In the present study, the poor correlation between EVLW and CXR may be due to factors other than just technically inadequate CXR. There is no mention of the hemodynamic characteristics of these patients. In a report by Goodwin and Pruitt of five thermally-injured patients, EVLW (as measured by the thermal dye technique) decreased slightly as cardiac index progressively increased. In this same group of patients, EVLW (as measured by the rebreathing technique) increased significantly as cardiac index increased. Fallon et al.

Clearly, the use of CXR in the bedside management of critically ill patients is not perfect. However, to minimize its usefulness based on a technique with its own inherent limitations would be unwise.

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To the Editor:

As noted in our report and discussed by Dr. Karras, several studies have sought to evaluate the portable chest radiograph as a monitor of extravascular lung water. In our study and in others, a linear correlation can be demonstrated between chest radiograph scoring and measured extravascular lung water. However, although there is statistical significance using a linear correlation, the correlation coefficient is frequently low. (Halperin et al. r = 0.5, p<0.05; Snashall r = .45, p<0.01; upper lung zones, Pistolesi and Giuntini, r = 0.51, p<0.02).

In the intensive care setting, serial portable chest radiographic examination is frequently used to assess change in lung water. Our study showed no significant correlation between serial radiography interpretation and accompanying lung water determination. That is, changes in extravascular lung water do not correlate with changes in radiograph score. Sivak et al. have also confirmed the poor accuracy of sequential radiograph for determining whether or not a change in lung water has occurred. Low correlation limits the usefulness of the radiograph as a tool for following changes in lung water.

Several reports have questioned the ability of the thermal-dye technique to accurately measure extravascular lung water in states of reduced cardiac output. Dr. Karras questions whether our data may have been influenced by low cardiac output. None of our patients had a low cardiac output during the time of the study. All were hemodynamically stable, so it is unlikely that this is a cause of the changes measured in lung water. We have recently reported that, in an animal model, the measurement of extravascular lung water correlated closely (r = 0.93) with lung water determined by gravimetric analysis after a mean cardiac output reduction to 36 percent of baseline. We do not question the value of obtaining portable chest x-ray film in the ICU. What we do question is the ability of the chest x-ray to accurately reflect changes in lung water on a day-to-day basis. We thank Dr. Karras and his colleagues for his interest in our study.

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Washington State Screening of Prison Inmates

To the Editor:

The report entitled "Tuberculoscopy Screening in Washington State Male Correctional Facilities" (Chest 1986; 89:117-20) presented a review of admission skin testing provided to inmates admitted to Washington State penal institutions prior to 1982 and still residing in the institution in June, 1983, and inmates admitted during 1982 and still residing in the institution in June, 1983. As such, the data collected were appropriately used to make the following comparisons between inmates admitted during the two different time periods: 1) the number and percentage of inmates tested upon entry; 2) the number and percentage of inmates found to be skin test reactors upon entry; 3) the number and percentage of reactors who were candidates for therapy or prophylaxis; and 4) the extent to which followup activities were needed for inmates in the two groups as of the time of the review.

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The study design, however, does not support the conclusion of the authors that "there was no evidence of tuberculosis transmission within the institutions." Their record review of 4,744 inmates showed that only 347 (7.3 percent) had documented repeat skin tests during incarceration. The extent to which this small percentage of inmates, some of whom had been released from prison between skin tests, represents the entire inmate population is not known. Because of the absence of data on serial tuberculin testing on 92.7 percent of the prison population, the authors' conclusion cannot be supported that inmates who stayed in the prison system did not experience skin test conversions. Of the 345 inmates with repeat skin tests, there were three identified converters, all of whom were identified after a break in incarceration and reimprisonment. Similarly, in the absence of serial testing of inmates it cannot be concluded, as the authors claim, that the readmitted inmates were infected while outside the prison.

I applaud the efforts of the authors to measure the cost and some of the benefits derived from a large admission screening program. However, what is also needed from future evaluations is a measure of the extent to which screening and case-finding activities are protecting inmates from tuberculosis infection and disease. Results of retests on a representative sample of inmates for whom the number of years spent in prison is known would partially address this need. Incarceration can provide an ideal setting for transmission of tuberculosis, and the lack of reported cases in a prison system does not preclude the existence of unrecognized and unreported disease.\(^2\) Infected prison inmates may be at higher risk of disease than nonincarcerated individuals because reported TB risk factors such as IV drug abuse, alcoholism, and HTLV-III/LAV infection have been shown in some studies to be disproportionately represented in prison populations.\(^3\)

The prison population consists mainly of young adults, most of whom will return to the community. Inmates are a relatively accessible and potentially high-risk group. We need to assume that all feasible approaches are being taken to prevent this group from becoming (or perhaps continuing to be) a focus for unnecessary future tuberculosis morbidity and mortality. Thorough evaluation of existing programs will determine which approaches can most effectively achieve this purpose.

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Differences in EPAP and CPAP

To the Editor:

We read with great interest the recent study by Layon et al. (Chest 1986; 89: 517-21) in which the authors observed no difference between the effects of CPAP and EPAP in anesthetized dogs. As discussed by the authors, these findings are not in agreement with our findings that FRC with CPAP was higher than that with EPAP at both 5 and 10 cm H\(_2\)O pressure in human subjects. Layon et al. argue that we did not directly measure end-expiratory pressure, relying instead on the expiratory pressure valve to quantify expiratory pressure. If this were true, expiratory pressure during EPAP would have been spuriously high, possibly accounting for the differences in CPAP and EPAP we observed. However, as stated clearly in the methods section of our study, we measured airway pressures directly with appropriately calibrated pressure transducers. As shown in our study, end expiratory pressures between CPAP and EPAP were equivalent. Therefore, we do not think technical problems in our study account for the differences between the results of Layon et al. and our findings. Although the explanation for the discrepancy between the findings of Layon et al., Gherini et al., and our results is not completely clear, there are significant methodologic differences among the studies. We proposed in our study that alterations in chest wall elastic recoil between EPAP and CPAP may have been responsible for differences between EPAP and CPAP. These alterations may not be manifested in anesthetized experimental animals. Therefore, while we agree that possible differences between EPAP and CPAP may be related to effects of conscious alterations in breathing, we cannot accept the hypothesis that they are related to technical problems in our study. Furthermore, since positive end-expiratory pressure during spontaneous breathing is usually applied in unanesthetized patients, the probability that CPAP more effectively increases FRC and PaO\(_2\) and results in less work of breathing than EPAP should be considered in choosing between these two modes of applying end-expiratory pressure.

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To the Editor:

We thank Quan et al. for clarifying the methods by which they measured airway pressure in their study comparing CPAP and EPAP.\(^1\) We do appear to have misinterpreted their "Method" section and, for this, we are pleased to be corrected.

The disparate findings in our study (Chest 1986; 89:517-31) and those of Schlobohm and colleagues\(^2\) are due to several factors. First of all, there were differences in the subjects studied: Schlobohm and colleagues studied 18 intubated, spontaneously breathing, supine patients in the intensive care unit. Twelve of 18 were comatose and six of 18 had recently been weaned from mechanical ventilation. Three of Schlobohm's patients had a history of chronic obstructive pulmonary disease.\(^1\) We studied ten healthy mongrel dogs who were anesthetized and spontaneously breathing. Schlobohm and colleagues found a statistically significant difference in the functional residual capacity (FRC) at 5 and 10 ml of water CPAP and EPAP. Yet