

DISCUSSION

DR. I. RIDGEWAY TRIMBLE (Baltimore): Discussing Paper No. 2:

I must disagree with one of my associates and partners. I wish to comment on the paper of my good friend and his associates, Dr. John Madden.

It would be most helpful if those of us who are struggling to cure cancer of the breast, surgeons and pathologists alike, would study and restudy the pathways by which the cancer, originating in the breast, reaches out into other tissues, especially into its customary first stop, the axillary lymph nodes. A more thorough search of surgical specimens, especially by surgical pathologists, should include the fascia and muscles, as well as the customary search for the tumor itself in the lymph nodes. We know without doubt that the fascia over the pectoralis major muscle is involved in a high percentage of cases. How often is the underlying muscle involved? How do the cancer cells from the breast tissue reach the axillary lymph nodes without traversing the lymphatic channels in the connective tissue septa between the pectoral muscle bundles?

Volkman almost a century ago removed only the breast, the fascia over the pectoralis muscle, and the axillary contents. Later on, discouraged by this operation, he began removing the pectoral muscles also.

Halsted in 1894 wrote: "If we may judge from incomplete description of the operations, Volkman is the only one—Billroth perhaps excepted—of the surgeons whose work we have considered who occasionally removed the pectoral muscle. I am at a loss to know how to explain this, because I operate not infrequently on cases in which the disease has involved at least the fat and areolar tissue between the muscles, if not one or both of the pectoral muscles."

Volkman's operation, according to Halsted, "is manifestly an imperfect one. It admits the frequent division of tissues which are cancerous, and it does not give the disease a sufficiently wide berth. Why should we shave the undersurface of the cancer so narrowly, if the pectoralis major muscle or part of it can be removed without danger and without causing subsequent disability, and if there are positive indications for its removal?"

He further states: "The pectoralis major muscle entire, or all except the clavicular portion, should be excised in every case of cancer of the breast, because the operator is enabled thereby to remove in one piece all of the suspected tissues.

"The suspected tissues," he said, "should be removed in one piece (1) lest the wound become infected, a division of tissues invaded by the disease, and (2) by the division of lymphatic vessels containing cancer cells, because shreds or pieces of cancerous tissue might readily be overlooked in a piecemeal extirpation."

Later on, of course, he removed the pectoralis minor muscle, and I must say that I am just unable to do a complete dissection of the axilla, especially high up in the apex, or clean the axillary vein up

there, without dividing at least the pectoralis minor insertion.

Finally, I believe I can speak for the great majority of us who were raised in the shadow of the Halsted tradition by saying that we still believe in, and still employ, his typical radical operation as the best means in our state of imperfect knowledge at the present time for the cure of cancer of the breast.

DR. C. MELVIN BERNHARD (Louisville): Discussing Paper No. 2: I have been using modified radical mastectomy as the preferred operation for cancer of the breast since 1965. I am delighted to hear Dr. Madden's results and of his favorable comparison in terms of survival with the modified version. I am particularly impressed with the over-all comfort of patients who have had the modified radical procedure, over those who have had the standard radical operation.

My patients have had less discomfort and greater use of the arms postoperatively. There has also been much less swelling of the arm.

Let me briefly tell of one patient. Fifteen years ago she had a standard radical mastectomy of her left breast. A year ago she had a modified radical operation of her right breast for a second carcinoma. Every time I see her, she tells me of how much less discomfort she had after the second operation, and she continues to demonstrate the lack of swelling and greater use of the right arm over the left.

DR. ERNEST A. GOULD (Washington, D. C.): Discussing Paper No. 2: I think because my conscience was bothering me with the Patey procedure, we began doing some vital stains, using either Prussian blue or methylene blue, injected at the site of the tumor after it had been removed and a frozen section had confirmed the diagnosis of carcinoma. We found that, despite the classical routes of lymphatic drainage of the breast, lymphatic vessels go through the pectoralis major and also go around, medial or lateral to the insertion of the pectoralis minor.

This is of great concern, particularly with the larger lesions, the Stage II lesions. One should be very selective about the use of the modified procedure. For the present it should be used only in lobular carcinomas, or early Stage I. This should be a preliminary observation to a continuing study on lymphatic drainage of the breast.

DR. BEVERLY C. SMITH (New York): Discussing three papers: We have had three interesting, provocative reports on the treatment of carcinoma of the breast. Dr. Madden has described his modified, radical procedure which has gained more acceptance in the recent past. The follow-up results on this procedure are not of sufficient number and duration to adequately compare with the Halsted procedure which has been the method of choice for carcinoma of the breast since its introduction by Dr. Halsted. There has recently been considerable emphasis on cosmetic appearance and re-

habilitation following radical and modified radical breast surgery for carcinoma. Those of us at Presbyterian Hospital in New York have always accepted and performed the standard Halsted procedure for this pathology.

Hugh Auchincloss, Jr's recent report has again focused attention on the inadequacy of radical surgery when the highest lymph nodes in the axilla are involved. When the pectoral muscles are left, of course, the connective tissue and lymphatics therein between the muscles remain and may be a residual source of carcinoma. The percentage of such involvement is, at present, unknown. "The manner in which a woman is treated for carcinoma of the breast, at present, largely depends upon whom she consults!" The long and numerous follow-up results of the Halsted procedure in advanced nodal involvement has provided an area of investigation as to its efficacy. It would seem this modified procedure is still on trial and it is too soon to substantiate its reliability. The crux of therapy in this disease is early diagnosis and complete excision or destruction of all cancer material. It is entirely possible the disease may be eradicated if it is sufficiently early and localized, without removing the pectoral muscles, but we should not be too hasty in discarding proven principles.

As to Dr. Hartman's interesting report, I recall many discussions with Dr. Arthur Purdy Stout of the Department of Pathology at Presbyterian Hospital in New York as to whether or not cancer could ever be called cured, I vividly recall a case in which a biopsy revealed a scirrhous carcinoma in the lower outer quadrant of the breast which the patient stated had been present as an ulcer for 12 years. She refused operation. Fifteen years after biopsy she permitted a radical procedure and she was well for 12 years following this operation, but subsequently 12 years after radical mastectomy she died of metastatic scirrhous carcinoma. This is one incident of a known life history of a carcinoma for more than 40 years which ended in a carcinoma fatality.

I was particularly interested in Dr. Finney's report because of the following experience.

In 1935 I did a radical left mastectomy on a 40-year-old woman who had no axillary gland involvement. Pathologic diagnosis was invasive ductal carcinoma. Ten years later in 1945, I did a similar procedure on the right breast. Again there was no axillary involvement. Dr. Stout and his department studied this case in great detail and concluded these were different carcinomas. The pathologic diagnosis was the same.

In 1965 I operated on the daughter of the above-mentioned patient at the age of 44. A left radical mastectomy was performed and there was replacement of a single lower axillary node by metastatic carcinoma. In 1966 I did a radical mastectomy on the daughter's second breast for a small carcinoma in the upper outer quadrant in which there was one lower axillary node involved. This is the only example I know of a bilateral carcinoma which occurred in a living mother and daughter. The mother is still alive 37 years after the first procedure and the daughter died of a

rapidly spreading carcinomatosis in January 1969, 4 years after the first and 3 years after the second procedure. The carcinoma of the right and left breast of the daughter were invasive ductal carcinoma and again diagnosed as separate tumors.

Possibly geneticists or immunologists may in the future throw light upon the different behavior of these two carcinomas in mother and daughter. I have not been able to find a similar example of bilateral carcinoma in mother and daughter in the literature. One should constantly keep in mind that carcinoma of the breast is a deadly disease and should be slow to modify the therapy that has stood the test of time, because of emotional and cosmetic problems.

DR. GEORGE H. BUNCH, JR. (Columbia): Discussing Paper No. 2: I believe that Dr. Madden's procedure has merit which I personally prefer to use except for very advanced carcinoma of the breast that involves the undersurface and perhaps the muscles; but I wish to make one technical point, Dr. Madden.

When I began to use this procedure several years ago, I was not completely satisfied that there was adequate exposure for the apex of the axilla. I was not sure that I was being intellectually honest with my patient. I do not recall how the pectoral muscles can be relaxed. I know that some drape the involved arm sterilely, so that it can be rotated medially, to relax these pectoral muscles, but most often we have the arm strapped down to an arm board at the side of the table. After two or three experiences I found this method to be unsatisfactory. I asked the anesthetist or the anesthesiologist to speed up the Anectine drip, or other muscle relaxant being used, after which the pectoral muscle relaxed dramatically, and one nurse with one retractor could expose the apex of the axilla very adequately. I would mention this as a possible addition to exposure under the pectoral muscles when they are not divided.

DR. SAM A. WILKINS, JR. (Atlanta): Discussing Paper 2 & 4: We have been interested in exploring the possibilities of limiting a radical mastectomy for a number of years. We do not have our results as yet, but we will certainly be interested in comparing them, as they are studied, with Dr. Madden's.

I should like to comment on the last report, No. 4, and show a slide in regard to the bilateral incidence of carcinoma.

[Slide] We have eliminated from this study patients with advanced mammary cancer and inflammatory carcinoma. The study was carried out in congress with Dr. Robert Egan's mammography program. You will note that the local excision of breast lesions has increased, and the increase of cancers has developed more slowly. This, I believe, would be expected.

In the first 5-year period there was a fairly low incidence of bilaterality, the same in the second, and then in the third period, from '61 to '66, when the influence of mammography had us doing more local excisions, we did find a definite

increase in bilaterality. For the next 5 years, so far as we can judge at this point, the incidence of bilaterality is being maintained, and may be even slightly higher.

We attribute most of this increase in bilaterality to the mammographic aid which we have, and only partly, if at all, to that of increased diagnostic acumen.

DR. LOREN J. HUMPHREY (Kansas City): Discussing all three papers:

The best operation for cancer of the breast is that procedure which is most likely to encompass all of the cancer. For example, Dr. Madden's 17 per cent 10 year survival in patients with positive lymph nodes, compared to the literature, creates some worry for all of us.

Removal of the axillary lymph nodes does not remove memory for cancer, and it does not leave the patients immunologically incompetent. We showed earlier in the year, that three of 15 patients who had radical mastectomies for carcinoma of the breast developed, after removal of the breast and axillary lymph nodes, antibody to breast cancer. Interestingly enough, since then in complement fixation studies, the sera from these three patients reacted with breast cancer tissue, and not with normal breast tissue.

Hence, removal of the axillary lymph nodes is not immunosuppressive, but may remove additional cancer cells in a certain per cent of patients.

On the other hand, I would like to close by pointing out that Co⁶⁰ is immunosuppressive, with a decreased total peripheral lymphocyte count years after radiation therapy and, I personally feel, should never be used with a curative procedure, but only for palliation.

DR. CLAUDE E. WELCH (Boston): Discussing Paper No. 4: I shall confine my remarks to the report of Dr. Finney and associates.

Our whole problem in cancer of the breast, it seems to me, is based upon one fundamental problem. On the one hand we have the desire not to mutilate a person. This is obviously shared by the patient—even to a greater extent. On the other hand, we as surgeons have an almost missionary zeal to destroy cancer.

To try to put these together is extremely difficult. Perhaps it becomes a little bit easier with the bilateral cancer of the breast. After a previous mastectomy, the patient perhaps is not quite so worried about mutilation of the second breast. On the other hand, the chances of a second carcinoma appearing, by your figures, are somewhere around 7 to 10 per cent; and furthermore, the chances of cure become rather low, with only a 50 per cent 5-year survival when cancer does involve the second breast.

The question that I want to pose to the authors is this. You have identified very nicely a group of patients that is more likely to develop this second cancer; that is, the ones who have had a family history, those who have had difficulty with gestation, et cetera. Has your philosophy changed to the point where you think something different

should be done about these patients? Are you satisfied with the present methods of follow-up? Is mammography going to be the answer? Are the new immunologic technics going to be the answer? Or should you recommend a second mastectomy on the other side before the patient develops trouble?

This is the practical point that I want to put before you now, because I believe this is what we need to know at the present.

DR. JOHN L. MADDEN (Closing Discussion of Paper 2): I would like to reply first to Dr. Trimble.

Of course, the relatively recent study of Gray has definitely shown that the theory of permeation of the subfascial lymphatics as was promulgated by W. Sampson Handley many years ago has not been substantiated.

I do not know whether or not you noticed on the last slide of the illustrated operative technic that the proximal portions of both rectus sheaths were removed as practiced and recommended by Handley (W. S.). In the past 5 years we have stopped doing this and have not noticed any differences in the results. Therefore, the importance of the subfascial lymphatics as far as the permeation of the cancer cells is concerned is certainly doubtful.

Relative to the problem you posed about the nodes in the apex of the axilla, I think the study of Auchincloss is most interesting. He showed that of the 38 patients who had the apical nodes involved, 34 had died and only four were living after 8 years. Accordingly, the question arises if the apical nodes are already invaded, what is the benefit of extensive dissection of the axilla.

The most interesting thing to me was to study the surgical papers of Halsted and to review his 50 cases of radical mastectomies which were abstracted by Joseph Colt Bloodgood and to note that 24 of the 50 patients were alive and well; a percentage rate of 48. The shortest follow-up was 46 days; the longest was 3½ years; and the average follow-up was about a year and one-half. Only two of the 24 patients were followed more than 3 years. This study has always been used as a standard for comparison but the results are not believed satisfactory when each case, as reported by Dr. Halsted through Dr. Bloodgood, is analyzed in detail.

As far as the modified radical operation is concerned, we can go back to Banks in 1882 who practiced this procedure wherein he did not remove the muscles. But John B. Murphy in 1912 stated that he had abandoned the procedure of Halsted and did not remove the muscles. The question, of course, arises: If this is the wrong thing to do, then there should be an increased incidence of local recurrences. However, Patey has never seen a local recurrence. John B. Murphy never saw one, nor has Handley (W. R.). Finally, we have not seen one recurrence in any of the 94 patients so treated. This leads to the question of Dr. Gould about lymphatic staining. I was interested in this study and believe it should be pursued. It demonstrates very well a pathway for lymphatic spread but is it of practical surgical

importance? The only comment I would make is that we leave the muscles *in situ* and not see their invasion by recurrent disease.

I agree with Dr. Bernhard that edema occurs less but a varying degree of edema occurs in all of the patients. After an adequate axillary dissection, some edema should occur. If it does not occur, one should question the adequacy of the axillary dissection. However, when our own incidence of edema is compared with that following the classical radical mastectomy, it is definitely less.

I am in complete agreement with Dr. Beverly Smith that time will tell what the answer will be.

I want to thank Dr. Bunch for his continuing study in this regard because this is the only way that we can finally determine the true value of this operation. That is to say, if surgeons in various areas do the operation and observe carefully the results obtained. The ideal method would be to have a large, prospective, randomized, controlled study for proper evaluation of this procedure.

In reference to dissection of the axilla, I have not found the muscles a particular problem. The arm is kept completely free and manipulated as necessary during the course of the operation. I have not used Anectine for muscle relaxation but I do think this is an excellent suggestion.

I am pleased to know that Dr. Wilkins is carrying out this operation and I am grateful for what Dr. Ochsner had to say relative to the pectoralis minor muscle. No doubt, leaving this muscle *in situ* may be the factor that has resulted in the decrease in edema.

Dr. Humphrey is rightly concerned about our 16 per cent 10-year survival rate in patients with "positive" axillary nodes. But then, Dr. Harrington's report of more than 4,000 patients with nodes "positive" in each of whom the standard radical procedure was used, his 10-year survival rate was also 16 per cent. In George Finney's report before this Association, the 10-year survival rate was 19 per cent.

I would like to mention that in a recent review by Auchincloss of a breast study done at the Columbia Presbyterian Hospital—in which most of the patients were operated upon by Haagensen—there were 107 patients with "positive" axillary nodes. I might also say that this series was highly selective in every way and excluded patients that I feel sure many of us here would have operated upon as far as the cancer of the breast was concerned. Yet, despite this selectivity, the 8-year survival rate was only 28 per cent.

I am glad that Dr. Humphrey also agrees that cobalt should be used only for palliation since that is just what we have done throughout the whole of this study.

In closing, I really do not know which procedure is the correct operation for cancer of the breast. When the axillary nodes are involved, I am not sure that we can claim superiority for any particular procedure. If the nodes are not involved, then maybe a simpler procedure will be equally as suitable as either the standard or

modified radical mastectomy. As experience accumulates, it may prove that Dr. Crile is correct; namely, do the simple mastectomy but do not remove the axillary nodes unless they are believed to be invaded. Doing this, you would, of course, accept a 10 per cent error in Operative Stage I and a 25–30 per cent error in Clinical Stage I patients. According to his thesis, leaving the nodes would preserve the normal immunologic barrier to the spread of the primary disease as suggested by his experimental studies. However, Dr. Humphrey has stated that he could not confirm such studies. The fact that Dr. Crile's 5-year survival rate with the secondary axillary dissection is equal to or better than that obtained when the nodes were removed prophylactically certainly supports his contention.

I believe, as Dr. Bernard Fisher has stressed, that we need an extensive, prospective, randomized, controlled study to determine truly the best operative procedure for the treatment of cancer of the breast. Then we will have scientific facts rather than clinical conjectures.

DR. CHARLES C. BROWN (Closing Discussion of Paper No. 4): With regard to Dr. Smith's comments on what we might term familial bilateral breast carcinoma, we have had no such instances in our series of 50 cases. I believe there was one case reported in the literature from the Lahey Clinic citing a series of three such cases.

With regard to Dr. Wilkins' comments, it was interesting to see the series from Atlanta, having been on that staff for a while, and would agree that the use of mammography is very important in finding these other lesions.

In response to Dr. Welch's question, I would say that we have not reached the point yet where we can identify this group of patients so precisely that a prophylactic mastectomy would be in order.

Finally, I would like to show a few slides on one particular type of carcinoma in which we found an increase in our series of bilateral carcinoma.

[Slide] This is the tubular carcinoma about which we spoke. Clinically, this has no distinguishing characteristics. It looks pretty much like any breast cancer presenting in the same way. Grossly, at the operating table, it resembles breast carcinoma, being a firm, hard, gritty mass. Microscopically, under low power, it is easily distinguished as a carcinoma because of its diffuse infiltrating nature throughout the breast.

[Slide] This is just another higher power view of this same tubular carcinoma. Its name is derived from the consistency of these proliferating little ductules, or glands, or tubules, as it were, infiltrating through this dense, fibrous stroma.

[Slide] This is a highpower view of this lesion. It causes pathologic ambiguity because of its very innocuous looking appearance. Under the microscope, it does not show much in the way of nuclear pleomorphism or mitotic figures. Nevertheless, it is a carcinoma because of its diffuse infiltrating nature.