Is a VO2 Plateau Necessary?

Oxygen consumption at maximal exercise (VO2max) has been measured for many years. It is one of the "gold standards" utilized to measure the maximal aerobic capacity of an individual. Among the uses of VO2max are to evaluate the change that occurs after training; to help assess functional capacity, by comparison with the value in healthy individuals; and, by comparison with the population average VO2, to assess the effectiveness of fitness programs in school systems.

In the practice of pediatrics, there are clinical situations when the improvement in "fitness" comes into question, such as the following: Can patients who have undergone cardiac surgery show a training effect? Do patients with cystic fibrosis improve with a long-term pulmonary therapeutic plan? Are there beneficial effects of treatment of chronic renal failure with erythropoietin?

The traditional research laboratory exercise evaluation on a treadmill or a cycle ergometer has included the assessment of oxygen consumption. There are several methods that can be utilized, including a mass spectrometer or an apparatus utilizing a mixing chamber. The article in this issue by Rowland and Cunningham (see page 485) has utilized the latter method. Previous work cited by these authors comprehensively reviews the knowledge extent with respect to reaching of an oxygen consumption plateau during dynamic exercise. Data are available for both treadmill and cycle ergometer, and previous work has suggested that there is an anaerobic threshold reached at approximately 65 to 70 percent of the maximal VO2. This implies that anaerobic metabolism is utilized from this stage until the maximal workload can be performed. The implications of an anaerobic threshold are beyond the scope of this article.1 There are significant questions with respect to whether it is a technical difficulty or a difference in physiology that led to such a low percentage of children who achieve a plateau in VO2.

Whether a tight-fitting face mask or a traditional mouth piece is utilized during exercise in pediatric patients, both may contribute to the limitation of exercise time because of discomfort or fear on the part of the child. Thus, many theoretical issues may never be clarified.

Rowland and Cunningham have demonstrated that 15 of 21 children who volunteered for study had sufficient data available for analysis. Of these 15, only five had a true plateau of their VO2 measurements. Inspection of Figures 1 and 2 shows a continued steady rise of VO2 for those children who did not reach a peak. The ramp protocol utilized changed the workload each minute, and it may be argued that steady state was not achieved at the end of any minute. In early work by Gordon Cunningham,8 it was noted that children require approximately 1 to 2 min in order to achieve a "steady state" during aerobic exercise. This may have been one of the technical limitations to the study by Rowland and Cunningham. In any case, the other criteria utilized for maximal exercise seemed to be convincing enough that lactate levels are unnecessary. This reviewer believes that a trained pediatric exercise technologist can assess a maximal effort adequately to ensure accurate data for any clinical study and for virtually any research study. The requirement that VO2 plateau within strict parameters may not be necessary for comparison of "maximal" studies.

Bruce S. Alpert, M.D.
Memphis

Department of Pediatrics, University of Tennessee College of Medicine.

REFERENCES

Still Going Around on the Flow-Volume Loop

The association between atypical configurations of the flow-volume loop and upper airway obstruction due to structural lesions has been recognized for two decades.4,5 These abnormal configurations are usually described in quantitative terms as the ratio of the forced expiratory flow to the forced inspiratory flow at mid-vital capacity (FEF50/FIF50). Customarily, a normal value is considered to be less than 1.

Approximately ten years ago, another atypical flow-volume loop configuration was identified in patients with obstructive sleep apnea and was termed the "sawtooth" sign.5 At its conception, this sign was thought to reflect airflow oscillations due to underlying upper airway instability and to represent a marker during wakefulness of clinically relevant abnormal upper airway function during sleep. Because of the importance of detecting obstructing lesions in the upper airway, as well as the substantial practical advantages

CHEST / 101 / 2 / FEBRUARY, 1992 301
associated with the availability of a test performed during wakefulness that predicts abnormal upper airway function during sleep, there has been considerable interest in further examination of both saw-toothing and the FEF\textsubscript{50}/FIF\textsubscript{50}. Unfortunately, despite numerous investigations, insufficient light has thus far been shed to assist clinicians in concluding if, when, and how these flow-volume loop parameters should be used.

Surprisingly, the sensitivity and specificity of the FEF\textsubscript{50}/FIF\textsubscript{50} for anatomic lesions of the upper airway have yet to be determined by large-scale studies. The need for such data is highlighted by the relatively small study by Rotman et al., who reported no significant difference in the FEF\textsubscript{50}/FIF\textsubscript{50} when comparing 14 normal subjects with 11 patients with structural obstructing lesions of the extrathoracic or intrathoracic upper airway. In view of these findings, it seems inappropriate to uniformly conclude that the absence of an increased FEF\textsubscript{50}/FIF\textsubscript{50} eliminates the possibility of an obstructing lesion or that its presence is definitive evidence of one. Moreover, as indicated by the data of Rotman et al. and the data of Neukirch et al, which appear in this issue of Chest (see page 425), there is probably substantial variability in the “normal” FEF\textsubscript{50}/FIF\textsubscript{50}, and unity may not invariably be the single most appropriate cutoff value. Thus, while it may be reasonable to be concerned about the possibility of upper airway obstruction in an individual patient with a “plateau” on the inspiratory limb of the flow-volume loop, too little is known about this variable to justify its use as an epidemiologic tool.

The literature addressing the significance of saw-toothing on the flow-volume loop is conflicting and controversial. The sensitivity and specificity of this sign as a predictor of sleep apnea have varied widely among studies, making its utility difficult to place in proper perspective. Sensitivities have ranged from less than 29 percent to 85 percent, and specificities have varied from 54 percent to 100 percent. These wide ranges limit confidence in the use of the saw-tooth sign as a clinical screening tool and as an epidemiologic instrument for sleep-disordered breathing.

Potential contributors to the great variability in the data include differences in recording devices (i.e., computer-sampling algorithms may smooth the curves), nonuniform definitions of sleep-disordered breathing among studies assessing the saw-tooth sign, and the presence of factors promoting upper airway dysfunction during sleep that were not present during performance of the flow-volume maneuver, and vice versa (e.g., alcohol). Perhaps the most significant factor is the absence of uniform criteria which define saw-toothing. It has become evident that even the relatively detailed definition of Sanders et al leaves too much freedom for individual interpretation. This is exemplified in an article by Vincken and Cosio, which illustrates by 40 flow-volume loops putatively demonstrating saw-toothing. It is clear, however, that the saw-toothing contours of the different loops are widely disparate and may reflect different physiologic or pathophysiologic processes.

Finally, it has been reported that saw-toothing may reflect the presence of upper airway dysfunction irrespective of its etiology, having been identified in patients with extrapyramidal neurologic disorders, as well as in individuals with injury to the upper airway. Unfortunately, polysomnography was not performed in these studies, and it remains possible that sleep-disordered breathing was indeed present in these patients with saw-toothing on their flow-volume loops. Therefore, to properly assess the potential relationship between a laboratory or clinical sign and upper airway dysfunction, it is essential to conduct evaluations of patients during sleep as well as during wakefulness, since the integrity of upper airway function may vary with a sleep/awake status.

In summary, even several decades after recognition of these “abnormal” flow-volume loop configurations, more uniform standards remain to be defined, data-collection techniques need to be standardized, and complete epidemiologic evaluations need to be conducted in both sleeping and awake populations. Until this work is completed, the full clinical value of the flow-volume loop in general, and of saw-toothing in particular, will remain uncertain. The report by Neukirch et al in this issue of Chest, noting a 13 percent prevalence of saw-toothing on the flow-volume tracing in a population of French policemen, provides an excellent study base for such an investigation. However, the success of this effort will require that the investigators define saw-toothing in very specific, quantifiable terms and assess its relationship with objectively documented upper airway abnormalities during sleep as well as during wakefulness. Concurrently, the sensitivity and specificity of the FEF\textsubscript{50}/FIF\textsubscript{50} for upper airway abnormalities should be clarified. Carefully performed, comprehensive studies such as these are time consuming and difficult. However, after 20 years of questions, it is time for some answers.

Mark H. Sanders, M.D., F.C.C.P.* and Ronald A. Stiller, M.D., Ph.D., F.C.C.P†

Pittsburgh

*Associate Professor of Medicine and Anesthesiology, University of Pittsburgh School of Medicine, and Director, Pulmonary Sleep Evaluation Center, Presbyterian University Hospital.
†Assistant Professor of Medicine and Anesthesiology, University of Pittsburgh School of Medicine, and Assistant Director, Medical Intensive Care Unit, Presbyterian University Hospital.

REFERENCES
1 Jordanoglou J, Pride NB. Comparison of maximum inspiratory
and expiratory flow in health and in lung disease. Thorax 1968; 23:38-45
5 Sanders MH, Martin RJ, Pennock BE, Bogers RM. The detection of sleep apnea in the awake patient. The “saw-tooth” sign. JAMA 1981; 245:2414-18
6 Botman HH, Liss HP, Weg JC. Diagnosis of upper airway obstruction by pulmonary function testing. Chest 1975; 68:796-99

**Aortic Dissection**

**The Diagnostic Dilemma Resolved**

A cute aortic dissection is the most common aortic pathology in the United States needing emergent diagnosis and treatment. The prognosis is decidedly dismal, with a mortality rate of 25 percent within an hour of the event, 50 percent cumulative mortality at one week, and 90 percent at one year in untreated or unrecognized cases. Prompt and accurate diagnosis is critical in determining the outcome of this medical and surgical emergency. Available technologies include angiography, contrast-enhanced computed tomography (CT), magnetic resonance (MR) imaging, and combined transthoracic-transesophageal echocardiography. It is important to remember that one or more of these technologies should be employed when the suspicion of acute aortic dissection has been entertained after using the mundane, often forgotten, but very useful methodology called history and physical examination. The issue of which of the available technologies to utilize in order to provide the best care to the patient is a subject of debate.

The principles involved in choosing the investigation for the diagnosis of aortic dissection from a surgeon’s perspective can be summed up in three essential steps: (1) confirmation of dissection, (2) determination of ascending aortic involvement, and (3) demonstration of abnormal anatomic features. However, there are several other important issues to be considered in choosing the imaging modality best suited to making or excluding the diagnosis of aortic dissection. These include the rapidity with which the test can be obtained in a given hospital; the skill of the interpreting physician and technologist performing the examination; the experience with the technique in a given center; the proven cost-benefit advantage (let us not forget the era in which we are living); and the specificity, sensitivity, and positive predictive accuracy of the test. If we keep all of these factors in mind while selecting the imaging modality or a combination of complementary modalities, we will be able to deliver high-quality care at a reasonable cost, without jeopardizing safety, in our patients with suspected aortic dissection.

Angiography, considered one of the gold standards for the diagnosis of aortic dissection, has at best an accuracy rate of 95 percent to 99 percent.3 False-negative results are not uncommon and are shown in one of the cases reported by Chan in this issue of *Chest* (see page 406). The procedure itself has a defined mortality and morbidity. Erbel and his colleagues,4 in a multicenter European trial involving 164 patients with suspected aortic dissection, demonstrated that angiography had a sensitivity of 88 percent and specificity of 94 percent.

The sensitivity and specificity of contrast-enhanced CT are as good as, and in most instances better than, those of angiography. Pooled data indicate that it has a sensitivity of 82 percent and a specificity of 100 percent.5 In the multicenter European trial reported by Erbel and his colleagues,4 sensitivity and specificity were 83 percent and 100 percent, respectively.

Magnetic resonance imaging has been shown to have high sensitivity and specificity of up to 98 percent, as reported by Christopher and his col-

CHEST / 101 / 2 / FEBRUARY, 1992 303