The fiberscope is also useful in patients with ankylosing lesions of the cervical spine or other disorders preventing extension of the neck. In fact, in such instances, the fiberscope provides a method of inserting endotracheal tubes for the anesthesiologist. Finally, the fiberscope may be indicated in the endoscopic examination of some debilitated individuals who may not tolerate hyperextension of the neck (eg those with brain tumors or cerebral vascular lesions). In such cases, however, one should bear in mind that the fiberscope does compromise the airway and at all times an open tube bronchoscope should be available in case it is needed.

It is our opinion that bronchoscopic examination, whether performed with the rigid or fiberoptic instrument or not, should be done by individuals who are fully trained in endoscopic techniques and capable of handling any problem that may arise. We strongly recommend that residents in thoracic surgery, thoracic medicine and otolaryngology be trained in open tube bronchoscopy and be able to use both the rigid and flexible instruments. The well-trained endoscopist will learn to use both instruments skillfully and will be able to handle any emergencies that may arise. Either examination should be done with a minimum of discomfort to the patient and with all possible speed. It should be borne in mind that usually examination with the open tube bronchoscope can be accomplished much more quickly than with the fiberoptic instrument.

It is also our opinion that bronchoscopic examination, whether performed with a rigid or flexible instrument, should be carried out in a facility which is equipped for endoscopy. Thus, the procedures are best done in an operating room or specially designed unit for bronchosophagology or in an intensive care area where all available safeguards are available to the endoscopist. Except under special circumstances, we do not advocate the performance of bronchoscopy in the patient’s room or in the doctor’s office; but this does not imply that hospitalization is necessary for endoscopic examination. Many patients can be handled on an outpatient basis.

Again it should be emphasized that the bronchofiberscope is a valuable addition to the armamentarium of the endoscopist. It has its greatest usefulness in the early diagnosis of lung cancer. It should be used, however, by qualified bronchoscopists under optimum circumstances.


Reprint requests: Section of Publications, Mayo Clinic, Rochester, Minnesota 55901

The Enigma of the Vineberg-Sewell Implant Operation

Of the many procedures advocated and popularized for the relief of myocardial ischemia, the original Vineberg operation (implantation of the internal thoracic [mammary] artery into the ventricular myocardium) had one of the longest latent periods. Not until after angiographic evidence of implant patency and communication with coronary arterial branches had been demonstrated by Sones and Shirey (about 16 years after Vineberg’s first report), did this seemingly improbable procedure (or its modification by Sewell) become popular in this country. Many experimental studies followed, and surgical procedures were performed on several thousand patients. However, the experimental studies were conflicting, and there seemed to be almost as many clinical reports which were favorable as those which were unfavorable or inconclusive. For these reasons, a cooperative study of the problem involving a randomized controlled clinical trial was begun in 1968 by several Veterans Administration hospitals. As it turned out, the study was probably begun too late to provide definitive answers. After fewer than a hundred patients had been entered into the study, attention and interest shifted almost completely to the newer aorto-coronary saphenous vein bypass operations. Very few implants have been performed in Veterans Administration hospitals in the past several years. In like manner, interest in the procedure has waned almost to the vanishing point among most cardiac surgeons in this country.

Yet, there were occasional cases in the experience of most surgeons dealing with sizable numbers of patients, in which at subsequent operation for whatever reason, deliberate temporary or inadvertent permanent interruption of flow through the implant was associated with either a significant arrhythmia, a significant alteration in the ischemic electrocardiographic pattern, or, in a very few instances, in death. In these instances, at least, there was fairly suggestive evidence of dependence of the myocardium on the blood flow provided by the implant. These cases were few and far between, to be sure, but they appeared to vindicate the hypothesis on which the Vineberg concept was built. Thus it was perhaps premature to assign the internal thoracic implant operation to the proverbial closet, already rather full of the skeletons of previous operations devised for myocardial ischemia, and later discarded. In effect, this has already happened. As matters now stand, most recent studies of ventricular function following the implant operation are not encouraging. How-
never, the precise effects of the implant operation on morbidity and mortality remain inconclusive, and the opportunity to determine these in the future is almost surely now gone. The most opportune time, early in the application of the operation, was lost. This has been the case with almost every other operation for this disease. The methods of assessing the effectiveness of surgical procedures for myocardial ischemia were, and for the most part continue to be, imprecise and nonobjective. In a condition with the complexity and variety of coronary arterial occlusive disease, with its multiple anatomic and physiologic variables and risk factors, it is a delusion to think that "proper patient selection" can answer the question of measurable therapeutic effectiveness, that is, whether or not the surgical procedure in question significantly affects morbidity (incidence of subsequent myocardial infarction, or of congestive failure) and longevity. There is no safe or logical alternative to the randomized controlled clinical trial, for answers to these questions. However, there is no longer any chance to accomplish such a trial with the implant operation, and, indeed, there would be little justification now for doing so, since the advent of direct myocardial revascularization procedures.

The studies which are available are mostly uncontrolled. Examples of such studies are published elsewhere in the issue of Chest (see pages 227, 235). Dr. Kay's paper, specifically, poses a number of serious problems. The patients reported represent the evaluation of a mixed bag of surgical procedures, i.e., implants, with and without ventricular excisional surgery. These patients represent but a small fraction of the total number of patients on whom operation was performed. The technique of measuring volume flow (cineangiography) is of questionable validity. The differences in measurements of ventricular function before and after operation are of doubtful statistical significance.

Our series can justifiably be criticized also, in that it is composed mostly of single implants, in many instances for diffuse disease. At least it has the virtue of complete follow-up over a longer period.

Aside from those issues, however, neither Dr. Kay's study nor ours is likely to shed much additional light on this subject, because in neither study are there appropriate controls nor statistically valid objective measurements. Thus, the Vineberg-Sewell operation remains an enigma. It has raised several intriguing questions. Why does the freely bleeding implant not produce a hematoma in the myocardium? When anastomoses form between the implant and the coronary artery, why do they develop only with arterial tributaries of coronary vessels, and not with the venous branches, (in which blood pressure is considerably lower)? Why does the ventricular myocardium in a small minority of cases seem to be dependent on the implanted internal thoracic artery, while in the vast majority of cases, no relationship can be established between patency of implants and either relief of symptoms, or longterm benefit? Finally, does the operation deserve any place at all in the surgery of coronary arterial occlusive disease? It is a rather sad commentary on our scientific methodology that the latter question would now probably have to be answered in the negative, after many hundreds of patients have been operated on in totally uncontrolled series. And only a handful of objective pre- and postoperative measurements of various parameters of left ventricular function are available to muster as evidence for or against the operation. We ought to be able to do better than that in the future.

Timothy Takaro, M.D.  
Oteen, N.C.

*Chief, Surgical Service, Veterans Administration Hospital.  
Reprint requests: Dr. Takaro, VA Hospital, Oteen, North Carolina 28805

Chronic Obstructive Pulmonary Disease: Some Thoughts on the Current State of our Knowledge

Despite intensive investigative efforts, the two most common forms of lung disease afflicting patients in this country, emphysema and bronchitis (chronic obstructive pulmonary disease [COPD]) have, to date, managed to escape any real advances in diagnosis or longterm management. Having arrived at this depressing conclusion about COPD, it seems worthwhile to examine what we actually know, or think we know, about this elusive entity. Our efforts in this direction are confined to three areas which have occupied our particular interest. They are: the early detection of COPD, its course, and the results of therapy.

Early Detection

Early detection, detection prior to the onset of symptoms, has been hampered by the shortcomings inherent in pulmonary function tests. These shortcomings are in the main due to the uncertain range of "normal," which, in turn, is largely due to the variability of the tests themselves. This variability springs from the fact that the most widely used and practical method of early detection, spirometry, is highly dependent on the subject's motivation and performance. This poor reproducibility, although