appropriate facilities and enough hospital beds are available, together with experienced spinal surgeons and good postoperative nursing, the modified Hong Kong operation as performed by its originators, has definite advantages. Compared with the other methods of treatment investigated by the Working Party, it has produced substantially earlier bony fusion, vertebral reconstruction and no increase in kyphosis. It makes the most demands on the surgical staff and on hospital beds. Where adequate facilities are lacking, reliance should be placed on ambulant chemotherapy alone because the results of the latter are also very good. 9

MRC patients were randomly allocated to treatment groups and only one of 150 patients in the Fourth Report had neurologic abnormality. 3 Three of our four surgical patients with thoracic involvement had paraparesis. They had operation only for severe findings or progression of findings while on chemotherapy. In the early MRC studies, patients who could not walk due to neurologic deficit were excluded. Thus several of our patients would not have been included in the study. In the Fifth Report of the MRC, seven patients originally assigned to chemotherapy required later operation for paraparesis. 6

The modified Hong Kong operation (similar to that used in our cases) differs from surgical debridement as performed in the early reports cited by Professor Enarson in that autogenous bone grafts are inserted in place of the radically debrided vertebral bodies to promote bony fusion. The Fifth Report of the MRC states that "it has been shown that at three years the radical operation with bone grafting produces . . . healing in a substantially greater proportion of patients than does a simple debridement operation." 3

We were unable to reference the many detailed studies of the MRC due to space limitations but we did include a review by Dr. D. L. Griffith, Chairman of the MRC Working Party (Secretary until 1974). He states regarding paraplegia that "it is arguable . . . that all such cases are best treated by operative decompression, for not only are the recovery rates much higher after such surgery but the paralysis disappears so rapidly after adequate decompression that it appears unsound to allow a patient to lie for months awaiting spontaneous cure when operation can produce recovery in a matter of days." 9

I believe our statement that "early operative intervention must be considered (we are not advocating operation for all) in patients with Pott's disease involving the thoracic spine" (where neurologic impairment is a significant risk) is well supported not only by our own experience and that of others (ie, our references 8 and 18), but also by the later studies of the MRC Working Party on Tuberculosis of the Spine.

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Continuous Monitoring of O₂ Saturation

To the Editor:

"I am afraid of the dark and suspicious of the light"—Woody Allen

The article entitled "Continuous Monitoring of Mixed Venous Oxygen Saturation in Patients with Acute Myocardial Infarction" by Kyff et al (Chest 1986; 95:607-11) stresses the importance of continuous monitoring of SvO₂ in patients with complicated myocardial infarction. The authors have demonstrated clearly in this patient population that there is little correlation between intermittent measurements of cardiac output and continuously-monitored SvO₂. While this has been noted in numerous studies in other patient populations, it has not been reported in patients with myocardial infarction. As the authors have pointed out in the discussion, it had been assumed that patients with myocardial infarction maintain a relatively constant oxygen consumption. Clearly, however, this is not the case and, as in other critically ill patients, oxygen consumption changes significantly, often in a manner that is undetectable by clinical assessment.

The authors' concluding paragraphs are of concern. While it is clear that continuous measurement of SvO₂ is of limited value in predicting outcome, its use in evaluation of the patient's response to therapy and as an early warning of hemodynamic deterioration has not been assessed by this study. Furthermore, while changes in SvO₂ were neither sensitive nor specific for changes in cardiac index, and SvO₂ values are meaningful only in the context of other hemodynamic and clinical parameters, the conclusion that use of the monitoring technique is not warranted is no more substantiated than the conclusion that measurement of cardiac output every 12 h is not warranted.

Mixed venous oximetry provides a sensitive and continuous monitor of change in the balance between oxygen supply and demand in critically ill patients. It is sensitive but nonspecific and should not be expected to correlate with changes in cardiac index. The real utility of SvO₂ monitoring is 1) to give us additional information regarding patient stability in patients in whom further measurements are not needed and 2) to provide an early warning as to oxygen transport balance changes in patients in whom additional information is needed. It therefore reduces the number of measurements required in stable patients and improves our ability to appropriately time measurements in unstable patients.

To paraphrase Woody Allen, we should continue to be afraid of the darkness of every-12-h measurements of cardiac index, but we should not be suspicious of the light of the end of oximetry catheter in providing insights into the stability of our patients.

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To the Editor:

We wish to thank Dr. Nelson for his comments. We are aware of Dr. Nelson's work, which we have referenced in another publication. 14 We support his contention that SvO₂ is a sensitive but nonspecific monitor of oxygen balance and indeed may serve as an
early warning of hemodynamic instability. However, it is this very lack of specificity that results in a greater number of hemodynamic measurements. We documented this fact in a recently completed study comparing similar groups of patients undergoing coronary artery bypass surgery before and after the availability and use of continuous \( \text{SV} \text{O}_2 \) monitoring pulmonary artery catheters.\(^2\) Of the parameters compared, the only pertinent data reaching statistical significance were that there were a greater number of cardiac outputs measured in the group with continuous \( \text{SV} \text{O}_2 \) catheters (mean \( \pm 3.6 \) vs \( 13.9 \pm 10, p = 0.02 \).\(^3\) Since Dr. Nelson likens 12 h measurements of cardiac output to darkness, the only way to reduce cardiac output and blood gas measurements would, in fact, to be in the habit of doing them more frequently! In that case, one could readily demonstrate in stable patients that more frequent assessment of hemodynamics and oxygen transport are unnecessary. We are aware of 4-h, or even hourly, measurements in some institutions. We maintain that except in circumstances where rapid volume loading, vasoactive agents or PEEP therapy are being titrated, such measurements are unnecessary with or without continuous \( \text{SV} \text{O}_2 \) monitoring.

Our study of acute myocardial infarction patients also demonstrated that changes in \( \text{SV} \text{O}_2 \) did not correspond to similar directional changes in hemodynamic measurements. This makes changes in oxygen balance very difficult to interpret and perhaps not an appropriate single parameter upon which to base therapeutic intervention. We have published a study which questions the usefulness of \( \text{V} \text{O}_2 \) measurement after acute myocardial infarction.\(^4\)

Neither we nor Jastremski have been able to document the cost effectiveness of a more expensive form of monitoring.\(^5\) Thus, the problem with the light at the end of the oximetry catheter is that—by itself and without thoughtful interpretation such as Dr. Nelson has applied to our paper—it is not very illuminating. His comments regarding our patient with cardiac arrest are physiologically correct, but require a level of sophistication not usually available at the bedside. In a way, we may consider it a Woody Allen type of joke that a gadget continues to display numbers which are normal at the time of death.

**Jeffrey V. Kuff, D.O., F.C.C.P., University of Michigan, Ann Arbor, MI; and Vnood K. Puri, M.D., F.C.C.P., Mount Carmel Mercy Hospital, Detroit**

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**Pulmonary Artery Catheterization**

To the Editor:

Dr. Spodick's editorial ("Flow-directed Pulmonary Artery Catheterization: Moratorium vs Clinical Trial." Chest 1989; 95:489-90) proposed a prospective randomized trial of the utility of Swan-Ganz catheters. I presume he believes that current data are inconclusive. However, the proposed design of the trial excludes all "emergent" cases where panel of three "experts" agree that bedside pulmonary artery catheterization is appropriate. Current controversy revolves around whether any "experts" can properly assess the indications for pulmonary artery catheterization. A definitive study designed to settle this issue would avoid the exceptions Dr. Spodick proposes.

Daniel M. Raybin, M.D., F.C.C.P., St. Mary's Hospital and Medical Center, San Francisco

To the Editor:

I am not sure that Dr. Raybin and I understand each other. He is, of course, correct that whether any "experts" can properly assess indications for Swan-Ganz catheterization remains controversial. But that is the reason for the proposed clinical trial. I presume that by "a definitive study" Dr. Raybin means allocation of all cases—emergent and nonemergent. I had considered that, but how can one mobilize the entire expert panel that I proposed (and they should always be the same troika) for night and day emergencies, where time must not be wasted?

Dr. Raybin writes: "I presume he believes that current data are inconclusive." Of course! Otherwise why propose a study? The next sentence escapes me.

The necessarily simplistic trial that I believe is clearly outlined in my article\(^1\) is an initial approach that considers the practicalities of a medical cosmos in which flow-directed PA catheterization is well established and, for many, an article of faith. The trial as outlined should test, at a minimum, the impact of the unmeasured physiologic handicap of the presence \( \text{p} \text{O}_{2} \) of intravascular instrumentation.\(^2\) It should also refine current indications\(^3\) for flow-directed PA catheterization, a procedure that is and probably will continue to be valuable where demonstrably indicated.

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**Wild Boars and Pulmonary Paragonimiasis**

To the Editor:

I read with interest the article by Sharma (Chest 1989; 95:670-72). Although pulmonary paragonimiasis is well known to be caused by eating raw or undercooked freshwater crabs or crayfish, it has

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