risk and evaluate criteria for timing of surgery. No significant increase in morbidity rate was documented by this study. We are confused by Dr. Frank's comments that seroma rates of 28% are unusually high; most studies document comparable (10% to 35%) seroma rates.[2]

Although we did not try to make a case for preoperative chemotherapy improving patient survival, we were interested in determining if surgical complications that delayed the reinstitution of systemic therapy might impact survival. A delay in instituting postoperative therapy in advanced primary breast cancer has previously been shown by us to impact survival, and this study only confirms and supports the previous report.[3] Dr. Frank is correct in stating that preoperative chemotherapy has not been shown to improve survival when compared with postoperative chemotherapy. Certainly, he would agree that all patients with advanced primary breast cancer should have aggressive systemic therapy. In addition, preoperative chemotherapy minimizes the extent of surgery required for effective local disease control. In our series, all patients had mastectomy without the need for chest wall resection, or skin grafting. It is not appropriate for him to compare survival in his series with our group of patients, because this analysis is meaningless.

We continue to support the use of preoperative chemotherapy in patients with advanced primary breast cancer. The high response rates allow surgical resection without the need for skin grafting or chest wall resection. Our study supports the safety of preoperative chemotherapy, and our criteria for the timing of surgery after aggressive chemotherapy seem appropriate. This information remains important because several cooperative groups are now using preoperative chemotherapy in clinical trials for less advanced breast cancers.

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June 19, 1991

Dear Editor:

The article by M. C. Wilhelm et al. (Nonpalpable Invasive Breast Cancer, 1991; 213:600–603) contains data for which there are alternate interpretations. First, the false-positive rate was 72%, meaning that almost three fourths of all biopsies were nonbeneficial. Second, the data are not population based and it is not known how many interval surfacing cancers occurred between screens. Third, the types of breast cancers detected by screening are more biologically indolent and have an anticipated longer survival as a result of the length bias sampling inherent in the screening method as well as a lead time bias. Fourth, in the absence of a population based study with controls, the statement cannot be made that the probability of dying of breast cancer has been reduced.

The fourth proposition has been reviewed by Eddy (Screening for Breast Cancer, *Ann Intern Med* 1989; 111:389–399). Eddy, analyzing all population-based controlled clinical trials, concluded that no more than an average of 25 woman-days of life can be anticipated from mammographic screening. For every 10,000 women screened for 10 years, there will be some 2500 false-positive diagnoses with biopsy. The cost for the general population will be about 1.3 billion dollars per annum.

The purpose of my comments is to raise caution in the interpretation of non–population-based, uncontrolled retrospective studies. They do not prove that the breast cancer problem is solved, and to imply that it is in such studies creates false expectations leading to a myriad of additional medical, medicolegal, and fiscal problems.

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July 19, 1991

Dear Editor:

We appreciate the opportunity of replying to Dr. Spratt's letter. We agree that retrospective institutional reviews require cautious interpretation. This paper sheds light on the prognosis of, and by extension, treatment requirements for women with invasive cancer detected by screening mammography. We do not think, however, that the breast cancer problem is solved.

The false-positive rate of mammography was 83% at our institution. This is similar to other large series, as outlined in the paper and discussion session. We believe that this is a major issue in breast cancer today. The high number of negative biopsies performed in the United States strains resources, is psychologically devastating to women, and may keep women from screening. Research with techniques such as stereotaxic localized sampling is necessary to reduce the need for surgical biopsy. This issue is irrelevant to the subject of the paper, however.

Similarly, Dr. Spratt's second and last points on the role of screening are important, but of little bearing on the issues we raise in the paper. Although Eddy argues that breast cancer screening is at best minimally cost effective, there are ample controlled population based data that screening reduces breast cancer mortality.

The last point is that screening-detected cancers are more indolent. As reviewed in our paper, we and others found metastatic nodal disease in screening-detected cancer with alarming frequency (about 20%). These women fare as poorly as women with larger tumors with involved nodes. For those without nodal metastases, this and other series, some with much longer follow-up, show that these women do exceptionally well. Indeed, they do much better than T1 N0 groups in controlled adjuvant trials. This suggests that women with screening-detected, node-negative, invasive breast cancer are a subset for whom the adjuvant therapy beneficial for node-negative women in general is not necessary.

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