extrinsic obstruction of the trachea is an extremely rare phenomenon. Elder et al. mentioned a rare patient with mediastinal lymphoma who required immediate tracheostomy. Our patient did not present with peripheral bulky disease and her chronic lymphatic leukemia was in partial remission. In these reasons we did not suspect upper airway obstruction due to adenopathy until tomogram and subsequent surgical removal of enlarged lymph nodes was performed. We can find no previous mention of this occurrence and we suggest stridor occurring in patients with chronic lymphatic leukemia requires a thorough evaluation to rule out this etiologic possibility.

Charles F. Winkler, M.D.,
Roberto Corpus, M.D.,
Veterans Administration Medical Center,
Louisville, and Trover Clinic, Madisonville

Bilateral Carotid Body Resection

To the Editor:

In reply to the letter by Dr. Chai (Chest 75:534, 1979), his outline of medical therapy for treatment of severe asthma in children is not unique, but is in common use by competent physicians and may correctly be called "conventional" therapy. However, this therapy, although competently administered, may fail to control the disease in a certain percentage of "intractable" patients. For such patients, the Winter technique of bilateral carotid body resection can be recommended as safe, and capable of substantial long-term benefits. Warning symptoms of hypoxemia are not necessarily present following this surgery, and most patients report ability to sense hypoxic environmental conditions. A few, against advice, have remained in such environments in spite of symptoms, and have reported loss of symptoms through some adaptive mechanism.

As to the abstracts in my bibliography which appeared to confuse Dr. Chai, the studies of pulmonary function1 concerned adult patients with chronic obstructive airways disease and showed mean values for FEV1 of predicted to be improved 13 percent at two days postoperation, with 24 percent improvement at one year or more. The "deterioration" reported for 23 percent of the study group, referred to all reduced postoperative values as compared to preoperative data points including minor differences, and indicated statistical information without clinical relevancy. Since all consecutive patients were included, some adult severe asthmatic subjects with little underlying obstructive airways and normal, mild, or moderate pulmonary impairment, were included. The studies of perfusion lung scans2 concerned adult patients with chronic obstructive airways disease, for whom such scans had been called "irreversible" on the basis of failure to obtain improvement following maximal medical therapy under optimal hospital conditions.3 In 135 of such patients, independent rating by two experts in nuclear medicine, showed unequivocal major degrees of improvement in 31 percent at two days postoperation and 48 percent of patients studied at one year or more. The mechanism of action of bilateral carotid body resection does not involve hypoxic ventilatory response, which is largely lost following surgery, but appears related to major reduction of afferent input into the hyper-reactive chemoreceptor bronchoconstric-
tor reflex system of patients with asthma and other obstructive airway diseases. The resultant effect appears to be a rise in activation threshold of the reflex system, reduction of triggering, reduction of bronchoconstriction in the lungs, emptying of secretions, and associated clinical improvement.4

Finally, I find it incomprehensible that a single, admittedly deficient case report, could form the basis for insistence that bilateral carotid body resection is unnecessary and that the risks are unacceptable. I contend that such statements are without justification or merit, and a disservice to patients who are in desperate need of the benefits that this surgery can provide.

Benjamin Winter, M.D., F.C.C.P.
Los Angeles

Reprint requests: Dr. Winter, 6233 Wilshire Blvd, Los Angeles 90048

REFERENCES


REFERENCES


CHEST, 77: 2, FEBRUARY, 1980
Young Adults Surviving Myocardial Infarction

To the Editor:

The recent report by Warren et al (Chest 75:667-670, 1979) merits comment. They found, in a group of 68 patients under the age of 36 who had suffered a myocardial infarction, nine (13 percent) had normal coronary arteries on angiogram.

There is no doubt that we are seeing younger patients in our coronary care units with their first myocardial infarction. Most cardiologists are subjecting these patients to coronary arteriography as part of their post discharge evaluation for the reasons cited by the authors. Their incidence of arteriographically normal coronary arteries seems quite high, but the series comprises a select group of patients. They all survived their infarctions and some were referred from other hospitals. It must be realized that this was not a consecutive series of all young myocardial infarction patients seen in the coronary care unit and then subjected to angiographic evaluation.

The implications of the study by Warren and colleagues are significant, especially in light of recent knowledge concerning the pathophysiology of ischemic heart disease. Coronary vasospasm must be considered in all patients with angina and/or myocardial infarction, especially in the absence of significant atherosclerosis. Dual channel ambulatory monitoring (even in the absence of chest pain) may aid in its detection. In a recent study in three patients with variant angina, 1045 spontaneous episodes of ST segment elevation were observed, 89 percent of which were asymptomatic.1 Ergonovine testing during diagnostic coronary angiography should also be considered in such patients. Even though the sensitivity and specificity of ergonovine testing for the detection of coronary vasospasm is not defined, the test appears to be highly specific in patients with typical Prinzmetal’s angina.2 It has recently been reemphasized that patients with coronary vasospasm may have one or more myocardial infarctions even in the absence of atherosclerosis.3

Two clinical conditions which may mimic ischemic heart disease and/or be associated with myocardial infarction in the absence of atherosclerosis, must also be considered. The first is the mitral valve prolapse syndrome. These patients often have electrocardiographic changes suggestive of ischemic heart disease; furthermore, they can present with acute myocardial infarction and angiographically normal coronary arteries.4 Coronary spasm has been suggested as the underlying mechanism and receives further support by a recent study. Buda et al5 retrospectively found 11 patients with angiographic spasm (eight were at the catheter tip), and nine had evidence of mitral prolapse. We have recently encountered four patients with the prolapse-click syndrome and evidence of coronary spasm (Mautner RK, Phillips JH; unpublished observations). Hypertrophic obstructive cardiomyopathy (HOCM) is a second condition in which patients may present with angina and electrocardiographic evidence of infarction. Furthermore, Maron et al6 recently reported seven patients with HOCM who, at necropsy, had evidence of transmural myocardial infarction with no significant atherosclerosis of the extramural coronary arteries. This study and our recent report of three patients with HOCM and associated coronary vasospasm7 suggests a causal relationship.

In conclusion, coronary artery spasm should be considered and its documentation sought in patients with ischemic syndromes and no demonstrable atherosclerosis. The total contribution of spasm in the clinical area has not yet fully unfolded; however, there is no doubt that its impact is significant.

Richard K. Mautner, M.D., and
Guy Katz, B.S.,
Tulane University Medical School,
New Orleans

REFERENCES

6 Maron BJ, Epstein SE, Roberts WC: Hypertrophic cardiomyopathy and transmural myocardial infarction without significant atherosclerosis of the extramural coronary arteries. Am J Cardiol 43:1086-1162, 1979

Effort Independence and Forced Expiratory Flow

To the Editor:

The May, 1978 issue of Chest contained an editorial by Dr. Sobol entitled, “Effort Independence and Forced Expiratory Flow—End of an Era?” I have not seen any reaction to this editorial since that time, and therefore, I am writing this letter.

Dr. Sobol suggested that the term “effort independent,” as applied to the flow-volume curve, be abolished. I would like to take exception to this suggestion and to the implications of such a recommendation.

As I see it, Dr. Sobol’s thesis is that since measurements from the flow-volume curve, such as maximal flow at a given percentage of the vital capacity, are not totally reproducible, it therefore follows that the entire concept of relative effort independence of maximal expiratory flow is no longer a useful or valid concept. The term effort independence was coined to describe the phenomenon of expiratory flow limitation, namely: there is a limit to maximal expiratory flow at a given lung volume such that pressures in excess of the lowest pressure necessary to produce maximal flow do not increase the flow. In the early description of this phenomenon, it was emphasized that there was a reproducible precise limit to