stain very good, but apt to fade too quickly. Another method: Stain with Ehrlich's acid hæmatoxylin, wash in distilled water, then "blue" in distilled water rendered faintly alkaline with lithium carbonate. Again, another: Stain with saturated aqueous solution of fuchsin S. or carbol fuchsin S., then dehydrate in absolute alcohol strongly tinged with aurantia, clear in clove oil tinged with aurantia, and mount in xylobalsam. The size of the cells is very various. Plimmer reckons them at from 0.004 mm. to 0.04 mm., or even more. I have seen some which have a circumferential row of fine dots disposed just within the capsule, and others in which the nucleus has an appearance of being split up and the portions somewhat twisted. Again, there are some which have most delicate lines, radiating from the centre to the circumference. Some writers have described and figured quite an array of different forms of the parasite, but personally, possibly owing to my want of skill, I have not been able to make sure to myself of more than the varieties mentioned above. As far as regards their mode of reproduction, Plimmer is of opinion that it is by division into two by the process of budding, or the nucleus divides into two while the capsule throws in septa which meet between these two halves, and so divide the parasite into two. Dr. Buchanan thinks they spore, but in his specimens I have not been able either to make sure of anything definite as regards this point. I think Plimmer's immense experience makes his opinion the one to be received at the moment at all events. In a recent interview which I had with him, he most kindly showed me his specimens, and gave me the very beautiful one which I show you to-night. Those of you who may care to turn back to his article in the *Practitioner* will see that Fig. 1, Plate 1, represents this in the most faithful manner.

I also saw specimens identical with Fig. 8, Plate 2, where the nucleus, becoming somewhat oval, had then divided into two.

I would like here to quote some of Dr. Buchanan's conclusions: "By careful comparison I found in these bodies a constant recurrence of definite structure, a beauty and regularity of design marked by stages of enlargement and growth with many evidences of a truly developmental process. Degenerative processes are essentially irregular, with no definite recurrence of form and with no beauty of design. I entertained two ideas:

"1. That if these bodies were truly parasitic from without two conditions were necessary for their entrance into cells: (a) either they must be capable of movement constituting change of locality, or (b) the (epithelial) cell must be capable of movement sufficient for it to enclose the parasite, or a combination of both conditions at one time.

"2. That, as a cancer is progressive in growth, the parasite must develop *in situ*; if it do not, then, if it be an exciting cause, the growth must eventually cease."

He then goes on to express his opinion that the parasite is capable of movement, and that its appearance in many cases is due to definite change of shape, such as is generally recognised in amoeboid cells; that in its young state it enters a cell and there develops, dividing and redividing into a number of spore-like bodies, which may eventually fill and swell the capsule into a cyst, in which the spore bodies lie like a mulberry mass, and the cyst resembles a sporocyst; that these spores eventually find exit from the capsule, probably through a special aperture, whence they infect other cells; that the parasites are probably fertilised to ensure reproduction, although the method has not yet been demonstrated; that they are living entities introduced from without to whom the cancer cell occupies the position of a host.

It has been matter of wonder why, if the parasites are such well-defined bodies, they have not been noticed long ago. But the fact is that on any average tumour they are comparatively few in number and have to be searched for, and, a most important point, they are not to be found in the hard centre of a scirrhous mass at all, but only at the growing edges of a tumour. The centre is largely composed of fibrous tissue and cells in course of degeneration. It is the inactive part of the tumour, where Nature is making an effort at cure. All the vigorous proliferating epithelium is at the margin, and it is only inside active cells that the parasite is found. Then the processes of staining required to display the parasites are very delicate and complicated, and are really only the product of the last few years.

Ignoring, then, the question of whether the parasites are to be considered as yeasts, and to be named blastomycetes, or whether they are protozoa or coccidia, has anything experimentally been done with them? I will begin with some recent observations made by my friend, Mr. Keith W. Monsarrat, Assistant Surgeon to the Liverpool Infirmary for Children, in order, if possible, to confirm the observations of Sanfelice, Roncali, and others. Having obtained a fresh cancerous tumour, he made into its outlying parts a series of some twenty or thirty incisions with a sterilised knife. Scrapings were taken from the edges of these incisions and inoculated on glucose-agar. From this subcultures grown on wort-agar and wort-bouillon were employed, and were plentiful after twenty-four hours' incubation at 37°C. From this again guinea-pigs were inoculated by intraperitoneal injection of 1 c.cm. of a culture 48 hours old. They showed no symptoms of illness, and were killed at periods from two to six weeks after the injection. On opening the abdomen the omentum and general peritoneal surface were found to be studded with nodules from the size of a pea to that of a pin's head; to the naked eye nodules were visible in the lungs, liver, spleen, and kidneys. The parasites were found reproduced in the cultivations and in the inoculation growths. Mr. Monsarrat employed the following staining process for them, which he believes very accurately differentiates them and picks them out from surrounding structures. Fix with saturated solution of perchloride of mercury and harden in alcohol; place in acetic carmine for half an hour and then wash in water, followed by two minutes in methyl violet 1 per cent., and two minutes in picric acid 25 per cent., then transfer to clove oil until no more

methyl violet will come off. The primary growths on the peritoneum are composed of proliferated endothelial cells. The organisms are present in considerable numbers, some within the cells, but most outside them. This is contrary to what occurs in primary cancers in the human body, where the extracellular parasites are few in number compared with the intracellular. In many of the nodules there is some attempt at the formation of a connective tissue capsule, and in others the central parts are broken down. In the lungs the nodules are made up of endothelial cells; in each nodule there are organisms present, and usually centrally situated. In the liver and spleen the nodules are very similar in appearance to the primary omental growths. The origin of the cells composing them is doubtful. In the kidney, where again the nodules are of endothelial origin, the cells are derived from those lining the Malpighian corpuscles and tubules. In no case was there any alveolar arrangement of the cells or any appearance resembling the endotheliomata of man.

With regard to the cultures, the appearances agreed in the main with those described by Sanfelice and Plimmer. Fresh specimens from them are spherical, from 4 to 10 microm. in diameter, and in most instances take an aniline chromatin stain diffusely.

I think we may regard Mr. Monsarrat's work as quite confirmative of what Mr. Plimmer has detailed with such admirable clearness and precision in the Cancer Number of the *Practitioner*. There his own experiments on the same lines are to be found, and there the fullest directions for staining according to the method which he has found the best, and which produces specimens at once beautiful and convincing. To anyone who wishes to know about the parasites of cancer this article is invaluable. But what has been briefly touched upon in this description regarding the results of the inoculation of animals with the parasite has been far outdone by Sanfelice, who began his researches in 1894, and whose last conclusions were published in 1899. Dr. Russell has written a most admirable summary of Sanfelice's last experiments.¹ If you will allow me, I will give you the barest outline of them. Sanfelice was investigating the characters of blastomycetes, when he found that some of them were pathogenic to animals and caused the growths of tumours in them. He selected a special organism for purposes of experiment, which he denominated the "*saccharomyces neoformans*." Inoculations of guinea-pigs with it produced a general infection of the animal with the formation of tumours, consisting partly of masses of the parasite and partly of proliferation of the cells of the part. The general microscopic appearance of the new growth seems to have been pretty much what Plimmer has recorded as the result of his experiments, and what Monsarrat has described as produced in his, as we have just noted. Bodies were perceived having the appearance of what the earlier investigators described as coccidia (protozoa), and what Russell had described and regarded as blastomycetes. These bodies as found in the tumours differed somewhat from those in the cultures as regards appearance. He inoculated with pure cultures of the *saccharomyces neoformans* certain cats, and found typical forms of Russell's bodies in the tissues. To make sure of the matter, he investigated the tissues of normal cats, but no parasites were found in them, although this occurred invariably after the injection of the special blastomycetes. The cats, in short, were very susceptible. Cats, by the way, are liable to cancer, although not nearly in the same proportion as geldings. Next, the experimenter proceeded to pass the blastomycetes with which he was experimenting through a number of dogs—that is, from dog to dog. In two instances something very specially tending towards malignancy was produced. In one case the dog was inoculated in the two posterior nipples with the blastomycetes which had been passed through several dogs. The animal died ten months later with tumours in both teats, showing retraction of the nipples and tacking down of the skin, with secondary growths in the inguinal and abdominal glands. All these showed the type of adeno-carcinoma. In the other case, a dog which was inoculated in both testicles died with adeno-carcinoma of these organs, with secondary growths in the neighbourhood of the penis and glands. If all this be true, it will constitute a remarkable advance in

the history of the parasitic view of cancer, more especially when it is considered that these experiments were conducted with cultures made from an organism obtained from a fruit juice. These he found much more active than the cultures made from cancer. I have seen a microscopic section of a tumour produced after passing a blastomyces through many animals, but I confess I could not recognise in it a typical carcinoma, although it assuredly tended in that direction, and had a certain amount of connective tissue in its composition.

If I were now asked what is the impression produced on my mind by what I have seen and read about the so-called Russell's bodies or cancer parasites, I would put it briefly thus: (1) There is plainly a structure of a definite nature which is found in man at the marginal or growing edges of carcinomata. It is not found in any healthy tissue nor in any other neoplasm except sarcoma. It used to be regarded as a protozoan or coccidium, but most investigators seem now to regard it (as Russell did at first) as a blastomyces which is a form of saccharomyces, reckoned to be a development stage of certain fungi. Most parasites are inside the epithelial cancer cells; some are outside. There is a strong probability that they spread by sporing, although much more knowledge on this point is wanted. How they get into or originate in the human body is not known, and what action, if any, they have upon the epithelial cells has not yet been demonstrated. But, admitting an action, it is doubtless that of an irritant to them, which causes them to proliferate. (2) There is no doubt that from these parasites cultivations in certain media can be made, and that, when such are injected into animals, growths are produced in them, mostly of a fatal character, which contains these parasites. (3) Sanfelice asserts that he can produce the same results in animals by the injection of a blastomyces obtained from a fruit juice. Furthermore, that by passing the parasites through a series of animals a tumour is at length produced which is an adeno-carcinoma. On these points I cannot say anything of my own knowledge beyond what I have said a few sentences back regarding a section of such a tumour which I have seen. (4) That as yet there is no clear and indisputable proof of the infectivity of cancer; that consequently any strong assertions to that effect or any statements that surgeons infect wounds by disseminating cancer products through them during operations, are not justified. That, on the other hand, the possibility of this infectivity must not be sneered at or treated lightly in view of the remarkable information which has been obtained during the last few years concerning the cancer parasite and certain of its attributes. This is not the time for positive assertion. This is the time for patient experiment and the collection of facts.

OPERATIVE PROCEDURES IN MAMMARY CANCER.

When I left my medical school in 1862 my recollection of operations for cancer of the breast was that they consisted of local removals of the tumour of the most imperfect description. If any glands were affected, not merely in the case of breast cancer, but in all instances, *ipso facto* that was a sufficient bar to any operation. In 1867 Mr. Moore, of the Middlesex Hospital, whose experience of cancer in that institution was great, wrote what is now an almost historic paper on Inadequate Operations in Cancer, which set a great many minds thinking, inasmuch as it was a revolt against constituted authority. It made me think too, so that I became deeply interested in the question. I hope that in what I am about to say I may not appear to be too self-satisfied with what little share I have been enabled to take in it. But this does seem an opportune occasion for presenting to the medical public a review of the history of the operations for mammary cancer, seeing that the changes that have occurred in a very brief period have been of profound moment, and what has been done only a few years ago seems wonderfully soon forgotten. Well then, during the ten years following Mr. Moore's protest, great progress was made by the persevering and bold manner in which Lister and Bryant and Annandale and Heath and other able surgeons attacked secondary glandular growths, and demonstrated that all that was required for their safe removal was careful, patient dissection. Among English-speaking surgeons Samuel Gross, of Philadelphia, was the next who made a notable move in favour of the free removal of breast

cancers,² and to him much is due for the earnestness and enthusiasm with which he pushed his cause, as will be remembered by all those who, like myself, had the privilege of being his friend.

My first paper on the subject was read to the Lancashire and Cheshire Branch of the British Medical Association in 1877, and was termed "A Plea for the more free Removal of Cancerous Growths." In it I said "I can point to several cases of hard scirrhus of the breast in which the whole mamma has been removed which have now remained well for years. There is no mystery about this; all that is required is to take the precaution to cut freely through sound textures. But this is exactly what surgeons will not do. I have over and over again watched a surgeon remove a breast, and have seen him make his incisions so as to include a little elliptical bit of skin containing the nipple, after which he would proceed to scoop out the breast so as to get nice flaps, which fell easily together and covered in the wound. He never considered that, in the brawny dotted skin over the tumour were already implanted the germs of the disease, and that the most dangerous thing he could do was to leave plenty of skin; one of the tissues above all others along which cancer cells love to make their way. I can safely affirm that in the great majority of operations for cancer which I have seen—and they are not a few—the cancer has not been removed root and branch. When the tumour has been carefully looked at after the excitement of the operation has been over, there has generally been found some spot where everyone wished that a bit more had been taken away. Not long ago I was looking over some old illuminated manuscripts. Among other pictures of the tortures inflicted upon the faithful there was one in which a horrid wretch was represented with a great crooked knife mangling the body of a devout female. He had slashed off one breast, the former site of which was represented by a great bleeding chasm, while with one hand he was clutching the remaining organ, and with the other was preparing with one sweep to lop it off also. I could not help wishing that I had this picture to hang up in the students' room in our school; not in *terrorem*, but as my idea of what most amputations of the breast ought to be; not a niggling, half-and-half digging out of the tumour, but a radical sweeping away of the whole mamma, skin and all." . . . "I have a very strong notion that it would not be a bad plan when removing a breast always to clear out the axillary glands whether we can feel anything wrong with them or not, and I mean to try it. At present we only remove the glands when we can distinctly feel them enlarged, and that frequently through a great deal of fat and a thick skin. How do we know they are not already infected? And then the patient disappears, and doesn't return till the axilla is full, and the great vessels are so mixed up with the disease that we despair about being able to clear everything out and so leave her alone." . . . "By disabusing the lay mind of the unreasonable dread of operation, the public will come to regard an operation for cancer in the same light as they do an operation for stone or for cataract; a bad job at the best they will say, but still with a prospect of ultimate cure. They will come to us when their cancers are small and request us to remove them, and, if we could only extirpate them freely, what might we not expect? Have you ever imagined what the results would be if all cancers were thoroughly excised when they were no bigger than peas?"

Twenty-three years have elapsed since I said the above words, and I have very little more to say now.

In 1880, three years after the above appeared, Mr. Samuel W. Gross's admirable work on *Tumours of the Mammary Gland* was published. In it occur the following sentences:

"This point demonstrates that the wiser course is to explore the axilla, even if its glands cannot be detected from without. By clearing out that cavity in all operations we may naturally expect to diminish, if not prevent, further local dissemination, and remove foci of general infection."

My next papers were read before the meeting of the British Medical Association in Worcester in August, 1882 (On Free Removal of Mammary Cancer with Extirpation of the Axillary Glands as a Necessary Accompaniment) and to the Liverpool Medical Institution in the same year. They are in purport the same paper. The results of 46 cases were given. May I quote a paragraph or two from it? I said, referring to

my paper written in 1877: "In this paper it was asserted that surgeons do not remove cancers of the breast. After an interval of five years I feel obliged to reassert the statement. Surgeons, as a rule, do not remove cancers of the breast. They persuade their patients that they do, and they almost persuade themselves. But there is always that little bit which they leave behind, and which they fondly hope will not grow because it is such a little bit. Alas! that so little leaven should leaven the whole lump!"..... "In the present paper a principal object is to advocate the removal of the axillary glands, as well as the breast, in *all* cases, whether we can feel them enlarged or not—in fact, to make a clearing out of the axilla a necessary part of the operation for removal of the breast. I have been quietly practising this for three or four years, having been driven to the conclusion that it was the right thing to do by discovering that, even in those cases where certain glands could distinctly be felt enlarged, when the axilla was opened small ones were discovered which, although most palpably affected, were quite incapable of being affected from the outside. From this it follows that the usual fumbling in the axilla, which is commonly practised, is all nonsense. When the glands are as big as walnuts any first-year student can tell that they are affected. But what I wish with all earnestness to insist upon is that there is a stage—the early stage—when they are certainly infected, although to the touch, through layers of fat and skin, nothing amiss can be felt. When the dresser at the end of his report upon a case of mammary scirrhus, winds up with the stock phrase, 'Glands in the axilla not affected,' I immediately stop him with, 'How do you know? All that you should say is that you can't feel them, which is a totally different matter.'"

In 1887 I read a paper to the Harveian Society of London, entitled "Extirpation of the Axillary Glands a Necessary Accompaniment of Removal of the Breast for Cancer," with details of 82 cases. I there reiterated the points I have just alluded to, and added some others which had struck me:

"Having cleared out the breast, I do not cut it adrift, but leave it suspended by its upper and outer end. I next open the axilla, and in so doing carry away a continuous mass of tissue upwards from the mamma with the object of including in it all the lymphatic vessels going from the breast to the glands. This is an important step and should never be neglected."..... "I have tied the axillary vein at least half a dozen times, and there was not the slightest difference afterwards between these cases and others, so that it seems certain that careful ligation thereof is not in the least a dangerous proceeding. Is it necessary to divide the pectoralis major? Very seldom, I think. I imagine that, out of my 82 recorded cases, it was not done more than 2 or 3 times. If the floor of the axilla has been widely opened, and if the arm is properly drawn up, you can feel with your finger quite well up to the clavicle, and pinch or pull down glands from the very top of the cavity. Dividing the pectoral muscle adds somewhat to the tediousness of the operation, but that is all."

Seeing that my paper, although urging the employment of a much more extensive operation than that usually performed, did nevertheless seem to prove that greater immunity from recurrent disease was obtained thereby, I had hoped to have received some little encouragement from the London surgeons present at the meeting. Exactly the reverse was the case. In the *International Journal of the Medical Sciences* (1888) is a most important paper by Mr. Samuel Cross, in which he said: "In the remarks on Banks's paper at the Harveian Society in the spring of 1887, in which that surgeon reported 82 cases of extirpation of the breast along with the axillary contents, Bryant and Butlin questioned whether the complete operation afforded better results than the incomplete; and Butlin, Pick, and Cripps held that clearing out the axilla in every case was neither necessary nor judicious, although Pick asserted that the skin should be freely removed. Owen was the only member who gave cordial support to the procedure urged by Banks, the prevailing opinion being that the operation was attended with an excessive mortality, and did not lessen the risk of recurrence." Mr. Thomas Smith, by the way, thought that the patients lived longest who had never been operated upon, and that it was a question for consideration whether a local excretion of cancer did not render patients less liable to constitutional disease. I returned to

Liverpool very much downcast after this meeting, but I am glad to think that after thirteen years Mr. Edmund Owen and I are here to-night to rejoice in the complete triumph of the principles for which we contended on the night of March 3rd, 1887.

MR BUTLIN'S VIEWS.

"Before I enter on the discussion of this question let me say that no personal feeling exists between myself and those who hold the doctrine of extensive operation, and that, if I speak at any time warmly on the points under discussion, it is because the subject is one in which I take the warmest interest and on which I hold very decided opinions. I entertain a great respect for both Dr. Gross and Mr. Banks, and only mention their names as the distinguished leaders of a school of surgery from which I venture to disagree." Mr. Butlin was good enough to make these very kind remarks in 1887, and I would like here entirely to reciprocate them. After this very generous introduction, however, he made some criticisms, which I am sure he will forgive me reproducing here, along with some remarks made in 1881 and 1898.

1881. *From the Transactions of the International Medical Congress.*—Mr. Butlin said that he quite agreed with Dr. Gross that in the case of any tumour of suspicious character the sooner it was removed and the more freely it was removed the greater were the prospects of complete recovery; and, further, he agreed thoroughly with Dr. Gross that in the case of carcinomatous tumours it was highly desirable to extend the operation so as to examine the axilla if there were any doubtful fulness, even though no definite enlargement of the glands could be detected.

1887. *From the Operative Surgery of Malignant Disease.*—Mr. Butlin wrote that no prolonged search need be made for the lymphatic vessels lying between the breast and the axillary glands. If there were not any distinct cords to be felt running up into the axilla, it was not necessary to search for the lines of lymphatic vessels or to remove a large quantity of the fat and other tissues in order to ensure their removal. Referring to Gross and Banks, these surgeons were in the habit of cutting off the mamma with all the skin which covers it, dissecting off the fascia from the pectoral muscle, searching for outlying lobules of the gland in the surrounding tissues, carrying the incision into the axilla, whether enlarged glands were previously felt there or not, and clearing out the glands and all easily removable structures. Both surgeons were impressed with the absolute necessity of very extensive operations if the patient was to be afforded a reasonable chance of complete recovery from the disease. Both surgeons had pushed this teaching so far that it had already gained many adherents, although he believed rather among foreign than among English surgeons. Indeed he was surprised to find how strong an opinion was expressed against it by some of the English operating surgeons at a recent meeting of the Harveian Society in the discussion of a paper by Mr. Banks. As he entirely disapproved of this wholesale way of treating cancers of the breast, and believed it to be unscientific and needlessly cruel to many women, he would venture to criticise it as thoroughly as he could. The operation recommended by Banks was not such an operation as that which was frequently practised in London and provincial hospitals, but was much more sweeping, for it included the whole of the skin over the breast, and not merely over the tumour. In fact the breast was, so to speak, lopped off as if the operation had been performed with a sword or a carving knife. Now, he quite admitted that, if the entire breast was to be removed in every instance of cancer, the operation should be of this sweeping character. But he was strongly of opinion that the practice of removing the whole of the breast in every case was theoretically and practically wrong. Probably a long time and much false reasoning were required to induce the acceptance of the doctrine of the necessity of the removal of the mamma for cancers of small size limited to a part of it; and he was afraid a still longer time and much good reasoning would be required to convince surgeons that the practice was radically wrong. So far as the removal of the glands was concerned, by all means let them be removed when they can be felt enlarged and to be within reach of removal, and in cases in which there was a doubtful fulness which was not apparent on the other side. But let no theoretical considera-

tions lead them further than this, and on no account let the axilla be opened in search of diseased glands of the existence of which there was not the least outward or palpable sign. In reply to all the arguments he had brought forward, Mr. Banks had at his command a very strong answer—namely, his results. He quite admitted—and with very great pleasure admitted—that, so far as his later results were concerned, they were the best he had ever seen from operations performed with the knife. The superiority of these statistics may be accounted for, not by the fact that the entire breast was removed in every instance, but because Mr. Banks was in the habit of sweeping away the disease itself and the surrounding textures in a fashion at which most operators would be horrified.

1898.—In the number of the BRITISH MEDICAL JOURNAL for November 26th, 1898, Mr. Butlin wrote that he had read Professor Halsted's paper on the Results of Operations for the Cure of Cancer of the Breast (1894), and his results appeared to him to justify a thorough reconsideration of the subject of the operative surgery of cancer of the breast. This operation was suggested by a paper in *Langenbeck's Archiv* (1889) on the Causes of Local Recurrence of Cancer after Amputation, by Dr. Heidenhain, and he had studied it along with a paper on the Surgical Anatomy of the Breast and Lymphatic Glands, reprinted from the *Edinburgh Medical Journal* (1892), which the author, Mr. Harold Stiles, had been so kind as to send to him. Both authors had come to the conclusion, from a general and microscopic examination of a number of breasts which had been removed, that cancer of the breast rapidly extends back towards and then into the pectoral fascia, even when there appears to the naked eye to be a considerable interval between the posterior border of the tumour, and that the pectoral fascia is often very difficult to separate satisfactorily from the surface of the muscle. Looking back on his own clinical experience, in order to see how far it seemed to be consistent with the investigations of Heidenhain and Stiles, he could quickly remember several cases in which recurrence had taken place in the substance of the pectoral muscle, after the removal of a cancer which appeared to be quite separate from the pectoral fascia, and even after the fascia had been roughly dissected off. (Then followed a long description of Halsted's operation, with Mr. Butlin's expressions of belief in its value.)

Now, if any gentleman will take the trouble to turn up Mr. Butlin's work on *The Operative Surgery of Malignant Disease*, he will find that the last chapter is devoted very largely to three things: (1) A subdued admiration of a certain Dr. Bougard, of Brussels, who used caustic paste for the destruction of cancerous mammae as well as of the axillary glands down to the vicinity of the main vessels and nerves; (2) an advocacy of the mere removal of malignant tumours of the breast and nothing more, seeing that the evidence for the removal of the entire breast was only very partially true; and (3) a very sharp attack upon the doctrines then being promulgated by Dr. Gross and myself. Dr. Gross presently wrote these words, "Had so good a pathologist as Butlin been in possession of the facts derived from my practice and that of Kuester, he would scarcely have given utterance to so pernicious a doctrine—a doctrine which, we fear, will so influence English surgeons as to counteract in great measure the recent advances that have been made in the treatment of carcinoma of the breast."

For myself I offer no criticism. I have merely quoted the *ipsisima verba* of Mr. Butlin in 1881, 1887, and 1898, and leave them to the consideration of the surgical profession.

HALSTED'S AND OTHER OPERATIONS.

In the BRITISH MEDICAL JOURNAL of December 8th, 1898, is a very full description of Halsted's operation. It contains fifteen paragraphs, with very minute details, down to the grasping of the tumour with the left hand, and the scrupulous dissection of the axilla with "the sharpest possible knife." The only points of any fundamental importance in which it differs from the operation ascribed by Mr. Butlin to myself (just quoted) consist in the removal of the sternal portion of the pectoralis major and the division of the pectoralis minor with reflection of that muscle. This is described as necessary for the complete removal of the glands in the axilla. I desire entirely to dissent from this view. I do not believe it necessary, still less that the entire pectoralis major need

be removed as is now Dr. Halsted's present contention. If the arm be properly manipulated and the great pectoral wall dragged upwards and inwards, the very topmost point of the axilla can be cleaned out. Moreover, if there is any doubt at all, the great pectoral muscle can be cut across with very little trouble, and then stitched together with catgut sutures, as I have every now and then done. In the *Transactions* of the American Surgical Association Dr. Halsted relates that he now almost invariably cleans out the supraclavicular region, and that our rule ought to be, "Operate on the neck in every case." For this purpose, however, he does not divide the clavicle as he did five or six years ago; but if it were very desirable to remove the supraclavicular contents in one piece with the axillary contents and breast, one need not hesitate to excise, if necessary, even the entire clavicle. He considers the neck involvement is no more serious than the axillary, and that the neck can be cleaned out just as thoroughly as the axillary region. He believes that in the near future we shall remove the mediastinal contents at some of our primary operations. It seems a pity he did not throw in the liver and lungs when he was at it.

With every wish to defer to the opinions of those from whom I differ, I feel sure that they will permit me to state my views as plainly as many of them have stated theirs. Some little apology I may plead, namely, that I have removed the breast and cleaned out the axilla over 250 times. It is a locality with which I am necessarily fairly familiar. Well, I have stated that I do not consider that carrying away the entire great pectoral muscle and dividing the small one are at all necessary for the satisfactory removal of the axillary contents. Moreover, I have given up attempting to get any good out of clearing out the supraclavicular region. In former days, when I thought I could remove any cancer, I did a good many cases, but never one survived. I do not say I will not do it again if an apparently suitable case occurs. I did it twelve months ago on a lady who had a small cancer situated about an inch below the clavicle, which had directly affected the cervical glands, but it was a failure as usual. Dr. Coley, in the article Cancer, in *Twentieth Century Practice of Medicine*, says: "The only positive evidence Halsted's statistics afford us as to the value of this operation of cleaning out the supraclavicular fossa rests upon two cases in which the operation was done secondarily, and in which the patients remained well three and three and a-half years after the operation. In view of what I have already said regarding the failure of a three-year limit as establishing a cure of the disease, we cannot consider the question as settled with much stronger evidence." *Natürlich!* My notion is this, that when cancer has laid hold of the supraclavicular glands it has got such a grip as to be past extirpation. It is like a burning house. When the fire is beginning even some buckets of water will put it out; when it has got to a certain pitch a whole fire brigade will not quench it. Moreover, an experienced fireman will tell you the minute he sees the fire whether he can put it out or not. Bennett May, in his *Ingleby Lectures* for 1897, says that he thinks it very doubtful whether some of these modern extensive operative procedures confer any additional immunity from recurrence at all commensurate with their severity. It is clear that in both classes of operation the majority of fatal cases die of internal metastasis without external recurrence, and against these no operation can protect. Dr. Matas, of New Orleans, at a meeting of the American Surgical Association in 1898 said, speaking of Halsted and Meyer's operations, that he firmly believed that surgery had there nearly reached, if it had not already attained, its maximum expression of effectiveness, beyond which it is absolutely impossible to advance without great risk to life, or with any further expectation of increasing the chances of permanent recovery. I entirely agree with Mr. Marmaduke Sheild when he says that the swing of the pendulum of surgical opinion is generally too extreme, and has now turned from too slight to too extensive operating.

One word more in reference to removing the great pectoral muscle. Do cancerous recurrences take place in the substance of that muscle? Are they found in the interval between the great and small pectoral muscles? Taking the latter query first, I have never seen such an occurrence. Referring to the first, I believe that when the great pectoral muscle is the site of recurrent growths they creep into it from

the subcutaneous tissues and pectoral fascia. They do not primarily originate in the muscle. There is, therefore, no need for its removal on that ground. No man in this country has had more experience of the pathology of cancer than Mr. Plimmer, from his connection as pathologist to the cancer Hospital. In a conversation with him the other day he perfectly concurred in this view, and, moreover, stated that he had never known anybody live long after distinct invasion of the supraclavicular glands.

I think I need say very little concerning the operation of Berger, which means removing the upper limb along with the scapula. It may be dismissed with the remark I once heard made by my friend Mr. Owen, about another matter slightly altered from the French Marshal's observation about Balaclava: "*C'est magnifique mais ce n'est pas la chirurgie.*" Surely there are hypodermic syringes and morphine still in the land.

In the operations, the statistics of which I am about to present to you, my plan has been this: I never make an angular, triangular, or multiangular incision, for they are a farce. I draw a circular "ring fence" with a knife outside the apparent limits of the mamma if the tumour is central, and, if it is lateral, the "ring fence" should bulge well beyond the tumour. I then slip the blade on the flat beneath the skin for about 2 inches, and undercut it all round. This skin is then well pulled back at successive points by an assistant, and where the undercutting stops there the removal begins. So one gets a long way beyond the rough limits of the mamma proper. Then the tumour is removed, and the fascia dissected off the great pectoral muscle. The great advantage of the undercutting is that one can get the fascia off the muscle as far inwards as the sternum, down to the ribs and up almost to the clavicle. As Mr. Butlin mentions, the removal of the fascia was one of the points dwelt upon by Dr. Gross and myself very many years ago. The axilla should then be opened by an incision just under the edge of the great pectoral and the upper end of the gland and the whole contents of this cavity, as far as they can be held together, carried away in one mass. The importance of this lies in the fact that recurrence in the axilla takes place for the most part not in glands that are left behind but in fragments of lymphatic vessels. I feel convinced of that. A good plan is to get hold soon of the axillary vein, and then there is nothing else to trouble about. One must take the matter very patiently, getting everything out of the cavity that can be got, and cleaning up the digitations of the serratus magnus. On no account do I introduce the "sharpest possible knife" into the armpit, nor any knife at all. I use a dissecting tool, two very long strong pairs of dissecting forceps, and curved probe-pointed scissors. But everybody according to his taste, except beginners, who ought not to use knives here.

A drain hole should now be made just above the edge of the latissimus dorsi. Concerning the covering in of the great wound, it is remarkable what can be done towards pulling it together by very strong wire and by the help of lead buttons. The very top of the axillary incision should be kept open. I put a little bit of gauze into this aperture and into the drain hole below. I wash my hands well with hot water and soap before operating. I pour hot water constantly over the wound during the operation, and see that it is dry from blood at the end. I use no disinfectant fluids, and I dress the wound with a piece of perchloride gauze and a pad of absorbent wood wool over it. I have used this plan of operating for over twenty years, and have only altered it slightly in the following points: After reading my paper to the Harveian Society in 1887, so soon as I had recovered from my first discouragement, I proceeded to remove more skin than ever, for I became more and more convinced that skin is the really dangerous tissue in this matter of cancer of the breast. Thiersch grafting was not then invented, and so I had great difficulty in covering the big wounds. It was then that I began the practice of undercutting, which I have demonstrated and taught ever since. If a wound of great size is left uncovered nowadays, we have the Thiersch to fall back upon. I have used it both at the time of operation, and afterwards when granulation was going on, and I prefer the latter plan.

The accompanying photograph shows the result of the operation just described. The patient was a small woman, the carcinoma about the size of a walnut, and the affected

glands few in number and not big. It was, therefore, a proceeding of quite moderate dimensions. Just as I finished a sudden thought struck me to have it photographed. My house-surgeon, Dr. Arkle, ran to his room for his camera, and took a rapid picture. If it is not very successful the flurry of doing it in much haste must account for that, nevertheless it will give an idea of what I regard as the "complete" operation in opposition to the "redundant" variety. This lady was up and walking about on the twelfth day with the wound entirely healed without a drop of suppuration and without a degree of rise in temperature.

A view is now being taught that there is great danger of infecting the wound with the knife that has made the exploratory incision through the tumours. I have seen no evidence whatever to this effect, and the extreme difficulty of getting the strongest cancer juice to reproduce anything in any cultivation medium is not in favour of it. If the operator is afraid of such a contingency let him wash his knife.

Before leaving this subject I wish to make a few remarks, and I find great difficulty in making them, so much do I fear lest what I may say should be regarded in any way as derogatory to the talents of two gentlemen for whom I have a very great admiration—Dr. Heidenhain and Mr. Stiles. In 1887 Mr. Butlin said with reference to mammary operations: "It was only during the writing of this book that I became impressed with the necessity for reconsideration of the subject." In 1898, in consequence of having read Dr. Halsted's paper, the results "appeared to me to justify a thorough reconsideration of the surgery of cancer of the breast." In this he was fortified by the work of Heidenhain and Stiles. Now, the researches of these gentlemen on the minute lymphatics of the breast and neighbouring tissues and the way cancer creeps into them are of the greatest interest from an anatomical point of view.

But I do not see that they have done anything very specially to influence the views of men who are occupied practically in endeavouring to eradicate mammary cancer. Dr. Coley says: "Still later, in 1889, Heidenhain, by his very careful and scientific investigation as to the manner in which cancer cells extended from the original focus of the disease into the neighbouring parts demonstrated the facts, many of which Moore and Gross had already from clinical observations previously assumed as true. The gist of Heidenhain's results may be summed up in a few words. A cancerous tumour that has become sufficiently large to be recognised clinically has

Fig. 1.—Parasites in a mammary carcinoma; two in centre well defined. $\times 375$. Reichert $\frac{1}{4}$ oil immersion. Dr. R. J. M. BUCHANAN.

Fig. 2.—Parasites in an axillary gland secondary to mammary carcinoma. $\times 375$. Reichert $\frac{1}{4}$ oil immersion. Dr. R. J. M. BUCHANAN.

Fig. 3.—A single well-defined parasite from infiltrated skin over a mammary carcinoma. $\times 375$. Reichert $\frac{1}{4}$ oil immersion. Dr. R. J. M. BUCHANAN.

Fig. 4.—Group of parasites in a mammary carcinoma. $\times 350$. Mr. KEITH W. MONSARRAT.

Fig. 5.—Coverslip preparation of parasites cultivated on artificial media from a mammary carcinoma. $\times 75$. Low power. Mr. KEITH W. MONSARRAT.

Fig. 6.—Lung of guinea-pig showing parasites, the result of the inoculation of cultivation, Fig. 5. $\times 350$. Mr. KEITH W. MONSARRAT.

already scattered its infective cells throughout the entire breast, the neighbouring chain of glands and in many instances the fascia overlying the pectoral muscle." Quite so, but there was no surgeon in 1889 who did know this as a matter of fact, although it had not actually been demonstrated by the microscope. It was not that surgeons did not know his—it was that they were not bold enough to act on their knowledge. In justice to myself, and to the memory of my late friend Gross, I think it rather hard that a certain group of surgeons should date the origin of the modern operative treatment of cancer from the microscopic researches of Heidenhain and Stiles, and the labours of Halsted and themselves.

TABLE OF STATISTICS.

CASES IN WHICH THE DISEASE PROVED FATAL BY LOCAL RECURRENCE.

Series 1.—(a) Cases which lived between 1 and 3 years.

Name.	Age.	Duration of Life.	Name.	Age.	Duration of Life.	Name.	Age.	Duration of Life.
		Y. M.			Y. M.			Y. M.
1 Mary N. (?)	51	18	Julia K. 46	34	Ann O. 44	1	9	
2 Miss B. 50	6	19	Mrs. L'E. 60	1	35	Jane C. 56	2	0
3 Eliz. P. 50	6	20	Miss M. 41	1	36	Mary A. M. 45	2	0
4 Rachel K. 61	6	21	Amy H. 35	2	37	Annie T. 44	2	0
5 Mrs. C. 50	8	22	Mrs. G. 45	3	38	Jane R. 39	2	0
6 Miss S. 46	9	23	Miss B. 55	2	39	Margaret J. 53	2	0
7 Eliz. M. 47	9	24	Mrs. H. 55	2	40	Eliz. P. 52	2	0
8 Miss B. 54	10	25	Ann H. 42	4	41	Miss W. 56	2	0
9 Mrs. H. 35	10	26	Mrs. C. 40	4	42	Miss L. 36	2	2
10 Mrs. V. W. (?)	10	27	Mary W. 45	6	43	Mrs. F. 42	2	1
11 Han. R. B. 41	11	28	Eliz. G. 40	4	44	Ann W. 54	2	3
12 Mary A. K. 33	1	29	Jane H. 48	6	45	Jane H. 49	2	4
13 Anna-Mc. 39	1	30	Mrs. A. 52	6	46	Jane H. 38	2	6
14 Eliz. C. C. 41	1	31	Mrs. B. (?)	8	47	Ann W. 54	2	6
15 Mrs. K. (?)	1	32	Janet J. 46	8	48	Miss S. 40	2	6
16 Eliz. D. 58	1	33	Margaret O. 46	8	49	Mrs. M. 50	2	6
17 E. H. 67	1	0						

Series 1.—(b) Cases which lived between 3 and 14 years.

50 Eliz. R. 40	3	0	56 Mrs. W. (?)	4	0	62 Mrs. T. 48	5	0
51 Cath. H. 52	3	2	57 Mrs. M. J. 43	4	0	63 Mrs. T. 34	5	7
52 Mrs. F. 43	3	3	58 Mary J. T. 47	4	0	64 Mary A. 67	5	7
53 Eliz. C. 48	3	5	59 Marg. W. 40	4	2	65 Mary M. 36	7	3
54 Jane T. 56	3	6	60 Mrs. F. 57	4	5	66 Mary S. 50	12	0
55 Mary C. 48	3	3	61 Jane B. 50	4	6	67 Mary D. 40	14	0

Series 2.—Cases alive and remaining free from disease.

(a) From 1 to 3 years.

Name.	Age.	Duration of Life.	Name.	Age.	Duration of Life.	Name.	Age.	Duration of Life.
		Y. M.			Y. M.			Y. M.
1 Mrs. C. M. 40	1	5	Miss R. 54	1	10	9 Annie R. 53	2	5
2 Margt. B. 49	1	6	6 Mrs. S. 56	2	0	10 Miss C. 56	2	5
3 Mrs. T. 40	1	8	7 Amelia H. 58	2	4			
4 Miss T. (?)	1	10	8 Jane E. 57	2	5			

(b) From 3 to 21 years.

11 Mrs. E. 56	3	0	24 Mrs. G. 50	5	6	37 Eliza J. 69	10	0
12 Miss S. 45	3	0	25 Kate B. 52	6	0	38 Miss R. 62	10	0
13 Miss D. 24	3	0	26 Mrs. C. 45	6	4	39 Miss C. 40	10	3
14 Martha H. 46	3	0	27 H. W. 68	5	4	40 Mrs. T. 55	11	0
15 Miss A. J. 45	3	0	28 Mrs. F. 47	7	0	41 Margt. F. 41	12	7
16 Annie V. 42	3	0	29 Louisa B. 43	7	0	42 Mrs. B. 46	14	0
17 Mary M. 38	3	0	30 Margt. B. 50	7	2	43 Miss R. 52	14	3
18 Mrs. S. 34	3	0	31 Louisa S. 55	7	6	44 C. R. 53	14	6
19 Mrs. B. 50	4	3	32 Mrs. B. 51	8	0	45 Mrs. R. 50	14	10
20 Mrs. L. 50	4	10	33 Margt. C. (?)	8	0	46 Miss M. 44	16	2
21 Mrs. L. 62	4	3	34 Margt. S. 63	8	2	47 Mrs. M. 42	17	10
22 Mrs. W. 54	5	0	35 Sarah C. 64	8	5	48 Maria R. 49	21	
23 Cthrn. S. 31	5	5	36 Miss H. 39	8	6			

Series 3.—Cases where the patient died without local or general return as far as known.

	Name.	Age.	Duration of Life.	Cause of Death.
1	Martha K. ...	48	2 months	Died in asylum—mania (?).
2	Mary W. ...	56	1 year, 4 months	Acute pleurisy.
3	Alice Ann D. ...	37	1 year	Heart Disease.
4	Margaret J. ...	36	1 "	Childbirth.
5	Mrs. L. ...	43	1 "	Acute meningitis.
6	Ellen W. ...	41	1 year, 6 months	Some nerve disease.
7	Mrs. A. B. ...	49	1 " 8 "	Acute pneumonia.
8	Mrs. F. ...	45	1 " 10 "	Sudden coma and collapse.
9	Margaret B. ...	62	1 " 9 "	(?)
10	Marjory G. ...	32	2 years	(?)
11	Margaret C. ...	46	2 years, 6 months	Bronchitis and heart disease.
12	Mrs. C. ...	57	2 " 11 "	Pneumonia.

13 Mrs. B. ... (?)	2 "	6 "	Acute tuberculous phthisis (doctor's report).
14 Anna C. ... 56	2 "	8 "	Liver cirrhosis.
15 Mrs. A. ... 48	2 "	7 "	Phthisis and hæmoptysis.
16 Elizabeth M. ... 54	3 years		(?)
17 Mrs. W. ... 38	3 years, 8 months		Diabetes.
18 Barbara McG. ... 50	3 "	9 "	Acute bronchitis.
19 Mary J. ... (?)	3 "	8 "	Do.
20 Eliza M. ... 42	4 years		Some cerebral disease.
21 Margaret H. ... 63	4 "		Renal and general debility.
22 Miss C. ... 61	5 years, 6 months		"No secondary cancer" (doctor's report).
23 Elizabeth W. ... 55	5 "	6 "	M
24 Mary L. ... 43	6 years		Pneumonia.
25 Miss A. ... (?)	8 "		Apoplexy.
26 Ann J. ... 40	8 years, 2 months		(?)
27 Mrs. L. ... 56	9 years		Heart disease.
28 Agnes R. M. ... 48	9 years, 3 months		Apoplexy.
29 Miss T. ... 50	16 years		Influenza.
30 Mrs. W. ... 53	18 "		"Weak heart" at 71.

Series 4.—Cases with no local but general recurrence.

	Name.	Age.	Duration of Life.	Sites and Causes of Recurrence.
1	Mary B.	49	3 months	Liver
2	Jane S.	26	8 "	Lung
3	Mrs. H.	42	1 year	Cancerous pleurisy
4	Caroline O'B. ...	50	1 " 2 months	Pelvic organs
5	Jane C.	42	1 " 2 "	Humerus
6	Edith T.	40	1 " 4 "	Liver
7	Mrs. G.	53	1 " 6 "	Lung
8	Annie H.	42	2 years	Stomach—probably cancer
9	Mrs. L.	61	2 " 1 month	Spinal column
10	Sarah P.	35	2 " 6 months	Lung
11	Mrs. E.	40	3 "	Lung—probably cancer
12	Mrs. B.	49	3 "	Stomach
13	Jane C.	51	3 " 1 month	Rectum
14	Mrs. B.	(?)	3 " 10 months	Uterus
15	Miss B.	56	3 " 10 "	Lung
16	Elizabeth J. ...	(?)	4 "	Uterus and rectum
17	Barbara G. ...	40	4 "	Lung—probably cancer
18	Mrs. E.	(?)	4 "	Lung
19	Mrs. B.	33	4 "	Rectum
20	Mrs. J.	62	4 " 10 months	Cæcum
21	Mrs. J.	52	5 "	Rectum
22	Mrs. R.	52	5 "	Esophageal disease (?)
23	Margaret C. ...	(?)	5 " 6 "	"Relapse"
24	Miss R.	48	5 " 2 "	Lung
25	Mary W.	67	7 "	Lung
26	Mrs. W.	40	7 " 2 "	Stomach and pleura
27	Mrs. F.	(?)	7 " 4 "	Liver and rectum
28	Mrs. E.	46	10 " 6 "	Ovary
29	Mrs. H.	58	10 " 6 "	Liver
30	Mrs. Anne J. ...	38	14 "	Stomach—probably cancer.

Series 5.—Imperfect cases in which it was found impossible to remove the whole disease at the time of operation.

Name.	Age.	Duration of Life.	Name.	Age.	Duration of Life.
1 Esther G.	52	6 months	5 Sarah R. ...	51	1 year, 6 months
2 Ann H. ...	36	7 "	6 Fanny P. ...	49	4 months
3 M. A. C. ...	51	7 "	7 Ann D. ...	54	2 years
4 Martha L.	42	1 year, 1 month			

Series 6.—Cases done under 1 year and about which nothing can as yet be said.

Sixteen cases have been operated upon during the past year successfully.

Series 7.—Cases which recovered from operation but cannot be traced.

These numbered 15, of whom 1 was alive after 2 years 10 months, another after 2 years 6 months, and a third after 8 months.

Series 8.—Cases operated upon during 1896, 1897, 1898, 1899 and three months of 1900.

1896.	1897.	1898.	1899.
1. Miss S. ... March	21. J. K. ... January	42. Miss C. ... December	
2. Mrs. E. ... January	22. A. V. ... February	43. Mrs. D. ... April	
3. Mrs. S. ... November	23. J. E. ... November	44. Mrs. D. ... March	
4. Mrs. H. ... October	24. A. H. ... September	45. Mrs. E. ... October	
5. Mrs. R. ... November	25. E. T. ... November	46. Mrs. C. ... March	
6. E. G. ... August	26. M. W. ... January	47. Mrs. H. ... January	
7. B. W. ... April	27. M. H. ... May	48. Mrs. F. ... October	
8. J. McG. ... February	28. A. J. ... March	49. Mrs. B. ... January	
9. M. H. ... October	29. A. W. ... June	50. Mrs. H. ... November	
10. C. Ob. ... August	30. A. H. ... November	51. R. K. ... August	
11. M. M. ... October		52. M. S. ... April	
12. A. V. ... December	31. Mrs. T. ... June	53. M. B. ... April	
13. J. C. ... March	32. Miss W. ... November	54. A. H. ... June	
14. J. K. ... December	33. M. N. ... November	55. M. D. ... July	
	34. Miss T. ... June	56. E. C. ... June	
	35. Miss R. ... June	57. A. McN. ... April	
	36. Miss S. ... March		
	37. A. McC. ... April		
	38. M. B. ... September		
	39. C. M. ... October		
	40. J. K. ... December		
	41. H. W. ... March		

Among these 60 cases there was no fatal one.

The above table of statistics speaks for itself. I am sorry that during the removal from our old Liverpool Infirmary to the present one the histories of many cases were lost. But what I have laid before you have been collected with as much care and accuracy as I could give to the matter. Mistakes there may be, but I am certain that they cannot be sufficiently numerous to invalidate in any way any general conclusions that may be drawn from the table. The cases number 213, of which 175 are available for statistical comparison. In Series I it is noteworthy that of the 67 persons who died of local recurrence 17 lived for periods between 3 and 13 years. Series II shows 38 persons alive and well between 3 and 21 years after operation. To Series III it may be objected that I cannot prove that some of the patients did not die from internal recurrence. It is a fair argument, and all I can reply is that I have taken all the pains I could to make sure of the causes of death, and have put any doubtful ones into Series IV. But, after all, if a breast cancer seems a fit subject for removal we do not speculate on internal recurrence, for our business as surgeons is to find out whether we can eradicate the disease locally, so that we may be able to give patients who apply early for relief a good prospect of future immunity. Now, the first four series comprise 175 patients and of these 108 have remained free from local recurrence. Of the 108 there have lived 73 over three years as follows:

Cases that lived between 3 and 6 years after operation ...	40
Cases that lived between 7 and 14 years after operation ...	28
Cases that lived between 15 and 21 years after operation ...	5

Take from these 73 cases half-a-dozen in case or errors, and still it follows that for every one of my 67 failures I can pit against it a case when the patient remained free from 3 to 21 years. With regard to tables of statistics in general the plan of taking percentages of small numbers of cases ought to be abolished. The figures in a surgical article nowadays make it look like an extract from an accountant's report on a bankruptcy case. As for the three years' cure system it, too, must be abolished if people are to be instructed fairly. Let no woman be reckoned cured of cancer of the breast till she is dead.

And now, gentlemen, I have only to thank you for the patient courtesy with which you have listened to my poor attempts in a line of surgery to which I have devoted the best efforts of my life during the past quarter of a century.

REFERENCES.

¹ *Lancet*, April 29th, 1899. ² *Tumours of the Breast*, 1880.

THE LUMLEIAN LECTURES ON SOME CIRRHOSSES OF THE LIVER.

Delivered before the Royal College of Physicians

By W. B. CHEADLE, M.D., F.R.C.P.,

Senior Physician and Lecturer on Clinical Medicine, St. Mary's Hospital
Consulting Physician, Hospital for Sick Children, Great Ormond
Street.

[ABSTRACT.]

LECTURE II.

GENERAL CONSIDERATIONS WITH REGARD TO THE FORMS OF CIRRHOSIS.

CARDIAC DEGENERATION: FIBROSIS OF OTHER ORGANS.

THERE are certain points connected with the pathology of these different forms of cirrhosis which deserve a passing consideration.

Degeneration of Heart Muscle.—Degeneration of cardiac muscle and cardiac failure are of course acknowledged results of chronic alcoholism, and their importance in that relation has been emphasised by Dr. Graham Steele,¹ but the frequency of such degeneration and failure in connection with cirrhosis, its pathological significance, and its bearing upon prognosis have, I think, been insufficiently appreciated.

Of the 53 fatal cases mentioned, the condition of the heart was especially noted in 36.

In 6 of these fatty degeneration, in some instances extreme, was verified by microscopic examination; active myocarditis was observed in 1, and in 8 others the muscle was found pale, soft, and presumably fatty. In 9 more there was atheroma of one or other of the cardiac valves.

There was, therefore, marked degeneration of heart muscle in 8, and in

less degree in 9 more, or 17 in all, out of 36, without counting the cases (9 more) in which atheroma of the valves only was recorded.

Of the 17 cases of fatty degeneration, the liver was enlarged in 11 instances, in 6 it was small and contracted. So that the tendency to degeneration is not limited either to the large or the small liver. In 13 the cirrhosis was distinctly traced to alcohol.

The frequent occurrence of serious degeneration of the cardiac muscle accounts for the fact that a certain proportion of these cases end suddenly in death by syncope, many more by rapid, if not sudden, heart failure. In the 53 fatal cases mentioned the patient died suddenly from syncope (not the result of hæmorrhage) in six instances. As I shall endeavour to show later, it is probable that the enfeebled cardiac condition may sometimes be the factor which determines the advent of ascites in alcoholic cirrhosis, and also be concerned in the development of the fibrotic change and influence its character.

Concurrent Interstitial Fibrosis of Other Organs.—In cases of cirrhosis—or at all events of alcoholic cirrhosis—the interstitial fibrosis is not confined to the liver, although its most extreme development may be found there, and very largely rule the pathological situation.

The kidneys suffer next to the liver in order. They were found to be distinctly affected in 25 of the 53 cases. In 5 instances the kidneys were granular, “moderately cirrhotic” or “fibrosed” in 13, in 5 they are described as “fibrosed” simply, in 3 there was parenchymatous nephritis. Again, in 12 of these cases in which the liver was markedly atrophic and small there was fibrotic change in the kidney in every instance, but in 5 only was the interstitial change advanced. In the remaining 7 it was extremely slight.

The spleen was enlarged in 23 instances, 11 of these being in atrophic, 12 in hypertrophic cases, and its condition was noted as fibrosed in 6.

The pancreas has only been systematically examined in a few of the recent cases, namely, in 14. It is described as cirrhotic in 3 instances, extremely hard or firmer than natural in 10; in the remaining case, which was one in which neither alcohol nor syphilis could be traced, the organ was large and soft. In 7 of the 14 cases alcoholism was established; in 2 syphilis; in the remaining 5 the cause was not stated.

This association of cirrhosis of the pancreas with cirrhosis of the liver is of interest in connection with Hanot's² observation that hypertrophic cirrhosis was an occasional complication of diabetes; and also the association of fibrosis and atrophy of the pancreas with diabetes independently. In the cases to which reference is here made the fibrosis of the pancreas was not limited to those cirrhoses of the liver in which that organ was enlarged, although these were in the majority, namely, 9 as compared with 5 of the atrophic form. In no case did diabetes occur, nor did sugar appear in the urine; but it is obvious that such a condition of pancreatic degeneration must seriously affect nutrition and be a factor in the wasting which attends hepatic cirrhosis.

Liability to Erysipelas.—These records also show that persons suffering from cirrhosis, vulnerable in so many respects, suffer from a special liability to erysipelas beyond that common to all chronic exhaustive diseases. Dr. Dreschfeld has remarked that erysipelas is not uncommon in Hanot's disease—but there is no exclusive association with biliary cirrhosis, or indeed with hypertrophic cirrhosis as compared with the atrophic kind.

In the 53 fatal cases of the last ten years death occurred from facial erysipelas in 3.

In two other instances of a previous period the fatal issue was due to the same cause—or 5 cases in all.

Of these the liver was enlarged in 3—two of which were syphilitic, one alcoholic. In the remaining 2 the liver was contracted and hobnailed, and the affection of alcoholic origin.

Liability to Tuberculosis.—The close association of tuberculosis with cirrhosis of the liver is well known, and marks again the vulnerability of these subjects of degenerated tissues. It is due chiefly, I imagine, to the proved influence of alcohol in this respect.³

RIGID CLASSIFICATION OF CASES IMPOSSIBLE.

The evidence which I have adduced in the first lecture supports, I think, the opinion which I ventured to express at the outset, namely, that the lines of demarcation between some of the different forms have been too rigidly drawn. They hold good with regard to typical examples only. Take, for instance, the mere question of size and weight. No strict line can be drawn between hypertrophic and atrophic forms, at any rate so far as alcoholic cirrhosis is concerned. The conclusion arrived at by Dr. Foxwell that all sizes, from 100 ozs. to 30 ozs., are met with, is more than borne out by my series of cases, in which all sizes from 152 ozs. to 18 ozs. are found. And thus with regard to other symptoms, there are many cases in which the symptoms agree in some respects with those attributed to