LETTERS TO THE EDITOR

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Dear Editor:

We read with interest the article by Broome et al.[1] The authors investigated quality of life after surgical treatment of necrotizing pancreatitis and reported good long-term outcome. Information on late results after surgery for necrotizing pancreatitis is limited. Therefore, the information given by Broome et al. is very important because it helps to justify the expensive costs of therapy in these patients. However, a few remarks on patients and methods in this study may be made.

Some data support the conclusion that the majority of patients in this study presented with milder forms of pancreatitis: the average APACHE II score on admission was rather low (9.0); more than a third of all patients were treated sufficiently by drainage and minimal debridement; "scheduled" operations could be performed in 23% of patients; and a fourth of all patients did not require intensive care.

In contrast to these facts, the authors demonstrated a high rate of renal failure (25%) in their patients. However, criteria of renal failure were not defined. Average hospitalization was 70 days, which seems to be long because lavage procedures or laparotomies were not applied.

Costs were calculated in 14 selected patients with median hospital stays of approximately 20 days less than average. As the authors indicated, reported costs surely underestimated real expenditure.

Long-term data were incomplete because 30% of patients were lost to follow-up.

In a recently published study,[2] we could demonstrate good long-term results after open treatment for necrotizing pancreatitis.[3] In a group of critically ill patients (average APACHE II score on admission: 16, percentage of patients with failure of at least one organ system on admission: 75%, rate of infected necrosis: 82%), late outcome was investigated.

We could achieve complete follow-up. Development of diabetes was the main problem in 29% of all patients. Compared with preoperative findings, functional state and employment were unchanged in the majority of cases (70%). No patient needed regular medical or nursing care as a result of necrotizing pancreatitis. Self-assessment of quality of life demonstrated good results in 77% of patients.

In conclusion, long-term outcome after open treatment for severe cases of pancreatitis was good and justified high costs of therapy. Our results confirm data published in the study by Broome et al.[1]

References

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3. Armbruster C, Kriwanek S, Beckerhinn P, et al. Differential therapy of acute necrotizing pancreatitis. Acta Chir Austriaca 1994; 26:259–264.

STEPHAN KRIWANEK, M.D.
CHRISTIAN ARMBRUSTER, M.D.
Vienna, Austria

Dear Editor:

We would like to thank Dr. Kriwanek and associates for their response to our article. Although it is true that the population of patients requiring debridement in our study presented with low APACHE II scores, results may have been biased because many patients were transferred from outside institutions after being stabilized. For the purpose of this study, acute renal failure was defined as serum creatinine levels greater than 3.0 mg/dL or failure that required dialysis. The average creatinine level in the study population was 6.2 mg/dL. Also, although renal failure was noted to be a major complication (25%), by far the greatest complication was fistula formation (43%). New onset of diabetes occurred in four (18%) of our patients postoperatively, but it was noted by three of the four that their diabetes was "easy" to control and did not adversely affect their quality of life. Financial data presented probably were a gross underestimation of actual costs because data available were for those patients hospitalized most recently (since 1991) and those whose stay was some 20 days less than average for the entire population. Unfortunately, 30% of patients were lost to follow-up. Had these patients been included, results for quality of life may not have been as good. Continued study of this subject is needed greatly because there is a paucity of information available. In addition, managed care and the issues surrounding quality of life are becoming increasingly more important.

AUDREY H. BROOME, R.N., M.S.N.
Durham, North Carolina

April 29, 1996

Dear Editor:

We read with interest the correspondence resulting from the article by Dietch et al.[1] We feel that the response of Drs. Marino and Deitch[2] to the criticisms offered by Riegler et al.[3] should not go unanswered.

Drs. Marino and Deitch are incorrect in asserting that the dimensions describing specific electrical resistance R of a membrane are in ohms per square centimeter (ohm/cm^2). The correct dimensions are resistance units $\times$ cm^2 (or ohm $\times$ cm^2), as noted by Riegler et al. The application of a current across a flat surface or an epithelium results in a current density, I, described as current units per unit surface area (amperes/m^2 or microamp/cm^2). The voltage deflection generated by the application of this pulse usually is described in millivolts. Thus, by Ohm's law, $R = E \div I$, and the calculation of the membrane resistance is: E (mV) $\div$ I (mcA/cm^2) = R (ohm $\times$ cm^2).

This also can be understood intuitively. Consider a copper surface characterized by the specific resistance R (ohm $\times$ cm^2) and its inverse property specific conductance G (mho or Siemen/cm^2). The overall resistance of the surface decreases, and the conductance increases, as the diameter increases. If, as Drs. Marino and Deitch assert, the specific resistance were described in dimensions of ohm/cm^2, then increasing the diameter of the copper surface would increase its overall resistance to current flow.

Drs. Marino and Dietch have misquoted Bertil Hille.[4] In fact, on pages 8 to 10 of his classic monograph, Dr. Hille gives the dimensions of specific resistance as ohm $\times$ cm^2.

We also would reiterate the concern expressed by Riegler et al., that measurements of P.D. and R may be altered in some circumstances without necessarily reflecting alterations in active ion transport by an epithelium. Caution always must be used when measurements of such transepithelial electrical parameters are taken to reflect events at the level of the epithelial cell membranes and paracellular pathways.[5]

References

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David I. Soybel, M.D.
Jeffrey B. Matthews, M.D.
William Silen, M.D.
Boston, Massachusetts

August 8, 1996

Dear Editor:

Soybel et al. are free to disagree with our choice of electrical units, but they have no basis whatever to label it ''incorrect.'' Nothing in their comment, even if accepted at face value, justifies such a charge. Continued discussion of the correctness of electrical units such as they invite is pointless because the choice of units always is governed by convenience, not physical theory.

In our opinion, the points raised by Soybel et al. are subjective and of no practical concern because none of the points raised would change the interpretation or meaning of the data in our manuscript—i.e., whichever choice of units was used, the results would be the same. Lastly, because we did not quote Bertil Hille, we could not possibly have misquoted him.

Andrew A. Marino, Ph.D.
Edwin A. Deitch, M.D.
Newark, New Jersey

Dear Editor:

Dr. Hay has done a thorough study comparing different methods of hernia repair.[1] I have two comments about the portion of his report that refers to the technique of a Cooper ligament repair.

First, Dr. McVay did not include the inguinal ligament and believed strongly that it was important to bring the transversus abdominis arch down to the Cooper ligament to restore normal anatomic planes.

Second, a relaxing incision was used in only 75% of the repairs. Estimation of the amount of tension on a repair under anesthesia is unreliable, and a relaxing incision is an essential part of a Cooper ligament repair.[2] It should always be done.

It would be interesting to know if his data show a difference in the recurrence rates of those Cooper ligament repairs done with or without a relaxing incision. The technique as described is not a true Cooper ligament repair.

References

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2. Rutledge RH. Cooper's ligament repair: a 25 year experience with a single technique for all groin hernias in adults. Surgery 1988; 103:1–10.

Robb H. Rutledge, M.D.
Fort Worth, Texas

May 2, 1996

Dear Editor:

Dr. Rutledge is entirely correct in underscoring that the Cooper ligament repair used in this study is not exactly that of McVay. This modification was outlined in the section methods of our article.

However, including the inguinal ligament in the suture does not prevent bringing the transversus abdominis arch down to Cooper's ligament. The needle purchases the transversus arch, the inguinal ligament, and then the Cooper ligament. The ingui-