among blacks. The authors commented that blacks may be somewhat less accepting of surgical procedures, since 81 percent accepted bypass surgery, as compared with 90 percent of the whites. They also pointed, however, to the possibility of differential behavior toward blacks among some physicians and to the possibility that structural features of medical care result in blacks' having access to physicians who are less inclined to recommend surgical procedures. It is also possible that blacks receive care in treatment settings where surgery is less commonly performed.

There is obviously a need for new research to explain why blacks and poor people are not receiving services that could clearly improve their vision. Cross-sectional vision surveys such as the ones reported by Sommer and Javitt and their colleagues in this issue of the *Journal* are needed. Future surveys should also include questions about awareness of the symptoms of eye disease, questions about health beliefs concerning eye disease and its treatment, and more detailed questions about the use of medical care services and payment for them, the recognition of visual deterioration, the decision to seek eye care, and the nature of the eye care received.

The reports of Sommer et al. and Javitt et al. are important and have raised concern that there is an excess of correctable blindness and poor vision among blacks and those with low incomes in the United States. The reasons for these disparities need to be carefully examined so that remedial steps can be taken. Just as ophthalmologic signs and symptoms may be indicative of many medical diseases, shortcomings in the delivery of eye care may be indicative of dangerous disparities in other sectors of our medical care delivery system.

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CORRESPONDENCE

INDEXES PREDICTIVE OF WEANING FROM MECHANICAL VENTILATION

To the Editor: In their article (May 23 issue)* on indexes predictive of outcome in patients being weaned from mechanical ventilation, Drs. Yang and Tobin comment that traditional indexes used to predict the outcome of weaning have limited power and are considerably inferior to the indexes the authors propose. This is a somewhat inaccurate appraisal. Tidal volume, for example, is a traditional index, and as the article states, it has a predictive value very similar to that of the rapid-shallow-breathing index and a greater predictive value than the index that integrates thoracic compliance, respiratory rate, arterial oxygenation, and maximal inspiratory pressure (the CROP index).

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*Yang KL, Tobin MJ. A prospective study of indexes predicting the outcome of trials of weaning from mechanical ventilation. N Engl J Med 1991;324: 1445-50.

To the Editor: In his recent editorial,[1] Marini quite correctly mentioned the potential importance of neuromuscular causes of difficulty in weaning patients from mechanical ventilation. The magnitude of this problem is still not generally recognized, however, nor is the fact that more detailed testing of the neuromuscular respiratory system will often pinpoint the problem.

A common cause of difficulty in weaning from mechanical ventilation is critical-illness polyneuropathy.[2-4] This axonal polyneuropathy occurs in 70 percent of patients who have the syndrome of sepsis and multiple organ failure,[5] which occurs in 20 to 50 percent of patients in medical and intensive care units.[6] Phrenic-nerve conduction studies and morphologic study of the phrenic-nerve, diaphragm, and chest-wall muscles[2,5] have provided direct evidence that the neuromuscular respiratory system is involved in these patients. Critical-illness polyneuropathy may be the most common cause of intractable ventilator dependence.[7] Other potentially important neuromuscular disorders are the lingering effects of neuromuscular blocking agents,[8] particularly if there is associated renal failure and unilateral or bilateral damage to the phrenic nerve from operative or other trauma. Finally, primary disorders of muscle may also be present, such as muscle-fiber necrosis associated with sepsis[9] or the phenomenon of respiratory-muscle fatigue.[10]

What is particularly important is that sites of dysfunction in the neuromuscular respiratory system can now be specifically tested.[11] Phrenic-nerve conduction studies[12] can provide information about the presence of demyelinating or axonal neuropathy.[13] Needle electromyography of the diaphragm[14] can be performed safely and with minimal discomfort. This technique provides valuable information about abnormal spontaneous activity in the diaphragm suggesting denervation. Abnormal patterns in the firing of motor-unit potentials during attempted voluntary respiration may point to a disordered central drive. The high incidence of encephalopathy in

patients with sepsis[15] indicates that a lack of central drive may be more common than is generally recognized. We believe that studies involving repetitive phrenic-nerve stimulation are too uncomfortable, but such studies of a limb nerve will detect the presence of a neuromuscular transmission defect.

Thus, although indexes that predict the ability to wean patients from mechanical ventilation, such as those reported by Yang and Tobin,[16] are clearly of value in clinical decision making, it is our view that only when the cause of the ventilatory failure is more clearly defined will it be possible to make accurate decisions about the management of weaning from mechanical ventilation.

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To the Editor: Yang and Tobin present very interesting information that may be potentially useful for critical care physicians. However, the limitation of using a single cutoff point for diagnostic tests has been emphasized.* When a test result has a wide range of possible values, clinicians do not interpret the test as if there is a single cutoff point between normal and abnormal. For instance, in the case of the ratio of respiratory frequency (f) to tidal volume (V_T), clinicians are likely to go further than concluding that patients with values under 105 are unlikely to be weaned successfully, whereas those with values over 105 are likely to be weaned. Instead, they will be confident of weaning those with very low values, uncertain about weaning those with values in the middle range, and very skeptical about the likelihood of weaning those with very high values.

This appropriate clinical intuition can be captured formally with likelihood ratios. We have used the original data presented by Yang and Tobin to calculate likelihood ratios for f/V_T (Table 1).

The simple interpretation of these data is that patients with values under 80 are extremely likely to be weaned, those with values between 80 and 100 have an even chance, and those with values above 100 are very unlikely to be weaned. By estimating the pretest likelihood ratios and using these ratios and a simple nomogram,* clinicians can arrive at a quantitative estimate of the likelihood of successful weaning. Likelihood ratios preserve information that is discarded when a single cutoff point is chosen and the results are presented in terms of sensitivity and specificity.

*Sacket DL, Haynes RB, Guyatt GH, Tugwell P. Clinical epidemiology, a basic science for clinicians. 2nd ed. Boston: Little, Brown (in press).

Table 1. Calculation of Likelihood Ratios Corresponding to f/V_T Values.

f/V_T Value	No. of Patients Weaned	No. Not Weaned	Likelihood Ratio
<80	29	3	7.5
80–100	6	6	0.77
>100	1	14	0.06

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The above letters were referred to the authors of the article and editorial in question. Drs. Yang and Tobin offer the following reply:

To the Editor: Contrary to Dr. Travaline's statement, tidal volume has not traditionally been listed as a predictor of the outcome of weaning. In a number of recent review articles and chapters dealing with weaning, this measure has not been included among useful predictive indexes.[1-6] It was included in a list of 20 potential predictive indexes in a review by Pierson,[7] but the citations for its inclusion were previous reviews. We are not aware of any previous prospective study that has supported its usefulness. In our paper, we pointed out that tidal volume was the most accurate primary index, and we devoted a paragraph to discussing this observation.

Dr. Bolton and colleagues provide a nice review of critical-illness polyneuropathy (of which we were aware). However, they appear to have missed the purpose of our study: to investigate the predictability of weaning outcome. Is it important not only to have a test that can predict weaning failure, but also to be able to predict weaning success. By identifying the earliest time that a patient can resume and sustain spontaneous ventilation, predictive indexes help avoid unnecessary prolongation of mechanical ventilation, with its attendant risk of complications. Likewise, by determining in which patients a trial of weaning is likely to be unsuccessful, predictive indexes can prevent premature attempts at weaning and the subsequent development of severe decompensation — cardiorespiratory, psychological, or both. Such setbacks may further prolong ventilator dependence. Of course, if a weaning trial in a patient is unsuc-

Table 1. Likelihood Ratios for Outcomes of Weaning in 64 Patients, According to f/V_T Value.

f/V_T Value	Weaning Success (N = 36)		Weaning Failure (N = 28)		Likelihood Ratio
	No. of Patients	Proportion	No. of Patients	Proportion	
<80	29	0.8055	3	0.1071	7.5210
80–100	6	0.1666	6	0.2142	0.7777
>100	1	0.0277	19	0.6785	0.0408

cessful, it is important to determine the cause, such as increased respiratory load, muscle weakness, polyneuropathy, impaired cardiac function, and the like.[8] Dr. Bolton and colleagues fail to make the vital distinction between deciding on the optimal time to undertake a weaning trial and undertaking diagnostic studies to determine the cause of weaning failure.

We are in complete agreement with Drs. Jaeschke and Guyatt that statistical analysis based on a single threshold value obtained in a diagnostic test is considerably limited. For that reason, we used receiver-operating-characteristic (ROC) curve analysis, which evaluates the performance of a test over an entire spectrum of cutoff points. We also agree that the calculation of likelihood ratios is an attractive method of summarizing such data. As Drs. Jaeschke and Guyatt suggest, we feel uncertain in our own clinical practices about undertaking a weaning trial in a patient with a f/V_T slightly above or below 100 breaths per minute per liter. Unfortunately, the calculations provided in Drs. Jaeschke and Guyatt's table are in error. Our calculations of likelihood ratios appear in Table 1.

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INITIAL TREATMENT OF PATIENTS WITH EXTENSIVE TRAUMA

To the Editor: Dr. Trunkey provides a useful overview of the care of the patient with life-threatening injuries (May 2 issue).[1] We certainly agree that timely and appropriate operative intervention is key to the successful treatment of the seriously injured patient. However, the decision to perform emergency laparotomy, particularly in the context of multisystem trauma, can be vexing. We wish to underscore the pivotal role of diagnostic peritoneal lavage in such cases. Dr. Trunkey acknowledges the usefulness of diagnostic peritoneal lavage, notably in the evaluation of the patient "whose condition is unstable." Moreover, he promulgates the tenets of the Advanced Trauma Life Support course, whose manual states that "the necessity for an emergent celiotomy in the multisystem trauma patient is often difficult to establish. It must be sequenced properly among other life saving procedures. For these reasons, [diagnostic peritoneal lavage] is a critical step in the evaluation of blunt trauma."[2]

At variance with these principles is the logic followed in the case Dr. Trunkey submits for discussion. This patient has hypotension unresponsive to initial fluid management with clinical evidence of an unstable pelvic fracture as well as a closed femoral fracture. He argues that improving mentation and the absence of hemothorax on the plain chest film dictate that laparotomy be the next step in the management algorithm. Certainly other sources of hypotension deserve consideration, such as myocardial contusion, valvular injury, pericardial tamponade, or profound ethanol intoxication.[3,4] A likely contributor, if not the prevailing source in his scenario, is the retroperitoneal hemorrhage emanating from the pelvic fracture. Indeed, recognition of an unstable pelvic fracture on initial physical examination implies major injury involving both anterior and posterior columns.[5]

Hemoperitoneum can reliably be neither identified nor excluded on the basis of physical signs.[6] This is especially true in the patient with multiple fractures and the confounding influence of shock, ethanol, toxins, or closed head injury. A primary virtue of diagnostic peritoneal lavage is its exceptional sensitivity in identifying intraperitoneal hemorrhage.[7] This procedure can be performed rapidly and safely within the confines of the resuscitation suite. The initial step, aspiration of the peritoneal space, requires less than 10 minutes. For the case under consideration, the recovery of free intraperitoneal blood by appropriate technique necessitates immediate laparotomy.[8] Conversely, negative aspiration prevents unnecessary operation and allows essential diagnostic and therapeutic maneuvers to proceed without delay. These include the control of life-threatening pelvic hemorrhage by external skeletal fixation or angiographic embolization as well as investigation of the pericardial contents (by echocardiography) and descending thoracic aorta (by angiography).

Although Dr. Trunkey espouses the concept of pattern recognition for the initial management of trauma, one of his previous clinical reports disclosed a 19 percent negative rate for celiotomy when the decision to perform this procedure was made without confirmation by diagnostic peritoneal lavage.[9] It is precisely the type of patient described by Dr. Trunkey who can least afford an unneeded and time-consuming exploratory laparotomy.

Whether emergency laparotomy should be undertaken in the critically ill trauma patient depends on a simple question: Is free intraperitoneal blood present in the patient who remains in shock after the administration of crystalloid? We believe this question is answered most rapidly and accurately with diagnostic peritoneal lavage.

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To the Editor: In Dr. Trunkey's discussion of the treatment of air embolism, two additional methods should be mentioned. As an emergency measure before definitive surgical treatment, selective