WERTHEIM'S OPERATION IN RETROSPECT *

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AT the annual meeting of the British Medical Association, held at Leicester in 1905, Professor Wertheim, of Vienna, read a paper before the gynæcological section in which he introduced and described the operation which now goes by his name (Wertheim 1905).

He claimed as the result of it 40 five-year cures out of every 100 patients operated on, as against the 10 or less obtained by simple abdominal or vaginal total hysterectomy.

So great an advance naturally caused a sensation, and the more so because he asserted that his operation was applicable to just on 50% of all cases of carcinoma of the cervix instead of the 10–15% which were the limit of the lesser procedures.

Abdominal hysterectomy for the disease had been advocated by W. A. Freund as far back as 1878, but the real advance began in 1895, when Ries of Chicago proved by experiments on dogs and on the cadaver that a radical operation on the lines of Halsted's operation for cancer of the breast was perfectly feasible, and in 1896 Clark of Johns Hopkins University first performed it on a living woman. Clark's example was followed by Howard Kelly, while, quite independently in far away Australia, Thring of Sydney was trying the same thing.

Wertheim did not begin his long series till 1898, but to his courage and persistence belongs the credit of establishing the operation as one of the recognised procedures of surgery.

His paper, however, was received with considerable scepticism by the older British gynæcologists of that time, most of whom had not undergone that training in general surgery which renders the mind receptive to and capable of exploiting new ideas; but what chiefly stuck in their throats was the high mortality attached to the operation—30 deaths in the first 100 operations that Wertheim performed, though by the time he read his paper he had reduced the rate to 20%.

It was no use arguing that the only alternative at that time was to operate on 10 out of every 100 patients by one of the lesser operations, cure one or two, and let the rest perish; the minds of the pundits were made up —it was a horrible mortality, and that was that. And this criticism has ever since been a weapon in the hands of the opponents of surgery, though today, with surgeons skilled in it, the operative mortality is very much lower; indeed, my colleagues, Frank Cook and Charles Read have shown that in selected cases it is not more than 3%.

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I was not present at this historic meeting, though I had gone up to Leicester to show certain exhibits at the pathological section; but of course I read Wertheim's paper, and I realised at once the wide vista which it opened. So revolutionary a procedure naturally appealed to the younger and (surgically) better-trained minds; and Cuthbert Lockyer, who was then attached to a small hospital at Plaistow, got Wertheim to go down there and demonstrate his operation, after which Lockyer himself began to perform it, being the first British surgeon to do so. Another of the younger men, Wallace, of Liverpool, by whose premature death surgery lost a great executant, followed suit soon after.

As regards myself, I decided to do the operation so soon as a suitable case came my way; and, since I had only the illustrations in Wertheim's article to guide me, I performed it several times in the post-mortem room at Middlesex Hospital.

In 1906 I opened the abdomen of a patient with advanced disease, only to find the growth irremovable; but early in 1907 I did so again, and that time completed the operation, though the patient died a few days afterwards. Later in the year I tried again, and achieved a successful result.

Comyns Berkeley, with whom at the time I was working in close collaboration, assisted me with all these three operations. At the end of 1907 he performed one himself successfully, with me as his assistant, and for the next seven years we always assisted one another.

In 1908 I joined the honorary staff of Middlesex Hospital, and this promotion greatly extended my field of action, for the hospital had had, for a hundred years, wards devoted to cancer, and in consequence it received a much greater number of cancer patients than any other institution in Great Britain.

RESULTS

By the middle of 1908 Berkeley and I were able to publish an interim report (Berkeley and Bonney 1908) on 18 cases of Wertheim's operation, with 3 deaths. The shortest operation took fifty-five minutes, and the longest two hours. By the end of the year we had performed it 30 times.

In 1913 we reported (Berkeley and Bonney 1913) 71 operations on a three-year cure basis, with an operative mortality of 22% and an operability-rate of 63%. The estimation of our operability-rate was then very easy, for Berkeley and I were the only surgeons performing the operation at Middlesex Hospital and Chelsea Hospital for Women, and all the cases of cancer of the cervix presenting themselves at the outpatient departments came to us.

Later on, as other surgeons began to do the operation and radiological therapy came into the field, it became impossible to disentangle those cases whose treatment was primarily determined by us from those in which it was determined by others, and in all our subsequent publications we assumed that our operability-rate remained at 63% on the good ground that our selection of cases had never varied, being founded simply on whether there appeared to be any chance at all of removing the growth.

Lest anyone think this an exaggerated estimate, I cite my figures for cases of carcinoma of the cervix seen by me in private practice. Between 1910 and 1929 I saw 140 such patients, and of these I operated on 113, an operability-rate of 80%.

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The technique I evolved departed somewhat from that of Wertheim. To begin with, removal of the regional glands was a routine, whereas Wertheim did not touch them unless they were obviously involved. Further, instead of removing only sufficient vagina to form a cap over the cervix, I began by removing half, and later on the whole, of it, and as early as 1908 I had replaced the clumsy vaginal clamps that Wertheim used by the instrument which goes by my name.

Our criterion of operability—a possibility of removal and nothing else—necessarily produced a good many cases in which on opening the abdomen extirpation of the growth was found to be impossible because of gross involvement of the bladder or both ureters. One ureter

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can be successfully resected and reimplanted in the bladder; but if bilateral reimplantation is decided on—and it requires a great deal of consideration—I think today that the colon should be chosen, though I have never done it myself.

In 1916, Berkeley and I published a joint series of 100 cases (Berkeley and Bonney 1916). Our operative mortality was 20%, and 39 of the patients were well five years after their operations. In this paper we classified our cases according to whether the regional glands removed at the operation were or were not carcinomatous, a method I have regularly followed since. Not only is it exact, being found on microscopical examination, but also at that time it served the useful purpose of proving that a proportion of the cases whose glands are involved can be saved by surgery. Some authorities had denied this.

The percentage of gland-involved to gland-free cases in a series is a general indication of the degree of the advancement of growth which the surgeon has tackled, but there is no fixed relation between the involvement of glands and the extent of the cervical growth. Generally speaking, of course, the more advanced the growth the greater the chance of involvement; but carcinomatosis of the glands can co-exist with a very small cervical growth, and conversely the glands are not infrequently found free in cases in which the growth in the cervix is very extensive, and post mortem in persons who have died of the disease.

In 1921 I recorded (Bonney 1921) the first 100 operations performed individually by me, with 20 deaths, 40 survivals for five years, and an absolute operative five-year achievement of 25%. And thereafter I published my personal results only, though Berkeley continued to do the operation as before, and by the time he retired he had performed it between 300 and 400 times. Unfortunately he neglected to keep in close touch with his patients, and lost contact with so many of them that his statistics were of much less value than they ought to have been. As far as they went, however, they showed that his results were substantially the same as mine.

By 1925 (Bonney 1925) I had done the operation 192 times, with results the same as those of four years previously, except that the general mortality had dropped to 16%; and by 1926 (Bonney 1926) I had done 214 operations, with the same outcome but a general mortality-rate of 15·8%.

In 1929, at the Royal Society of Medicine, I reported (Bonney 1929) 265 cases on the five-year basis with 40% cures, and 161 cases on the ten-year basis with 36% cures.

My experience had taught me, what I was the first to enunciate, that 10% of all recurrences occur between the fifth and tenth years (the latest either Berkeley or I have known being the eighth year), and that ten years' survival is 100% cure.

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In that paper I exhaustively examined the comparability of radiological and surgical statistics, and showed that of the various methods of computation not one was impartial, because they contrasted dissimilar groups of cases. Some favoured irradiation and some surgery, but they were all alike in this.

I pointed out that the only accurate comparison was between the results of the pure radiologist and those of the surgeon who operates on a proportion of the cases presented to him and irradiates the remainder. The 100 patients presenting themselves at each man's clinic are substantially similar, and the number of five-year and ten-year cures that each achieves out of *his* 100 cases is the measure of the efficacy of the system of treatment he follows, and as such can be directly compared with the other man's results. And here let me beg radiologists to abandon the division of their cases into stages, for every surgeon who has experience of operating for the disease knows how often the preoperative opinion is found to be quite wrong when the abdomen is opened. These so-called stages introduce into the statistics an element of inaccuracy, and, what is perhaps as bad, make them tiresome and boring. All that matters is the number of patients seen, the number treated, and the number of five-year and ten-year survivals.

In 1930 I delivered a Hunterian lecture at the Royal College of Surgeons, wherein I reported 284 cases on the five-year basis, with an operative mortality of 16·5% and a cure-rate around 40%; and 181 cases on the ten-year basis with a cure-rate around 36%; I also described very fully the technique, dangers, and complications of the operation (Bonney 1930).

There are two methods of assessing the number of cures. In the first method all patients lost sight of or dying of other disease are reckoned as having died of carcinoma, whereas in the second method these cases are excluded from the calculation. I hold that the second method is the right one, but throughout all my communications on the subject I have made a practice of giving both estimates. The difference in the figures resulting from the two methods of computation is considerable when applied to ten-year results; for, as the average age of the patients is over fifty, a proportion of them, in ten years, will surely have died of some disease other than carcinoma, and the number lost sight of will be considerably greater than in a five-year series. The first method of computation, which is Continental in origin, was designed to prevent wilful falsification of results. Those who prefer it, with its obvious errors, must have a very poor opinion of the honesty of their fellow workers.

In 1931 I reported (Bonney 1931) 310 cases, and in 1932 (Bonney 1932) 339 cases with results almost identical with those I have given, and in 1935 the American Gynecological Society honoured me by an invitation to give an address on Wertheim's operation. In it I presented (Bonney 1935) 384 cases on the five-year basis with a cure-rate of 39% or 41%, according to the method of calculation, and 283 cases on the ten-year basis with a cure-rate of 29% or 33%.

In that paper I emphasised the fact that a large number of the so-called stage III cases (styled "inoperable") could be successfully operated on, and I was asked to substantiate my contention by actual demonstration. I was very willing, and so one afternoon at the Women's Hospital, New York, I operated on two stage-III cases chosen by my old friend Gray Ward and his hospital colleagues, and I completed both operations. One of the patients, who had malignant regional glands, died of recurrence some years later, but the other, whose glands were not involved, is alive and well today.

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Finally, in 1941 I published (Bonney 1941) the results of 500 operations on the five-year basis, and 415 operations on the ten-year basis. The paper is so comparatively recent that I will only mention its main conclusions. For the 500 cases the five-year cure-rate was 40% or 43%, according to the method of calculation, and the operative death-rate 14%. For the 415 cases on the ten-year basis the cure-rate was 31% or 36%, according to the manner of computation, and the operative mortality slightly less than 14%.

Of the 500 operations, 300 of the patients were gland-free and 200 gland-involved, and the difference in the results obtained from the two groups was very marked. Thus, of the gland-free group on the five-year basis 53% or 58% were cured; and on the ten-year basis 42% or 49%; while of the gland-involved group only 22% or 23% were cured on the five-year basis and only

16% or 18% on the ten-year basis. There was a great difference too in the respective operative mortalities, that of the gland-free group being 10% and that of the gland-involved group 20%. It may be said, therefore, that gland-involvement doubles the risk of operative death, and more than halves the average chance of cure; but, on the other hand, it is to be remembered that all these gland-involved patients, left unoperated on, would have died of carcinoma, for the evidence shows that no method of irradiation at present known cures carcinomatous regional glands.

The absolute operative achievement is the number cured out of every 100 unselected patients seen. Reckoning my operability-rate as 63%, my achievement was 25% or 26% on the five-year basis, and 20% or 21% on the ten-year basis; or, to put it another way, surgery can effect 1 five-year cure out of every 4 unselected patients, and 1 ten-year cure (an absolute cure) out of every 5 unselected patients. This does not exhaust the cures that a surgeon can effect; for, besides the patients he operates on, he has a residuum—larger or smaller as his operability-rate is low or high—from which patients can be salvaged by irradiation.

This paper was my swan-song, so far as the statistics of Wertheim's operation are concerned; for, though I have performed it many times since I reached the 500 mark in 1936, I ceased to keep in routine touch with the subsequent patients, feeling that the addition of 100 cases to the series would shed no new light, and that, after labouring for twenty-nine years to keep in touch with the women I had operated on, I deserved a rest.

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Looking back on the figures I have published at various times, I think one must be struck with their uniformity, for my five-year cure-rate for all cases has hovered monotonously around 40%, and my ten-year cure-rate around 35%. It would be strange if it was otherwise, for over all these years my standard of case selection has never varied and my technique has not changed in material details. The one exception to this uniformity is my operative mortality, which was 20% for my first 100 cases, 14% for the next 200 cases, and 11% for the last 200 cases, and it might be thought that this reduction would be reflected in the cure-rates. But it must be remembered that most of the operative deaths followed operations on patients with very advanced disease, only a few of whom would have figured as cures had they survived.

The figures I have given relate to patients operated on over a span of twenty-nine years, during the larger part of which time none of the accessories, adjuncts, and aids which surgeons now enjoy existed, and the last of them was operated on thirteen years ago. Further, they are the results of pure surgery, for I have not used irradiation either before or after the operations except in a few cases where, the extirpation having been virtually completed, I discovered a mass or nodule so firmly attached to the side wall of the pelvis that its removal was impossible. These patients were treated with X rays; but, despite the fact that the position of the target to be aimed at was accurately known, every one of them died.

There is much evidence of recent years that pre-operative irradiation is helpful, but even at the time when my series was finishing it was not convincing, and to have used it then would have vitiated results obtained by pure surgery. Those that I have obtained are very much the same as those of Wertheim himself, and the other surgeons—including from this country Comyns Berkeley and Fletcher Shaw—who have practised the operation on a large scale and published their results. Those opposed to Wertheim were not backward in hinting that his figures were untrustworthy, but time has proved them entirely honest.

I also have encountered some opposition for keeping the flag of surgery flying, and it has been assumed that I am an opponent of irradiation, which is untrue. What I have done in the past, and what I again do now, is to insist that there are certain cases of cancer of the cervix best treated by surgery, as there are others best treated by irradiation. The views I expressed in my Hunterian lecture in 1930 remain unchanged:

> "The truth of the matter probably is that in certain cases surgery, and in others radium, would give the best chance of cure, each case being a law unto itself in this respect, but our investigatory methods are too coarse and imperfect to enable us to discriminate accurately. This is the position at present, but how long it will continue remains to be seen, for the control of the forces of radioactivity is yet in its infancy, and the future may see the power capable of disrupting the very atom into its component parts, harnessed and directed to a degree only dreamed of today. We have, however, to deal with things as they are, not as they may be, and hence I deprecate as altogether premature the appeals which have been made to the younger gynæcological surgeons not to embark on the operative treatment of cancer of the cervix, but instead to take up radium therapy."

Since that was written 19 years ago radioactivity has been the means of greatly increasing our power of destroying life; but alas! not of saving it.

Among the group of cases which should be treated by surgery are those where the growth is refractory to irradiation, and it is of the utmost importance that some method be found by which refractoriness or vulnerability can be distinguished before any treatment is decided on. We must all welcome, therefore, the contribution which Glucksmann and Stanley Way (1948) have made towards an achievement, which, when fully realised, will constitute a major advance in our battle against carcinoma of the cervix.

The specific cure for cancer has not yet arrived—some substance or agent which, put into the body, will single out for death the cancer cell, and/or the factor which energises it, and nothing else. When that day comes, tribute will be paid to Blair Bell, whose work, though it evoked much hostility, largely by reason of his own magnificent egoism, was, I have no doubt of it, along the right path.

Till then, surgery and radiotherapy alike are make-shifts—a humbling thought and one which should discourage the acrimony which has crept into their rival claims.

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". . . The so-called social sciences encourage students to talk endlessly about alleged social problems. They do not seem to equip students with a single social skill that is usable in ordinary human situations. Sociology is highly developed, but mainly as an exercise in the acquisition of scholarship. Students are taught to write books about each other's books. Of the psychology of normal adaptation, little is said, and, of sociology in the living instance, sociology of the intimate, nothing at all. Indeed, in respect of those social personal studies that are becoming more important year by year, no continuous and direct contact with the social facts is contrived for the student. He learns from books, spending endless hours in libraries; he reconsiders ancient formulæ, uncontrolled by the steady development of experimental skill; the equivalent of the clinic, or indeed of the laboratory, is still to seek."—ELTON MAYO, *Social Problems of an Industrial Civilization*. London, 1949.