

only palpation, as Daggett and others have suggested (*Lancet* 1: 483, 1981). Because of the frequent unsatisfactoriness of medical management for long-term hypoglycemia if a tumor is missed in these patients with negative preoperative imaging, however, the potential benefits of PVS is substantial. We have convincingly demonstrated that PVS is the single best preoperative localizing study and that intraoperative ultrasound (IOUS) is the single best intraoperative localizing maneuver. From a patient's perspective, if one had a rare, potentially life-threatening disease that had limited medical options for treatment, what would one want? From the viewpoint of cost-effectiveness, would it be correct to withhold a study that provides useful localizing information in at least three of four patients? The most cost-ineffective result would be unsuccessful surgery with persistent hypoglycemia. We have successfully avoided that problem in all but one of 12 patients with the methods described. Because of the rare nature of the problem and the remarkable success of our study (92%) in this subset of insulinoma patients with negative preoperative arteriographic studies, one can only do the same or worse by eliminating either PVS or IOUS, and not better.

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Dear Editor:

The article "Mastectomy Following Preoperative Chemotherapy" by Broadwater et al., published in the February issue of *Annals of Surgery*, contains misleading conclusions based on inappropriate comparisons between small and widely disparate groups of surgical patients. The preoperative chemotherapy group received less extensive surgery (12% modified radical mastectomy) compared with the mastectomy-alone group (61% modified radical mastectomy). Details of postoperative chemotherapy and radiotherapy are omitted.

The authors observe that there is no statistically significant difference between groups with respect to wound infection, necrosis, or delay in institution of postoperative chemotherapy, but fail to acknowledge that the *trends* observed are contrary to their null hypothesis (and could, conceivably, achieve statistical significance given a larger sample size). Stratification to determine differences in complication rates between *appropriate* groups by controlling such parameters as tumor size, local grave signs, or extent of surgery is not done. The "statistically significant" decrease in seroma rate observed in the preoperative chemotherapy group is not surprising because only 12% of that group received a full axillary dissection (*versus* 61% in the mastectomy-alone group). Although there was no significant difference between groups in the number of lymph nodes in the operative specimens, the numbers of nodes obtained are quite small (and documentation of how the nodal statistics are derived is not provided). The seroma rate of 28% in the mastectomy-alone group seems unusually high when compared with other modern surgical series.

The authors observe a significant decrease in overall survival among neoadjuvant patients when chemotherapy was delayed more than 30 days, but are unable to stratify this by stage or performance status. Similar analysis of the mastectomy-alone

group is not provided. Although "significance" was not achieved between the 30% of neoadjuvant patients and the 20% of the "control" patients whose postoperative systemic therapy was delayed, there is an absolute difference that might become significant with larger sample size. If delaying postoperative chemotherapy truly imparts a negative survival advantage, preoperative chemotherapy could conceivably be *detrimental* to survival.

The authors summarize that patient survival has improved with the use of preoperative therapy, but such a claim has been substantiated by neither their own study nor by randomized prospective trials comparing preoperative with standard postoperative chemotherapy. The case for neoadjuvant chemotherapy has been largely based on inappropriate comparisons between outdated historical control groups who received no chemotherapy and highly selected, favorable subgroups from nonrandomized neoadjuvant trials. At the Medical College of Virginia, a group of 118 patients with stage III breast cancer (1983 AJCC criteria, including 13 patients with stage IIIB inflammatory disease) treated with primary surgery and standard postoperative chemotherapy for positive nodes demonstrated a median survival of 69 months.<sup>1</sup> By comparison, the median survival of 54.3 months cited by the authors does not constitute an improvement in patient survival. Similarly modern prospective series using postoperative chemotherapy for locally advanced breast cancer document overall survival rates comparable to those touted by advocates of neoadjuvant therapy.<sup>2,3</sup>

#### References

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Dear Editor:

The purpose of our study was to examine the morbidity of aggressive preoperative chemotherapy in patients with advanced breast cancer to (1) determine if preoperative chemotherapy was safe, and (2) evaluate if our criteria for the timing of surgery were appropriate. This paper was not designed to analyze the efficacy of preoperative chemotherapy in the management of patients with stage III breast cancer.

There seems to be much confusion regarding the nomenclature for mastectomy. Extended simple mastectomy (ESM) is a mastectomy including a level I and level II axillary lymph node dissection. Modified radical mastectomy (MRM) is the same operation with the addition of removal of the level III lymph nodes. In this study and a previous analysis that we performed, these two operations are essentially equivalent with respect to morbidity and postoperative complication rates.<sup>1</sup> All patients with advanced primary breast cancer had the same postoperative chemotherapy and radiotherapy.

We agree with Dr. Frank that the sample size in our study is small and that a beta error effect could exist. This study, however, remains the only attempt in a large patient population with preoperative chemotherapy and mastectomy to define operative