SURGERY FOR ACQUIRED HEART DISEASE

EDITORIAL (CON) RE MINIMALLY INVASIVE PORT-ACCESS MITRAL VALVE SURGERY

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The article titled "Minimally Invasive Port-Access Mitral Valve Surgery," by Professor Mohr and his colleagues in Leipzig (see page 567), reports an interesting clinical experience in the context of the current interest in "minimally invasive" cardiac surgery by the business community, the public, and many of us in the thoracic surgery profession. Clearly, our profession is committed to performing cardiac surgery with the least possible morbidity for our patients. In this sense (and, by and large, in the operative technical sense), the various minimalistic approaches are not really new and certainly do not represent the "genie out of the bottle" phenomenon that is sometimes rather hyperbolically suggested. Smaller incisions, gentle handling of tissue and judicious use of retractors, avoidance of fracturing ribs, minimizing cautery, smaller and fewer chest tube sites, use of short-acting anesthetics and early extubation protocols, more liberal use of regional blocks, and even use of local anesthesia in combination with general and regional anesthesia are all part of the rapidly changing picture in performing cardiac surgery with less morbidity. Although the authors of this article state that "the aim of the study was to minimize surgical access," surely our focus should be on minimizing trauma rather than access. Minimizing access implies maximizing technical difficulty. Moreover, as we have learned with thoracoscopic lung surgery, effective access to and exposure

of relevant anatomy can be even better with video-assisted approaches in some kinds of operations.

The authors of this study should be commended for their objective and candid report of the clinical results. Bypass and crossclamp times, postoperative intubation times, and intensive care unit and hospital stays were all relatively long in this early experience. Two serious and clinically evident aortic dissections occurred, and both required emergency resection of the aorta, with one patient dying in the operating room. Three mitral valve repairs were unsuccessful, and three of the 23 patients undergoing valve replacement had perivalvular leaks and required a second operation. Three patients required reexploration for bleeding. Mortality was 9.8%. When the authors compared these patients retrospectively with other patients having mitral valve operations at the same institution, they observed no decrease in pain. The article states that the aim of minimally invasive procedures is "reducing postoperative pain, recovery time, and cost," and the information in this article suggests that these goals were not realized.

Although neurologic data are not presented in this article, "the high incidence of postoperative confusion" is mentioned, and we have seen numerous reports of neurologic problems with this approach described on the Internet. This increased incidence may be due to anatomically distorting effects of the balloon in the arch, retained air, or embolic phenomena. In any event, this is an important consideration.

As the authors acknowledged, demonstration of "feasibility" does not constitute a useful analysis of the relative merits of this approach as compared with standard cardiac procedures. Some have suggested the importance of a "learning curve," but it is important to remember that the central features of this approach are not new. Cardiopulmonary bypass, arguably the salient cause of morbidity, is conserved. Bypass is not new and not a factor in the learning

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Requested for publication Sept. 23, 1997; received Nov. 18, 1997; accepted for publication Nov. 19, 1997.

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J Thorac Cardiovasc Surg 1998;115:563-4

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0022-5223/98 \$5.00 + 0 12/6/88011

curve. By the same token, cannulation of the femoral artery and femoral vein are, for most cardiac surgeons, very familiar techniques and not part of a learning curve. Valve repair or replacement via thoracotomy is also quite familiar to many practicing cardiothoracic surgeons, not part of a learning curve. Thus further experience with these techniques is not likely to yield significant benefit.

The central hypothesis that needs to be tested is the hypothesis that multiple port sites, insertion of pulmonary artery vent and coronary sinus cannula by the anesthesiologist, and femoral cannulation for cardiopulmonary bypass taken together are significantly less morbid than median sternotomy. Parenthetically, the use of terminology such as "endopulmonary event" and "endoaortic clamp" obfuscates rather simple and well-established technical concepts.

In this and other proposed approaches to "video-assisted" thoracic surgery, the analogy with laparoscopic cholecystectomy is usually raised. With open cholecystectomy, the morbidity of the operation was largely related to the incision. This, of course, is not true for cardiac surgery. Avoidance of morbidity of the muscle-splitting abdominal incision in cholecystectomy was new. However, performance of incisions on the lateral chest wall is not new. Ablative surgery (laparoscopic cholecystectomy) via a surgical telescope was new to most general surgeons in the early days of laparoscopic cholecystectomy. Thoracoscopic surgery is not new to most cardiothoracic surgeons. Femoral-femoral bypass is well-established; cardiopulmonary bypass is well-established; performance of valvular surgery via thoracotomy is well-established. Thus the suggestion that reservations about this approach represent simple resistance to change is probably unfounded.

The decision to perform the operations in this

report was reviewed by a local ethics committee, and the patients were given the opportunity to sign a written consent form. Although the article does not include the specifics of this form, the informed consent should include a clear explanation of the fact that we do not know whether cardiac surgical procedures can be performed in this way with the same safety and outcomes that are expected with a standard approach via median sternotomy. The consent form should also state that we do not know yet whether this approach will result in less pain and a shorter hospital stay. Regrettably, patients are sometimes told that this procedure is "FDA approved," although the Food and Drug Administration has no jurisdiction over operations.

All of these considerations underscore the continuing compelling need for rigorous outcomes analysis in cardiac surgery. First, we need to continue to analyze what factors *actually constitute* important causes of morbidity. Technologies that hypothetically address these factors should then be evaluated. Second, new technologies such as this one should be studied in randomized prospective comparison with conventional techniques. Otherwise, any assertions about lesser morbidity or lesser cost will never be proven. More important, we will never know whether it is appropriate to offer these modalities to our patients.

This approach may have important application in selected patients. In general, we must maintain our commitment to reduction of morbidity and mortality, to offering better value to our patients, and to technical innovation (new devices, as well as fundamentally new strategies—e.g., transmyocardial channel formation, gene and growth factor transfer). These goals can be achieved only by dispassionate examination of the data and by well-designed prospective comparative studies.