Retropharyngeal Abscess and Endotracheal Intubation

To the Editor:

Heath and Peirce were kind enough to publish their report entitled “Retropharyngeal Abscess following Endotracheal Intubation” (Chest 72:776-777, 1977). The key to the problem probably lies in the fourth sentence of the case report, where Heath and Peirce note that “the intubation was difficult, requiring a number of passes via oral and nasal routes” (p 776). During the attempted nasal intubation, it seems probable that the nasotracheal tube dissected through a plane of tissue after (or during) the turning of the tube in the back of the nasopharynx. The tube then probably dissected between the planes of tissue. If this supposition is correct, an opening to the retropharyngeal space was provided, and the nasotracheal tube passing through the nose would have picked up a fair number of bacteria to seed the space. The nasotracheal tube employed for the attempted nasal intubation is not noted. If it was a stiff or hard one, such as the red rubber type, it would support my supposition.

When performing a nasal intubation, one should “keep listening.” The end of the stethoscope can be taped to the end of the tracheal tube so all breath sounds can be heard. Fittings are available that will slip onto the 15-mm connector and have a nipple that can be used for direct attachment to the tubing of the stethoscope. As the patient was probably obtunded and not breathing deeply, extra caution would be necessary.

We must all thank Heath and Peirce for sharing their experiences. Nasal intubations are not always easy, and their case report draws attention to a possible (probable?) complication.

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Pros and Cons of Surgery for Aortocoronary Bypass

To the Editor:

Thomas A. Preston’s editorial, like his book,1 vigorously, effectively, and one-sidedly presents the arguments against surgery for coronary arterial bypass as an accepted treatment for coronary arteriosclerosis, except in patients with lesions of the main left coronary artery. He begins from what some antagonists believe is an impregnable position, ie, the infallibility of the prospective randomized trial. Properly performed, such trials may give us powerful insights, but all studies have their faults, and in the field of bypass surgery, the prospective randomized trials have had their share. Some of the problems of the Veterans Administration’s cooperative study have been detailed elsewhere.4 A general theorem in this field (which, like other general theorems, may be unjust in some cases) states that centers that have good surgical results report them; centers with poor surgical results report prospective randomized studies.

Preston’s point that symptomatic improvement is difficult to evaluate in angina has general agreement. That we can discount all of the reports of symptomatic improvement to date as “simplistic” is less obvious. The
public will continue to demand the surgery because of its effect on symptoms. The profession can fortunately turn to other criteria, such as longevity. The caveat in the assessment of longevity is that the poorer the initial selection and surgery for coronary bypasses, the longer it takes for the favorable results of surgery to appear. Several reports show that good surgery can achieve rates of survival that are close to those of age-matched and sex-matched groups of the general population.++

Surgery on asymptomatic patients with angiographically demonstrable lesions that threaten the integrity of a functioning segment of myocardium cannot be dismissed cavalierly. In a disease where the first symptom may be myocardial infarction or death, insistence upon pain as a necessary part of the indication for surgery seems hazardous.

Little of value comes from speculations about the motives of one’s adversaries. Such points as financial rewards for cardiac surgery may be worth considering privately but contribute little to public debate. Similar speculations about the motives of those charged with holding down medical costs probably balance the scale and are equally inappropriate subjects for profitable discussion.

I see little merit in Preston’s³ suggestion that only articles that meet his standards of validity should carry a label of “class 1” and that others should be labelled “class 2.” If an editor accepts Preston’s dictum that the sole road to truth comes from the prospective randomized trial, he will reject reports, instead of labelling them as “class 2.” The author’s recourse is to try another editor.

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6 Kirklin JW: Research related to surgical treatment in coronary artery disease. Read before the National Heart, Lung, and Blood Institute, Bethesda, Md, Feb 7, 1978

To the Editor:

Greene brings out several points that are important in the controversy over coronary arterial surgery. Those who espouse controlled clinical trials probably know the limitations of such trials better than those who oppose them. As I said in my editorial,¹ controlled studies are the most accurate and ethical, especially in contrast to uncontrolled studies. I know of no one who believes this to be an “impregnable” position, and controlled studies certainly are not “infallible” (nor does anyone claim they are). The point that is relatively indisputable is that controlled studies are much less likely to give us inaccurate or misleading data than are uncontrolled studies, and to disdain the use of controlled studies when they are practical may be unethical, as it is rejecting the best means of obtaining accurate data. That there are limitations of controlled studies is well to keep in mind, but it is no reason for rejecting them. To state that I or others advance controlled trials as “the sole road to truth” is an obfuscation that delays understanding.

I agree with Greene’s statement that poor selection leads to difficulty in assessing coronary arterial surgery. Although some reports show that surgical patients have rates of survival close to those of the age-matched and sex-matched general population, we must remember that patients without surgery in the age group receiving the operation also have similar rates of survival.² Simply stating that surgical patients have rates of survival “close to” the normal population is another example of how we may mislead ourselves. The good survival may be due to surgery, it may be due to selection of patients who are good risks, or it may be due to other factors. We need a properly controlled study to find the answer.

I agree that we cannot and should not dismiss the possibility of using surgery to prevent myocardial infarction or death. But shouldn’t we first have evidence of benefit? Isn’t it “cavalier” to perform prophylactic surgery without first getting evidence that such a practice is in the public interest? If we do not insist upon evidence of efficacy prior to widespread use of a type of therapy, we condone and encourage the use of any unvalidated therapy.

Why is it that we must never discuss financial considerations? In any other enterprise involving the public interest, there is recognition and discussion of the central role of economics. The medical profession’s unwillingness to discuss a very important aspect of the operation is insupportable when it is primarily the public, not the profession, whose interest is at stake. Unwillingness to discuss what may be the most important factor in the overall utilization of coronary arterial surgery very likely could result in nonprofessional regulation of this and other activities that involve the public, and not just the profession.

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1 Preston TA: The hazard of poorly controlled studies in the evaluation of coronary artery surgery (editorial). Chest