change in minute ventilation or ventilatory pattern, is a major, significant reflex affecting the respiratory system as a whole. In the absence of this reflex, the human organism would be exposed to large changes in ventilation and, therefore, of arterial blood gas levels and acid/base balance with each change of posture.

Thus, Chhadha et al need to reconsider the probable cause for their very interesting finding of an increase in ventilation with passive tilt to the upright position, and the equally interesting finding of different responses within the smoking group.

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REFERENCES


To the Editor:

We wish to thank Dr. Burki for pointing out errors in our paper. We agree with Dr. Burki that there were no significant differences between supine values of Ti, Ti/Tot, Vt and f of non-smokers and smokers and are at a loss to explain how this error occurred. Further, the recomputation of Vt differences indicated that the probability values were < .001 for differences between non-smokers and smokers rather than p < .05 and p < .02, respectively, as we initially reported. We apologize for misquoting Dr. Burki’s paper in the Discussion section of our paper.

We disagree that Vt/Ti cannot be utilized as an indicator of respiratory drive when respiratory mechanics are altered. With moderate changes of respiratory resistance during induced bronchospasim, Vt/Ti reflects respiratory drive, whereas minute ventilation may not. Further, the difference between respiratory drive in the supine posture of the non-smokers and smokers for both Vt/Ti and Vt was present despite the lack of difference of airway resistance between the two groups, or 1.8 ± 0.4 and 1.9 ± 0.6 cmH2O/L per sec, as stated in our paper.

With regard to Dr. Burki’s contention that the human organism is perfect in maintaining ventilatory homeostasis during postural changes, we would like to review the studies upon which such an assertion is made. In reflexes affecting ventilation, we pointed out that part of the increased ventilation in the upright posture would take place because of the increase in physiologic dead space from the supine to standing posture.

Our conclusions remain the same: both mouth occlusion pressures during CO2 stimulation and passive tilting by assessment of Vt/Ti and Vt suggest that disturbances of respiratory center control are common in smokers without major obstruction of the airways.

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Intracranial Metastases in the Initial Staging of Bronchogenic Carcinoma

To the Editor:

Yellin raises several important issues in regard to our paper published in Chest (1984; 86:850-853). The first has to do with the impact CT findings might have on management. Although, of the 63 asymptomatic patients studied, only three with positive CT findings had extracranial metastases, management of such patients is generally chemotherapy treatment. The present chemotherapy regimen for non-small cell carcinoma is cisplatin and vinblastine sulphate. This combination, however, does not penetrate the central nervous system, so that this information is important in planning patient management.

While it is true that one patient was detected by EEG, unless routine EEGs were done this patient would have been missed. The estimated cost of $42,000 seems quite different from costs in New York City. At the time this study was done, a brain CT at our hospital cost $225. It has subsequently been raised to $300. This would make the total cost either $13,800 or $18,900, approximately the total cost of a ten-day stay for thoracotomy.

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To the Editor:

Mintz and associates (Chest 1984; 86:850-53) pointed out, once again, that neurologically asymptomatic patients with bronchogenic carcinoma may have brain metastases. Such a finding occurred in five of 63 asymptomatic patients. The authors confirm that in search for intracranial metastases, CT scanning is the single most effective study.

The crucial issue, alluded to only briefly, is the impact CT findings might have on management. Of the 63 asymptomatic patients studied, three with positive CT findings had extracranial metastases; the brain CT scan added nothing to making them inelligible for curative surgery. There were only two asymptomatic patients in this series whose brain metastases were the only site of extrathoracic spread, and whose CT scans were therefore crucial for management decisions; one of those was also detected by EEG.

In summary, 63 CT examinations were done to evaluate neurologically asymptomatic patients at an estimated cost of at least $42,000, in order to eliminate an unnecessary operation in one patient. Contrary to the authors, I conclude that routine CT of the brain is cost ineffective with minimal impact upon management of asymptomatic, neurologically normal patients with non-small cell lung carcinoma.

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Accuracy of 99mTc Gated Blood Pool Imaging

To the Editor:

I read with considerable interest the article by Konstantin et al (Chest 1984; 86:681-87) purporting to show the accuracy of Technetium-99m gated blood pool imaging for the measurement of right ventricular ejection fraction (RVEF). A study of similar intent was performed by our group with markedly dissimilar findings.1

We independently performed biplane right ventricular cineangiograms as well as 99mTc gated radionuclide studies in 14 patients. The radionuclide RVEFs were calculated using four different processing methods similar to those in the Konstantin study. The angiographic determinations of RVEF were done using the Gerling rule.2 Our study revealed marked variation in RVEF using different processing protocols as well as poor correlation with RVEF measured from the biplane angiograms (the r-values for the four techniques ranged from 0.23 to 0.74). We concluded that gated pool imaging of
the RV using 99mTc was too inaccurate to be used in individual subjects.

I suspect that Dr. Konstam’s excellent correlations stem from the method used in processing the data. In our study, the RVEF was calculated from the angiograms independently and blindly from the radionuclide RVEFs. In the study by Konstam et al, this does not seem to have been the case. As anyone who has used a computer light pen or joystick to draw right ventricular regions of interest can attest, it is very easy to consciously (or subconsciously) be a little generous in one frame or a little stingy in another, especially if the operator knows what the RVEF is supposed to be from having done the first pass measurement. Unless the first pass and equilibrium RVEF measurements were done independently, one must question the validity of the reported findings. Based on our study and experience, we remain skeptical about the accuracy of 99mTc gated blood pool RVEF determinations.

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REFERENCES


Assessment of Airflow in Sleep Studies by Oronasal CO₂ Detection

To the Editor:

Since apnea is defined by the absence of airflow, detection of air flow is an essential part of screening and all-night sleep studies. Adequate assessment of breathing during sleep studies requires the ability to detect airflow at both the mouth and nose, since many patients breathe alternatively through the nose and mouth in a single night’s sleep.

Nasal thermistors at the mouth and nose are commonly used to assess airflow. However, we have found it useful to use end-tidal CO₂ as a measure of airflow in certain situations; eg, when calibrated end-tidal CO₂ measurement is desirable, and for portable screening studies, in which we measure airflow and oxygen saturation only.

We have developed a simple, inexpensive oronasal cannula to monitor end-tidal CO₂ in clinical sleep studies.

We constructed the oronasal cannula from two nasal oxygen cannulae (Hudson Company, model no 1104) by cutting two small holes with a scalpel in the back side of the first cannula, just behind the soft nasal tip. From the second cannula we cut two pieces of clear plastic tubing, each about 4 cm in length. On each of the tubes, we cut a 1,5 cm bevel on one side. We inserted the two beveled pieces through the two small holes made in the first cannula, making sure that the beveled ends were face down, and that the other end of the tubing did not block the center of the cannula (Fig 1A). We placed two small drops of cyanoacrylate glue (Superglue) around the two holes to hold the tubing in place. After the glue dried (10 sec), we bent the two “fangs” between thumb and finger, to make the tubing bend down past the upper lip when the prongs are inserted in the nostrils (Fig 1B). We cut the large tubing off about an inch down past the connection to the oxygen source and used a stopcock instead to make the connection to the CO₂ analyzer.

A recording of oronasal airflow and the rib cage (RC) and abdominal (ABD) components of respiratory inductive plethysmography is shown in Figure 1C. We have found this device reliable, easy to use, and better tolerated by patients than a face mask.

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Tuberculoid Bacilli and Tuberculoidosis

To the Editor:

In a recent letter, Francis and Abrahams fired yet another salvo in a long-standing semantic war of nomenclature over those unfortunate, misbegotten mycobacteria which do not belong to the species Mycobacterium tuberculosis.1 In their advocacy for a four-tiered system ("Tubercle bacilli, tuberculoid bacilli, saprophytic mycobacteria, and leprosy"), they propose the term "tuberculoidosis" for disease attributed to the "tuberculoid bacilli." While one might quarrel with their sense of verbal aesthetics in preferring these terms over "nontuberculous mycobacterial," "mycobacteria other than tuberculosis," or "atypical mycobacteria," this would likely result in the type of struggle of style that is unlikely to be resolved. Rather, we would suggest that the derivation of their terms is fundamentally unsound and would not serve to promote clarity of communication as they proposed, but would merely beget more confusion and misunderstanding.

Tubercle, the root of tuberculosis, is derived from the word used by pathologists to denote the now familiar potato-shaped ("tuber") granulomatous organization of lymphocytes and macrophages at the site of inflammation due to mycobacteria. To the extent that all of the pathogenic mycobacteria elicit similar responses, they all produce "tuberculosis." The terms "tuberculoid" and "tuberculoidosis" do not accurately portray the situation in this regard; the lesions are not tuberculoid but are, in large measure, tubercles. History and contemporary usage generally reserves the term tuberculosis for disease due to M tuberculosis (although Dorland's Medical Dictionary uses the term for "any of the infectious diseases ... caused by species of Mycobacterium ... "). To resolve this vagueness, it is incumbent upon us to develop terms for disease due to the