6. Gharavi AE, Harris EN, Asherson RA, et al. Anticardiolipin antibodies: isotype distribution and phospholipid specificity. Ann Rheum Dis 1987; 46:1–6.

SAMY S. NITECKI, M.D.
Rochester, Minnesota

March 20, 1995

Dear Editor:

We appreciate Dr. Nitecki's interest in our paper. Dr. Nitecki recently has described an experience (*Eur J Vasc Surg,* 1993) with seven patients who had severe arterial and venous thromboses who were found to have antiphospholipid antibodies. Dr. Nitecki suggests that these patients may have been suffering from primary antiphospholipid syndrome. Dr. Nitecki suggests that it is these patients with antiphospholipid syndrome who are truly at risk for thrombosis and on whom therapeutic attention should be focused.

Interestingly, none of the patients in our study had clinical or serologic criteria for the diagnosis of systemic lupus erythematosus, nor were they being treated by drugs known to induce lupus-like disease. All of Dr. Nitecki's patients were identified as having primary antiphospholipid syndrome because of a history of severe episodes of thrombosis. Of course, we do not know the denominator from which these patients were drawn, nor the incidence of antiphospholipid antibodies in the general patient population undergoing vascular surgery at Dr. Nitecki's institution.

It was precisely to avoid this type of bias that we chose to perform a cross-sectional study in vascular surgery patients. The case finding approach to discovering patients with antiphospholipid antibodies, namely limiting testing to patients with severe symptoms, ensures that the antibodies will only be discovered in such patients. This means that an inappropriately morbid prognosis may be assigned to the presence of these antibodies.

We believe that our cross-sectional study clearly established that primary antiphospholipid antibodies in the absence of any lupus-like disease are found in a large number of patients undergoing vascular surgery at a tertiary referral center. Only some of these patients are severely affected by arterial and venous thromboses.

We thank Dr. Nitecki for his interest in our study.

LLOYD M. TAYLOR, JR., M.D.
Portland, Oregon

February 14, 1995

Dear Editor:

The recent article of Gordon et al. evaluating the cost and outcome of the Whipple procedure at Johns Hopkins Hospital *versus* Maryland's other hospitals is a template for future analysis of all institutions and their services. Unfortunately, it overlooks separate facilities which, within Maryland, have surpassed even its lofty standards. Peninsula Regional Hospital's statistics over the last 33 months show zero mortality, a length of stay of 20.5 days (preoperative diagnosis and postoperative care included), and a cost of \$22,559.65 for 13 patients undergoing Whipple procedures. One surgeon had superior results to these statistics, accounting for 5 of the 13 patients. It is obvious that the efforts of academic medical centers in training surgeons is paying dividends in improving patient care while reducing costs. To conclude that the excellence taught stops when the surgeon leaves the teaching environment is absurd. High-quality, affordable, accessible care is available in community hospitals because of the considerable efforts of the academic medical centers. These community hospitals are capable of providing sophisticated services while maintaining a community spirit if the public and third-parties are willing to evaluate each institution and physician on their own merits.

CRAIG J. SCHAEFER, M.D., F.A.C.S.

March 15, 1995

Dear Editor:

We would like to thank Dr. Schaefer for his kind remarks concerning our study, published in the January 1995 issue of the *Annals of Surgery.* We certainly are aware that a consecutive number of patients can undergo the Whipple procedure without mortality, having reported 145 consecutive pancreaticoduodenectomies without an in-hospital death in the May 1993 issue of *Annals of Surgery.*[1] We currently have performed our last 175 Whipple operations without an in-hospital death. We have to assume that at the Peninsula Regional Hospital, where they have done 13 consecutive patients without mortality, the patient who underwent surgery just before this series of 13 most likely died. If that patient is included, there has been 1 death in the last 14, for a hospital mortality of 7%. If their next patient dies, they will have two deaths out of 15, for a hospital mortality of 13.3%, almost exactly the mean mortality for the 38 community hospitals reported in our study. Thus, doing statistics on such a small series as reported by Dr. Schaefer in his letter is somewhat risky, and that is why we combined all 38 community hospitals in the state. In addition, during the 5½ years of Maryland state discharge data used in our study, there were 12 pancreaticoduodenectomies done at Peninsula Regional Hospital, with two deaths, for a mortality of 16.7%.

Perhaps a more important issue raised by Dr. Schaefer, however, is whether the institution or the individual surgeon is responsible for superior results for a high-risk operative procedure. Most studies[2–3] have found that large academic medical centers or regional providers with high volumes of complicated surgical patients have better outcomes. There is evidence that hospital volume is more important than individual surgeon volume.[4] In other words, as we point out in our article, it is the team effort, the experience and expertise acquired by caring for a large number of patients, and the commitment of institutional resources that result in the superior outcome, not simply an individual skilled surgeon, as Dr. Schaefer undoubtedly is.