I welcome the opportunity of responding to Dr. Anglem’s article which challenges and unfortunately misinterprets my position regarding the treatment of operable breast cancer. In this, the second, critique, Dr. Anglem implies that I have quoted Cleveland Clinic’s survival rates after various procedures to show the superiority of simple over radical operations. In fact, I have consistently emphasized that Cleveland Clinic’s five- and ten-year survival rates following less than radical procedures seem comparable to results from other institutions which employ radical mastectomy for patients with similarly staged cancer. I have always reported the details of staging, the proportion of patients with noninvasive cancer, the type of operation performed, the reason for the selection of cases and the observed mortality rate, not the age-adjusted or determinate death rates often used by Dr. Anglem for comparison.

However, I agree with Dr. Anglem’s call for analytic detachment and objectivity in resolving this controversy and am fully aware that this is not “an academic matter.” I thus again suggest that the American Cancer Society and the National Institutes of Health choose an impartial but clinically knowledgeable statistician to review the records in the Cleveland Clinic breast cancer series from 1957 through 1962, at which time less than one percent of the operations performed were radical mastectomies. I hope the statistician then classifies a similar number of patients treated by radical mastectomies at another institution during this same time period to determine the proportion of five-, 10- and 15-year survivals. This suggestion is not made in the hope of discovering which is the best modality of treatment of breast cancer or which is the most appropriate for a given type of cancer. There is far too much selection in our cases to allow meaningful comparison of various treatment results. But, by carefully classifying our patients and those of another institution, it could be determined whether radical mastectomy had any greater value than the several lesser operations that we now employ. If the results of the radical operation are not distinctly superior, the operation should be totally abandoned since, in my opinion, fear of a radical operation is the most significant cause of delayed treatment.

For the present, however, I would like to clarify certain fallacies in Dr. Anglem’s presentation.

Simple Mastectomy

In discussing my reported 10-year end results in a series of 226 patients with breast cancer operated on between 1955 and 1959, Dr. Anglem critically states: “Only 57 percent of the radically treated patients had Stage I disease as compared to 76 percent of the patients treated by simple mastectomy—an initial and very
substantial bias in favor of the simple mastectomy group. In reporting these figures it is well to remember that I originally stated: "A comparison of results in such disparate groups is meaningless."

Dr. Anglem's Table 1 (page 331) shows a 36.5 percent 10-year survival rate for all patients with breast cancer treated at the Cleveland Clinic from 1955 to 1959. However, it must be recognized that patients with intraductal carcinoma were excluded from this 10-year survey and patients not followed during this time were counted as dead. Moreover, since many of these patients underwent radical procedures, the results do not reflect those obtainable by simpler treatments. For instance, calculations reveal that since 1957 when radical mastectomy was virtually abandoned at Cleveland Clinic, the 10-year survival rate was 42-43 percent, identical to that reported in patients treated predominantly by radical mastectomy. (Figures were obtained from the cancer registries of California, Connecticut and Massachusetts as well as from six university hospitals and published in the National Institutes of Health's, "End Results in Cancer," Report # 4, 1972.) The five-year survival rate at the Cleveland Clinic is 72 percent, 11 percent higher than that reported by the National Institutes of Health. The incidence of true Stage II breast cancer is the same in both groups. These figures suggest that the only difference in results among patients with cancers in comparable stages is that the radically treated patients die a little faster. (Table.)

| TABLE. COMPARISON OF STAGING AND SURVIVAL AT 5 AND 10 YEARS |
|-------------------------------------------------------------|
| Cleveland Clinic and National Cancer Registry                |
| Staging | Cancer Registry |
| True* Stage I | 52% | True* Stage I | 53% |
| True* Stage II | 48% | True* Stage II | 47% |
| Survival | Cancer Registry |
| Lived 5 years | 71% | Lived 5 years | 61% |
| Lived 10 years | 42**-43% | Lived 10 years | 42.8% |

*True Stage II means that involvement of nodes was either proved by histology at time of operation or appeared later in nonirradiated patients.

**Two Stage I patients lost between 6 and 10 years and counted as dead.
I also challenge the validity of Dr. Anglem's Table 2 (page 332) which compares survival data reported from centers in which radical mastectomy is the primary method of treatment by repeating what I said before: "There is simply no solid evidence based on randomized studies to indicate that higher survival rates are produced by one type of treatment than by another. To compare the survival rates in different institutions is meaningless because the staging and the stages of the disease may be different. Until we have solid data, I believe all of us would be better to keep our minds open and stop making ill-founded attacks on one another."

Although Dr. Anglem quotes some very high 10-year survival rates, it must be considered that the figures from Finney, Leber and Anglem represent survival rates of patients seen in private practice where tumors tend to be smaller and more favorable for treatment than those seen in an institutional practice where all cases are included. Finney confirms this by saying: "These figures cannot be compared with large unselected series since most of the patients were from middle and upper socioeconomic groups." In addition, some of Finney's survival figures are based on age-adjusted survival rates (a correction is made for patients dying of causes other than cancer) rather than on observed rates, causing a five to 20 percent higher rate depending on the age groups involved and whether survival is measured at five or 10 years. Despite these favorable factors, Finney's observed five-year survival rate, including patients with Stage I and II disease is only 68 percent compared with our 72 percent; the 10-year survival rate is 50.9 percent compared with our 42-43 percent. Also, Finney's 10-year rate in patients with operable cancer was not 55 percent as quoted by Dr. Anglem in his Table 2 (page 332) but 50.9 percent. Dr. Anglem derived the 55 percent figure by calculating treatment results in a selected and favorable group of patients treated by radical mastectomy, omitting from consideration 63 patients with operable cancers who because of age, infirmity or stage of disease were treated in other ways.

Payne's survival rate of 55 percent is not an observed survival rate but one that is based on actuarial or "life-table" expectancies and it also does not include poor risk patients or those with advanced cancers who were treated by operations other than radical mastectomy.

The high survival rate quoted from Robbins cannot be evaluated because it is based on a personal communication. Haagensen has always been highly selective in the patients he chooses for radical mastectomy (the title of his article is, "The Treatment of Early Mammary Carcinomas") and he uses the Columbia-Presbyterian classification which excludes from consideration many of the advanced cancers included in Stages I and II of our Manchester classification.
Finally, Dr. Anglem has misreported or at least misrepresented even his own figures. His 56 percent 10-year survival is not based on the observed survival of all his patients, but on the survival of his "determinate" group (patients who died of causes other than cancer or who were lost to follow-up before 10 years were not counted as dead). Anglem's observed 10-year survival rate, published in the article to which he refers, was only 50 percent and would have been lower if 54 patients with advanced disease had been included.

Even the most dedicated advocate of radical mastectomy could not contend that removing uninvolved axillary nodes increases the 10-year survival rate of patients with completely removed primary tumors. Even the most dedicated advocate of simple mastectomy could not ignore obviously involved axillary nodes. Therefore, discussions are centered on how and when the nodes should be treated. It must be remembered that since less than half of all operable patients have involved nodes and only a quarter of those with involved nodes are permanently cured, there can be no more than 12 percent difference in the rate of cure between the most radical treatment and no treatment at all.

Randomized trials have shown little or no difference in survival at either five or 10 years between radiation and surgery in the treatment of involved nodes.\(^2\) The question is then whether there is any advantage in removing or radiating nodes at the time of first treatment which, although not palpably involved, still contain occult cancer. Our studies show that in terms of survival at five years, there is no advantage. The same appears to be true at 10 years. Nine of 24 patients with occult cancer in nodes, to which no treatment was given at the time the primary tumor was removed, have lived 10 years. Twenty-two of these 24 patients were treated by delayed axillary dissection and two by cobalt therapy. One of the patients in the latter group is well 15 years after treatment and one patient died six years after treatment. Since the 10-year survival rate of these patients compares favorably with that of patients who had immediate axillary dissection for similarly staged cancers it is clear that immediate prophylactic dissection of nodes that do not appear to be involved gives no better results than deferring treatment until the involvement appears.

One also questions whether radical mastectomy might result in a higher survival rate than the modified radical operation. It is inconceivable that a radical operation could have any advantage over a modified radical procedure following which there was no nodal recurrence. And what is the incidence of axillary node recurrence after modified radical mastectomy? Our 10-year follow-up of 159 patients treated by modified radical mastectomy shows a local recurrence rate of seven percent; however, recurrence in axillary nodes occurred only
in one patient in this series, an incidence of less than one percent.

Again, I must challenge Dr. Anglem’s “further evidence of the poor performance record of conservative procedures . . . in those patients with more than minimal axillary node involvement” when he compares my series of 226 patients with his series of “an identical number of patients.” He is reporting 10-year results, whereas ours are 10- to 17-year results free of recurrence and thus the figures are not comparable.

I do not understand why my published figures have been altered. Dr. Anglem says that we used “local excision as the single form of treatment in only 25 (5.5 percent) of 406 patients treated between 1957 and 1966. In reality my article states, “Dr. Stanley Hoerr and I selected 52 patients with cancers in the periphery of the breast for treatment by local excision. These represent 11 percent of the total number of patients that were treated during that period.”

It is also difficult to see how Dr. Anglem can describe the patients selected for partial mastectomy as being an “extremely favorable group of patients with Stage I tumors” when my paper reads, “forty of the patients had clinical Stage I cancers and 15 had clinical Stage II.” This is clinical staging. By pathological staging, an additional 14 of the 40 patients would have been Stage II. Three of the patients were more than 80 years old, two more patients presented two of Haagensen’s grave signs and were judged inoperable, another was too sick to withstand a major operation and several others had axillary nodes described as more than 2 cm. in diameter. My article states: “Thus the patients with Stage II cancer were not selected because of their favorable status, but on the contrary were often treated by surgical biopsy and radiation because they were judged to be surgically incurable.”

Finally in respect to Dr. Anglem’s concern that partial mastectomies will be “inadequate operations for a large number of women,” one needs only to remember that many reports on local treatment of cancer, whether it be in the breast or rectum, indicate that local recurrence after local operation can usually be eradicated by a radical operation. In these patients, the end survival result is apt to be the same as patients with similarly staged cancer treated initially by a radical operation.

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