# BRITISH MEDICAL JOURNAL



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## LEADING ARTICLES



## PAPERS AND ORIGINALS



## MEDICAL PRACTICE



### NEWS AND NOTES



### SUPPLEMENT



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BRITISH MEDICAL ASSOCIATION TAVISTOCK SQUARE LONDON WC1H 9JR TELEPHONE 01-387 4499 WEEKLY PRICE 42p

# CORRESPONDENCE

*Correspondents are asked to be brief*



## Treatment of Early Breast Cancer

SIR,—We fully expected criticism of our paper (20 May, p. 423) giving an account of our 10-years trial of two methods of treating carcinoma of the breast, and we hope that more will follow. What we did not expect, however, was the kind of criticism appearing in two of the letters published in your issue of 17 June (p. 711). The tone of one of these letters was reminiscent of the reception accorded to the *Origin of Species*, and we are at a loss to know why our modest contribution should have earned such an accolade. Perhaps the term "radical mastectomy" has the same emotive content as did "evolution" in 1859.

A letter in a previous issue (10 June, p. 652) from Miss Diana Brinkley and her colleagues referred to an impression that the casual reader might have received from our paper and proceeded to correct it. We had hoped that there would be very few "casual readers," certainly among those who proffered criticism. How wrong we were!

In the first of the three letters in your issue of 17 June, from Mr. J. W. S. Rickett and Mr. A. G. Nash, the opening paragraph, while couched in courteous terms, says, "it would appear that there is no case for the operation of 'extended tylectomy' in the management of stage I and II carcinoma of the breast in patients over 50 years of age." How anyone who has read the paper, even looked at the figures, could make such a statement passes our comprehension. They then ask whether, if there is skin dimpling, the skin is removed, or, in the subareolar lump, the nipple is removed. Although not spelt out, the answer to these questions is implicit in the protocol. We thoroughly commend their observation that the ratio of the size of the lump to the size of the breast is important, which is substantially what we stated in our paper.

The intended "sting in the tail" of their letter, where they complain that the word "tylectomy" combines both Latin and Greek roots, dismays and bewilders us. Is it τυλος or 'εκτομη which is the Latin root? If the first part of their letter makes one wonder whether they have read our paper, the last sentence destroys our confidence in their asseverations.

Although we cannot complain if people do not read our paper, we certainly object to those who misread it. In the letter from Dr. G. Edelstyn and Dr. K. D. MacRae the objection is made that we stopped the trial on stage II cases in favour of one of our two tested methods when the 10-year survivals were too few to be able to form a judgement. There follows some unsophisticated arithmetic to show why this would be so. Had we, in fact, based our decision on the fate of the "37" patients who were followed up for 10 years, this comment would have been a shrewd thrust.

What we did, and this is clearly stated, was to compare the two treatments using all the patients followed up variously for 10, 9, 8, 7, 6, etc. years by means of a life-table. Drs. Edelstyn and MacRae should know that the probability values attached to the difference of two contrasted treatments can properly be calculated using life-tables with no less accuracy than when other orthodox methods are used, and generally with greater refinement. The results showed that it would be irresponsible to continue with the trial on stage II cases. We must remember that, if the mortality rate at five years is greater after one method of treatment than after the other. the likelihood that the mortality after 10 years will show the same trend is enhanced, and to wait for 10-year survivals before calculating a result may be mischievous. It is necessary to avail oneself of all the admissible evidence and to act when this becomes conclusive.

The penultimate paragraph of this letter, written in the readily recognizable style of the first signatiry, makes the point that patients treated by tylectomy received more irradiation to their lymph nodes than those treated by radical mastectomy, and they further suggest that, as irradiation might suppress the immune response, the former patients were put at a disadvantage. However, in the third and far more temperate letter from Mr. E. Stanley Lee and others our protocol is reasonably and carefully criticized in that we did not give enough radiotherapy to either group. One is tempted to cry with Mercutio,

"A plague o' both your houses!
They have made worms' meat of me."

This third letter requires a more serious and complete reply than it would be possible to give here. The fact is that, so far as we are aware, the optimum radiotherapy programme in regard to complications and survival has never been put to the test of a controlled clinical trial. This letter concludes with the statement, "It is regrettable that fuller use was not made of the potentialities of radical radiotherapy (Miss Brinkley and colleagues described our radiotherapy as "radical"), the application of which would have made the conclusions of the trial more meaningful, and perhaps quite different." Perhaps not. As Mr. Stanley Lee and his colleagues observe, "Many radiotherapists *feel* that up to 6,000 rads given in six weeks . . . is necessary to prevent local recurrence of carcinoma of the breast" (our italics). May we make a plea that radiotherapists give up *feeling* and, in the Hunterian tradition, put the matter to the test.

This letter has the approval of my colleagues J. L. Hayward and A. B. Wayte, who would wish to be associated with it. Unfortunately, it has not been possible to obtain their signatures as they were not available in time to secure publication.

—I am, etc.,

HEDLEY ATKINS

Guy's Hospital,
London S.E.1