followed by premature RB beats, we must assume that RB complexes do not express conducted sinus impulses but are, in fact, escape beats, also suggested by their short H-V interval.

The phenomenon, however, would have been the same with any other form of second-degree A-V block provided that two levels of block occur, and no retrograde conduction of the escape impulses is manifested.

Other arguments against conduction of sinus impulses through Mahaim fibers include: no delta wave is evident in RB beats, and the direction of the initial vectors of ventricular activation is the same for both the RA and RB beats.

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Predicting Postpneumonectomy FEV1
Half Empty or Half Full?

To the Editor:

Drs. Ladurie and Ranson-Bitker are to be congratulated on reporting one of the largest and longest series of patients in whom FEV1 was predicted postpneumonectomy using split function tests. This technique has been used clinically now for over ten years and seems to have gained some utility and support. These investigators, however, express disappointment with the accuracy of the FEV1, predicted postoperatively using either bronchiospirometry, ventilation/perfusion scanning with multiple detectors, or Anger camera. FEV1 measured one year after pneumonectomy and then annually for up to ten years in some of their 159 patients seems, to them, to vary excessively from the predicted value. Their results recall the pessimist who views the glass as half empty; is there another viewpoint? If viewed optimistically, 87 percent of their patients were potentially benefitted in that the test was accurate enough to permit a surgical approach to a disease which without surgery has a grim prognosis. As with any good study, as many questions are raised as answered.

Was there any difference in predicted compared to observed postpneumonectomy FEV1 in the patients with preoperative FEV1 <2 L vs. >2 L? It would be interesting to know this since the patients in the study had a mean FEV1 of 2.4 L. I have never applied (nor advocated) this technique with patients whose FEV1 exceeds 2 L preoperatively.

What were the criteria used to accept or reject patients as to their physiologic operability? Most investigators have used a predicted postpneumonectomy FEV1 of 0.8 to 1 L as the lower limit, but we have suggested prospectively testing 30 percent of predicted normal as a possible lower acceptable limit.

Was there any immediate (<30 days) postoperative cardiorespiratory death? In our experience, despite the prediction, mortality in the patients with preoperative FEV1 <2 L may be as high as 18 percent. Having only six patients with difficulty postoperatively and two with cor pulmonale at ten years seems to be an excellent result.

Were all patients maximally treated for their COPD at the time of preoperative (and postoperative) FEV1 measurement, and preoperative repartition? We have seen patients preoperatively who, after therapy, experience improved FEV1, so much as to no longer need split function testing. Is it also possible that the right-left distribution of ventilation and perfusion vary from day to day in COPD? This has not been tested using radionuclide techniques, but is a real problem in using the lateral position test.

In summary, the prediction of postpneumonectomy FEV1 may not be the best test for predicting postoperative cardiopulmonary function. Its primary advantage is that it seems to work and is less invasive than pulmonary artery balloon occlusion and bronchiospirometry, which it supplanted. Will it in turn be supplanted by VO2max?

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To the Editor:

Here are some answers to Doctor Olsen's questions.

There is no difference in predicted compared to observed postpneumonectomy FEV1 <2 L vs >2 L (FEV1 <2 L, 48 patients, 6 conflicting results; FEV1 >2 L, 111 patients, 29 conflicting results).

In our hospital the lower result for operability is around 30 percent of predicted FEV1 value. Of course the predicted values used should be indicated (we use the CECA values, which are rather high) and some patients with only one functional lung have been operated on with a much lower FEV1.

This series concerns the postoperative FEV1; and all the patients survived at least one year; however, we studied another series of 609 patients who underwent pulmonary resection between 1981 and 1983. Postsurgical mortality inhospital does not depend significantly on FEV1 being under or above 60 percent of the predicted value. After pneumonectomy, mortality is 15 of 305 (4.9 percent); FEV1 <60 percent, four of 56 (7.1 percent); and FEV1 >60 percent, 11 of 290 (3.8 percent). After lobectomy, mortality is nine of 304 (2.9 percent); FEV1 <60 percent, one of 69 (1.5 percent); and FEV1 >60 percent, eight of 230 (3.4 percent). In the same series, the difference in

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postoperative morbidity is highly significant between the two groups for both pneumonectomy (p<0.01) and lobectomy (p<0.001).

It is difficult to say that all the patients were maximally treated for their COPD before the surgical procedure. Recent cases have been much better prepared than the former ones operated on during and after 1970.

All the same, we would like to state again the aim of our paper, which some readers did not seem to understand clearly. It was not meant to deny the value of predicted FEV1 calculation, but to discuss the reasons for its inaccuracy in 23 percent of our patients.

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Vectorcardiography and COPD

To the Editor:

I have read with great interest the article of Lebowitz et al.1 The authors state firmly the validity of the vectorcardiographic examination (VCC) to evaluate the cardiovascular consequences of chronic obstructive pulmonary disease (COPD) and show several changes related to it.

I’d like to commend the detailed description of the data they obtained in their study, as well as the complete and careful statistical analysis presented by the authors. All this leads, in my opinion, not only to valuable conclusions from an epidemiologic point of view, but also to useful criteria for the clinician to improve and make easier the early diagnosis of COPD and of the associated changes in the pulmonary circulation and in the right side of the heart, and even the risk of developing these problems.

However, I would like to point out that a VCG parameter is omitted which, in my opinion, is very efficient for detection of overload/hypertrophy of the right ventricle. Furthermore, it can be easily measured and quantified, which is why it seems especially appropriate to analyze VCCs of great population samples, as was the case with the study of Lebowitz and coworkers.1

I am referring to the measurement of the different quadrants of the three planes (frontal, horizontal, and right sagittal) of the VCG as a percentage of the total area of each planar projection and, more specifically, of those which explore the terminal and rightward QRS forces. They are easy to obtain and estimate, and their ability to detect early right ventricle hypertrophy have been emphasized by several authors2 who have pointed out a good correlation with mean pulmonary artery pressure (MPAP) at rest.

My own results, included in my doctoral dissertation,3 show that the percentage of the QRS loop area contained in the largest VCG quadrants right posterior of the H-plane (H3), or right inferior of the F-plane (F2) or right anterior of the H-plane (H2) (provided that its value is bigger than 15 percent of the respective area), has a very good correlation (r = 0.496, p<0.01) with the values of MPAP at rest in a series of 30 chronic respiratory patients. In this series, the VCG criterion "H3/F2/H2 (the greatest of them)>15 percent of the total area" identified patients with elevation of the resting MPAP (greater than 18 mm Hg) with a sensitivity of 94.5 percent and specificity of 75 percent.

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Theophylline Administration

How Often Is Often Enough?

To the Editor:

The relationship of the effect of theophylline to serum concentration has been extensively reviewed in multiple previous publications.4,5 The manuscript by Mangura et al6 describes mean (±SEM) serum theophylline concentrations that ranged from 7.4 ± 1.2 to 15.5 ± 1.6 μg/ml from a theophylline formulation administered once daily, while concentrations following twice daily administration of another formulation ranged from 10.6 ± 1.6 to 12.7 ± 2.2 μg/ml. The authors concluded that the 110 percent fluctuation of the mean serum concentration (mean peak 2.1 times higher than mean trough) during administration of the once daily formulation was comparable to the 20 percent fluctuation (mean peak 1.2 times higher than mean trough) during the twice daily dosing and that anhydrous theophylline may be administered as a single daily dose agent. Without disputing the authors’ assertion that theophylline may be administered as a single daily (or weekly, or monthly) agent if a prescriber wishes, I can only wonder what magnitude of difference in fluctuation the authors would have considered unsupportive of this conclusion.

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