The objective of our study was to determine the role of patients and physicians in detection of melanoma recurrence. Using our retrospective analysis, we were unable to answer the questions posed by Dr. Evans. Unfortunately, reanalysis of our data would not provide the answers to these important and intriguing questions because subgrouping of patients would result in numbers too small to perform meaningful statistical analysis. Additionally, much of the data needed to answer these questions were not recorded, a common problem within retrospective studies.

Dr. Evans should be complimented for his persistence in trying to answer these difficult questions concerning the surgical treatment of malignancy. His questions may be best answered in a prospective study, which will provide the most accurate data. Certainly these types of studies have changed forever the uniform use of radical surgery in the treatment of primary melanoma, regional melanoma metastases, breast carcinoma, and sarcoma. Most, if not all, of these data are published in peer-reviewed journals or discussed at medical meetings where the data had ample opportunity to be thoroughly scrutinized. Additionally, individuals have the opportunity to evaluate published data and come to their own conclusions, as Dr. Evans has. I would like to thank Dr. Evans for his insightful comments.

CHARLES R. SHUMATE, M.D.
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September 15, 1995

Dear Editor:

We congratulate Dr. Wade and associates for their outstanding study documenting the effectiveness of Whipple resection in patients with pancreatic cancer at Veterans Affairs (VA) hospitals.[1] However, we disagree with the authors' conclusion that patients experience little benefit from referral to high-volume regional centers. This conclusion was drawn from the comparison of volume-based experience across three groups of VA hospitals. However, we believe that the volume differences were too small to make valid conclusions regarding the relationship between institutional experience and operative mortality rates. The average VA hospital volume ranged from slightly more than two to less than one case per year, for an average operative mortality of 8% (n = 369), a higher rate than the experience of high-volume centers, as reported in this paper.

Wade et al. also concluded that lower mortality rates at referral centers occur because "referral centers encounter a younger patient population that tolerates an equal incidence of complications with fewer mortalities."[1(p257)] We concur that younger patients generally have lower mortality. However, in our study of Maryland hospitals, we found that the mortality differences between the high-volume referral center and the lower-volume hospitals did not change when adjusted for patient age.[2]

Most studies[3,4] have shown that large academic medical centers or regional providers with high volumes of complicated surgical patients have better outcomes, and we continue to support regionalizing high-risk surgical procedures in highly experienced centers.

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TOBY A. GORDON SC.D.
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October 1, 1995

Dear Editor:

Although "practice makes perfect" is intuitive, the applicability of this axiom to surgical outcomes has been tenuous at best.[1] We welcome the study from the state of Maryland by Gordon et. al.[2] as the fifth recent, population-based, multi-institutional report to document an 8% operative mortality rate (range, 6–9%) after a total of (now) more than 1600 Whipple resections.[3]

However, we must note that although the "regional provider" of Whipple resection in Dr. Gordon's study (Johns Hopkins) performed 54% of the resections in Maryland, with a laudable 2.2% operative mortality rate, the population's expected operative mortality rate of 8% was unchanged. Because Maryland's pancreaticoduodenectomy mortality rate was not lower than that of other populations, either: 1) surgical skills (and thus outcomes) in the other Maryland hospitals were notably inferior to those in U.S. Veterans Affairs hospitals, or 2) patients with a lower risk of operative mortality were selected for care by the regional provider.

We find the second explanation to be more plausible because the patients cared for by the regional provider had a series of qualities[2] associated with superior outcome: youth (p = 0.07),

commercial insurance ($p = 0.01$),[4] less pulmonary disease ($p = 0.01$), and a high incidence of benign disease (most recently 26% at Hopkins).[5] Thus, we believe the operative mortality rate after Whipple resection in Maryland was *not* reduced, but merely *redistributed*, by this very effective regionalization program and stand by our conclusion quoted by Drs. Gordon and Burleyson.

Especially in this current era of managed care, claims of superior outcomes must be balanced against the numerous, inherent, and variable risks in the patient population.[1] The advantages of risk avoidance surely are clear to Dr. Gordon, the Vice President for Planning and Marketing for the Johns Hopkins Health System.

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TERENCE P. WADE, M.D.
FRANK E. JOHNSON, M.D.
St. Louis, Missouri

October 30, 1995

Dear Editor:

We read with interest the letter by Drs. Wade and Johnson regarding our study of Whipple resections at Maryland hospitals.[1] Based on the data we presented, they conclude that the operative mortality rate at Johns Hopkins Hospital was lower than at other Maryland hospitals because patients with lower risk of operative mortality were selected by Hopkins. We agree with their assertion that certain patient characteristics are associated with improved outcome. However, the significantly lower mortality ($p < 0.001$), length of stay ($p = 0.05$), and hospital charges ($p < 0.001$) at Hopkins remained after we adjusted for differences in age, race, gender, source of payment, source of admission, and comorbidity. This led us to our assertion that regional medical centers, like Hopkins, have special expertise in procedures they perform in high volume.

Drs. Wade and Johnson also believe that "the operative mortality rate after Whipple resection in Maryland was *not* reduced, but merely *redistributed,* by this very effective regionalization program. . . ." We did not provide statewide mortality trends in our article. However, if redistribution of low-risk patients occurred, it would be expected that over time, Hopkins mortality would decrease, other hospital mortality would increase, and overall Maryland mortality would remain constant. Examination of the data, however, shows all three mortality rates have been decreasing while Hopkins' share of discharges has been increasing. In the first 6 months of 1995, 72% of Maryland patients undergoing the Whipple procedure were treated at Hopkins, and the statewide mortality rate was 5%. This is a substantial improvement on the 17% statewide mortality rate of 10 years ago, when Hopkins had only a 30% share of Maryland Whipple cases.

We believe that our study and the data that we have presented in this letter document the appropriate regionalization of care for one high-risk surgical procedure. The high-volume provider has increased its share of patients, reducing the statewide mortality rate. Although other providers have improved mortality rates over time, the most recent 6 months of data indicate that the relative risk of dying in a low-volume hospital is ten times greater (14.3% *vs.* 1.4%) than at the high-volume provider.

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TOBY A. GORDON, M.D.
GREGG P. BURLEYSON, R.N., M.H.S.
JAMES M. TIELSCH, PH.D.
JOHN L. CAMERON, M.D.
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June 30, 1995

Dear Editor:

We read with interest the article written by Deitch et al. regarding the effect of nutrition on intestinal epithelial barrier function.[1] Dr. Deitch and his coworkers present a very important study eliciting the role of nutrition-induced epithelial barrier failure for bacterial translocation. In contrast to chow-fed rats, rats fed intravenous total parenteral nutrition (IV-TPN) and elemental diet had bacterial translocation, as shown by histology and bacteriology.