

# Letters to the Editor

November 5, 1987

Dear Editor:

The article by Drs. Hayes, Bolton, Willis and Bowen provides a good overview of the currently accepted natural history and operative management of ampullary carcinoma. Their results demonstrated that the quality of the pancreatic remnant, in terms of the degree of fibrosis and ability to hold sutures well, correlated significantly with the rate of leakage from the pancreatico-jejunal anastomosis. I would like to take issue with their claim that ". . . this is the first report to show in a statistically significant way that the status of the pancreatic remnant strongly influences the fate of the pancreaticojejunostomy." Doctor J. P. Lerut and co-workers actually demonstrated this very same correlation in a study of 103 pancreaticoduodenal resections, 24 of which were performed for "periampullary" malignancies.<sup>2</sup> The statistical test used by Dr. Lerut was the same as that used by Dr. Hayes, with a greater difference actually being found in the former case ( $p < 0.03$ ).

I found the study by Dr. Hayes and his colleagues to be very informative and would like to thank them for their work.

ERIC R. FRYKBERG, M.D.  
Jacksonville, Florida

## References

1. Hayes DH, Bolton JS, Willis GW, Bowen JC. Carcinoma of the ampulla of Vater. *Ann Surg* 1987; 206:572-577.
2. Lerut JP, Gianello PR, Otte JB, Kestens PJ. Pancreaticoduodenal resection: surgical experience and evaluation of risk factors in 103 patients. *Ann Surg* 1984; 199:432-437.

November 30, 1987

Dear Editor:

We are indebted to Dr. Frykberg for his comments about our paper, and for bringing to attention Dr. Lerut's article, which deals with issues similar to those raised by our series.

However, there are substantive differences between Dr. Lerut's series and our own. Dr. Lerut's series documents the occurrence of 15 pancreatic fistulas in 103 patients who underwent pancreaticoduodenal resection for a variety of benign and malignant conditions. Included among the 15 pancreatic fistulas were three patients with a "poor quality" pancreatic remnant who were managed by pancreatic ductal occlusion, *not* by pancreaticojejunostomy. Many prior reports have demonstrated a high rate of pancreatic fistula after pancreatic ductal occlusion in patients undergoing pancreaticoduodenal resection. If these three cases are excluded from Dr. Lerut's analysis, and the rate of *pancreaticojejunostomy* leak is compared between the group with a fibrotic pancreatic remnant and the group with a normal pancreatic remnant, we find that there is no significant statistical difference ( $\chi^2 = 2.54$ ,  $p > 0.1$ ). In addition, Dr. Lerut's series includes two patients who suffered fatal pancreatic fistulas after emergency pancreatico-duodenal

resection for acute necrotizing pancreatitis, an extremely high-risk situation that is not encountered in patients undergoing elective resections for malignant disease.

Thus, not all of Dr. Lerut's pancreatic fistulas were true pancreaticojejunostomy leaks, and the wide variety of clinical conditions dealt with in his article confounds a close comparison with our own series. Our series is a more homogeneous group in that all patients had carcinoma of the ampulla of Vater, all underwent elective resection, and in every case the pancreatic remnant was anastomosed to the jejunum. Thus, our series focuses more clearly on the technical problems facing the surgeon carrying out a pancreaticojejunostomy after pancreaticoduodenal resection for malignant lesions of the ampulla. As demonstrated in our paper, in this group of patients, construction of the pancreatico-jejunosotomy was the single most important technical factor influencing the clinical outcome of pancreaticoduodenal resection.

JOHN S. BOLTON, M.D.  
New Orleans, Louisiana

December 14, 1987

Dear Editor:

Dr. Thomas S. Reeve and his colleagues should be complimented on their excellent paper, "Total Thyroidectomy: The Preferred Option for Multinodular Goiter," published in the *Annals of Surgery*, 1987; 206(6):782, and for their superb results. "There were no permanent complications when a total thyroidectomy was performed." These authors certainly demonstrate that a total thyroidectomy can be performed by experienced surgeons with minimal morbidity. I would suggest *caution*, however, in accepting their statement, "We recommend, therefore, that total thyroidectomy is the procedure of choice for multinodular goiter where the entire gland is involved." Although multinodular goiter almost always involves both lobes of the thyroid gland, one lobe or side is usually more involved than the other. During the past few years have the authors expanded their definition of indications for total thyroidectomy in patients with multinodular goiter or do more of their patients currently have extensive bilateral involvement of the entire thyroid gland? The authors in the discussion section of their paper state that, "In experienced hands, however, the incidence is acceptably low; for example Clark (*Ann Surg* 1982; 196:361-370, reports an incidence of 1% permanent hypoparathyroidism and no permanent recurrent laryngeal nerve damage in 82 consecutive total thyroidectomies. More than one third of these were performed for benign thyroid disease." Although this statement is true, there were reasons why total thyroidectomy was performed in these patients—the most common reason being a history of exposure to low-dose therapeutic irradiation. In this population, about 40% of the patients will have thyroid cancer but in only 60% will the cancer be in the dominant nodule. Because the position of the cancer is not always known, the entire thyroid is removed.

For most patients with multinodular goiter I would recommend *not* to perform a total thyroidectomy, the vast majority of these patients will do well with a total lobectomy on the side

of any dominant nodule or suspicious nodule that might be cancer by aspiration biopsy cytology, and a partial or subtotal resection of the contralateral lobe. Although all large (greater than 1.5–2.0 cm) or palpable nodules in patients with multinodular goiter should be removed, it is not necessary to remove the entire gland. I have one other question. What is the authors' rate of recurrent goiter in patients who have been treated by less than total thyroidectomy?

ORLO H. CLARK, M.D.  
San Francisco, California

but the preferred option for the patient, provided that the surgeon is comfortable with the operation required to meet that option.

T. S. REEVE  
Sydney, Australia

#### Reference

1. Reeve TS, Delbridge L, Brady P, et al. Secondary thyroidectomy; a twenty year experience. *World J Surg* (in press).

January 10, 1988

Dear Editor:

Thank you for the opportunity to reply to Dr. Clark's comments. We believe that there is a certain ambivalence in his comments in that, while he acknowledges our basic observation that multinodular goiter almost always involves both lobes of the thyroid gland, he then proposes that multinodular goiter should be treated in a manner not in general accord with well-accepted surgical principles—namely, to excise all diseased tissue when it is surgically safe to do so. This surgical tenet promotes the preservation of important anatomic structures unless they are involved in the disease process. Hence, while it is seen as desirable to perform total thyroidectomy in multinodular goiter, if anatomically necessary, a lesser procedure should be performed on the less affected side.

As we have pointed out, it is necessary to fully mobilize both lobes of the gland to ensure that no residual nodular thyroid tissue with growth potential is left *in situ* after thyroidectomy. This is important since "suppressive" therapy with thyroxine is not guaranteed to retard remnant growth.

It is the fundamental pathology of the disease and the anatomical features of the posterior lobule that have led us to expand our indications for total thyroidectomy in benign disease, and with increasing experience we are continuing to do so. In our unit, the percentage of cases where multinodular goiter has been treated by total thyroidectomy has increased from 50% in 1985 to 72% in 1987. We have also been able to achieve and maintain a morbidity pattern that is at least equal to that for subtotal thyroidectomy.

Dr. Clark rightly questions the rate of recurrence in our series of patients who have had less than total thyroidectomy. An idea of the rate of recurrence of multinodular goiter and the problems it poses can be gained from the following figures from our Unit. In the period 1976 to 1985, there were 311 secondary thyroidectomies performed, 173 of which (56%) were for recurrent multinodular goiter. During that same period, the total number of operations for multinodular goiter was 853; thus, over 20% of the surgeries performed for multinodular goiter was for recurrent disease. Of the patients who experienced recurrences, 64 (37%) had their initial surgery performed in this unit, and the remainder elsewhere. When one considers that the complication rate for secondary thyroidectomy is considerably greater than that for the initial procedure,<sup>1</sup> we believe that, in the patient with disease involving the entire gland, a total thyroidectomy is not only justified,

Dear Editor:

Since I read some of the issues of the *Annals of Surgery* with some delay, I can only now comment on one of the more interesting articles, "A Successful Cardiac Transplantation Program Using Combined University and Community Resources" by L. A. Gray, Jr. et al. Their contribution deserves the highest acknowledgement of the reader. The authors' final conclusion—that the orthotopic cardiac transplantation has become an effective treatment for end-stage heart-disease—seems perfectly right and justifies the enormous efforts of the pioneers of this field of medicine and surgery.

This is why I cannot agree with the first sentence of the article, "After the *first* human cardiac transplantation in 1967 . . . .",<sup>1</sup> citing C. N. Barnard's report, in which even the author did not use the word "first." But he did use the word "successful"<sup>1</sup> and I could accept the introductory sentence of C. Gray's article had he used this word, instead.

The first—and unfortunately, unsuccessful—human cardiac transplantation—considering of course the acceptor and not the donor, which was a baboon, due to compulsion—was performed by Mr. James Hardy and his team 3 years earlier, in 1964, and it was published in *JAMA*.<sup>2</sup> Unfortunately, since 1964, this milestone of surgical intervention has been overlooked several times, even in American medical literature, (for example, in an editorial of *American Journal of Diseases of Children*<sup>3</sup>).

It is typical human behaviour to consider as "first" only the first *success*. Whether good or bad, there are many examples of this misconception in the history of medicine. The partial gastrectomy is attributed to Billroth (1881), although it was performed earlier—albeit unsuccessfully—by Rydygier (1880) and Pean (1879).<sup>4</sup> Nor was it Whipple who made the first duodenopancreatectomy in 1935, but Sauv  (1908) and Kausch (1912),<sup>5</sup> and the original technique did not include gastric resection, as we believe and still perform today. Many distinguished surgeons contributed to the so-called Whipple procedure.<sup>6</sup> The pylorus-preserving pancreatoduodenectomy is considered Traverso and Longmire's modification (1978), but it was first suggested and performed by Watson in 1942,<sup>7</sup> as well as *successfully* performed. And, on your side of the Atlantic, who, beside your Oliver Wendell Holmes,<sup>8</sup> knows our Ign c Semmelweis's name, who discovered asepsis, long before the discovery of germs?<sup>4</sup>

Of course, the main point is whether the patient benefits. Nevertheless, even here in Hungary, I have had my own strug-