visible components in the ongoing professionalization of the field." Egans fifth edition, now edited by Scanlan, Spearman, and Shelden, includes chapters by 23 contributors and 850 illustrations.

An equally valuable major text originally edited by Burton, Hodgkin, and Gee is now in its third edition. It was first published in 1977, with a second edition in 1984. The third edition contains chapters by 61 contributors. I agree with the editors' predictions that "respiratory care will become increasingly therapist driven" and that "the need for RCPs [respiratory care practitioners] in the outpatient setting will become increasingly more obvious. Comprehensively trained, flexible, and innovative RCPs will do well in the new environment."

Now, in my opinion, comes the masterpiece. David Pierson, an original product of the Colorado influence, and another friend, Bob Kacmarek, both of whom have contributed an enormous amount of art, knowledge, and science to the field, have produced a most comprehensive and beautifully illustrated volume, which will delight all who work in the field, Fundamentals of Respiratory Care. A product of one of the longest literary gestational periods I am aware of, the book is complete and current in every respect. Aided by 54 contributors, the editors have produced an integrated text. It is not intended to be either a "doctor book" or a "therapist book," to quote them, it is, "a comprehensive resource for other health professionals who participate in the assessment and management of patients with disorders affecting the respiratory system."

Although no one book can be all things to all people and answer every question, this one comes very close. Remember that new questions and their answers always provoke more questions! Accordingly, the student or practitioner of respiratory care will never rely entirely on any one text for guidance. As new developments occur and are reported in the literature, additions and revisions of any book are required. But for now, all of us who are involved in this fascinating field have a new benchmark!

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REFERENCES
1 Barach AL. Principles and practices of inhalational therapy. Philadelphia: Lippincott, 1944; chap 1
2 Haldane JS. Symptoms, causes and prevention of anoxaemia (insufficient supply of oxygen to the tissue) and the value of oxygen in its treatment. BMJ 1919; 2:65-71
3 Barach AL. The therapeutic use of oxygen. JAMA 1922; 79:693-99
4 Standards of effective administration of inhalational therapy. JAMA 1943; 121:735-59

Reliability Coefficient
Man versus Machines

I suspect that Drs Haas, Axen, Salazar, and Schicchi had some inkling of the "second" medical question that they were addressing when they wrote their article (see page 64) on the use of the entire maximum expiratory flow-volume curve in assessing the presence or absence of exercise-induced bronchospasm (EIB). In addition to a well-documented primary thesis that use of only one variable noted in the maximal flow-volume curve is insufficient to exclude the presence of EIB, these authors have stated rather directly that patients' descriptions of their complaints have more importance and accuracy diagnostically than we have generally assigned to them.

This needs to be repeated over and over, more's the pity, as our profession has become so seduced by graphs, statistical analyses, scanning electron micrographs, and the scientific method in general that the words subjective and intuitive have taken on almost sinister implications.

In brief, I wish to state that within the article by Haas and colleagues there exists a sturdy argument for assigning greater importance to patients' complaints. (Also, by the way, to expressions by patients that they are well!) By this statement I am not denigrating the scientific approach to our profession—heaven forbid! Rather, I am acknowledging the primitive state of our understanding of almost everything that we deal with and the need to keep an open mind when our patients' histories are at odds with measured physiologic, biochemical, or morphologic variables. This is a natural conclusion when we see how often the pseudoscientific dictum "If I can't measure it, it doesn't exist!" has been proved wrong. This is not meant to diminish the importance or significance of scientific investigation; rather, this is a plea for humility vis-à-vis our major limitations in basic understanding of human physiology and biochemistry. This humility imposes on us the imperative of giving weight to patients' observations and using them as important

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data when we derive diagnostic conclusions and determine subsequent therapy.

People come to doctors when they feel something is wrong. We try to explain their complaints in terms of "disease"—that is, as an abnormality in a physiologic system—but we tend to ignore the "illness"—that is, the general discomfort of the patient. It was Eric Cassell who most recently drew our attention to this point, which was nicely highlighted by the two cases cited by Haas et al in their article. Each of the patients had proven EIB after careful, detailed study, but both of them were such remarkable performers physically and their preliminary pulmonary function tests were so near normal that their basic disease was overlooked because their "illness" was assigned too little importance. To maintain our credibility not only with our patients but with ourselves and our self-image as healers, we must be very careful about undervaluing the complaints made by our patients.

In summary, I wish to be clear: In general, patients' statements concerning their state of health are more accurate than the machines we use to measure the patients' physiologic and biochemical functions, and the data generated by a good history must be assigned a correspondingly high value. We must intensify our research into the physiologic and biochemical bases for patients' complaints so that we have more reliable data to explain, but not necessarily to contradict, what our patients tell us. Finally, we must develop scientific methods for better quantifying and discriminating the importance of what our patients have to say. I believe that this effort takes on added importance as our scientific capability becomes more and more sophisticated.

Drs Haas, Axen, Salazar-Schicchi, the challenges implicit and explicit in your paper leave me breathless!

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REFERENCE

1 Cassell EJ. The nature of suffering and the goals of medicine. New York: Oxford University Press, 1991; 254

The Effect of Theophylline on Sleep in Normal Subjects

Until recently, the effect of bronchodilators on sleep architecture has been relatively unstudied. The assumption has been that medications that relieve airway obstruction during the daytime should do so during the night as long as plasma levels are maintained. However, reestablishing airflow at night while remaining asleep is quite distinct from restoring normal airway function during wakefulness. It is well known that minor changes in various respiratory stimuli, such as hypercapnia, hypoxia, airway occlusion, and pulmonary irritation, produce arousals and disruption of the sleep pattern. Establishing, therefore, that a patient sleeps normally provides good indirect evidence that overall airway function is being adequately maintained.

In general, sleep architecture can be viewed in terms of "quantity," as determined by the total sleep time and efficiency (actual recorded total sleep time divided by total time in bed), and "quality," as assessed by fragmentation due to small arousals lasting 10 to 20 s or shifts to lighter stages. Clinically significant disruption in either the quantity or quality of sleep results in the sensation of daytime fatigue and sleepiness. Since patients with underlying lung disease are known to already suffer from disrupted sleep, it is useful to distinguish any potential deleterious effect that a particular medication might have on overall sleep architecture apart from its beneficial effect on airway function.

In this issue of Chest (see page 193), Kaplan and coworkers report the effects of theophylline on sleep architecture in ten healthy young men in a double-blind cross-over design. They found that theophylline decreased total sleep time by approximately a half hour (decreased quantity) as well as increased the number of arousals (decreased quality) by four episodes per hour. Otherwise, sleep efficiency, the distribution of stages of sleep, and sleep onset times were unaltered. These findings are in slight contradistinction to those of another recently published study of similar design, which showed no effect of theophylline on either sleep architecture or daytime cognitive performance. Since both studies were almost identical in design, it is hard to explain the differences in total sleep time other than to note that the study by Fitzpatrick et al contained a selection bias due to symptomatic dropout and a longer period of adaptation to theophylline.

In fact, both studies are giving us similar clinical information. That is, theophylline probably does not clinically alter sleep architecture enough to cause clinical daytime dysfunction. This conclusion is based on recent sleep deprivation work demonstrating that less than 1 to 2 h of chronic reduction in total sleep time will not likely affect either symptomatic or objective measurements of daytime sleepiness. Likewise, the minimal increase in hourly arousals in the study by Kaplan et al will probably not significantly alter daytime function since we know that older normal individuals, who have demonstrated much higher arousal rates, did not show clinically significant alter-