mycobacteria other than tuberculosis which accurately portray the pathogenic process and, unto themselves, stand clearly.

While it is appealing to strive for concision, it ought not be done by sacrificing clarity. Rather than introduce these new terms which through careless pronounciation or unschooled perception will predictably be heard as "tuberculosis," we would suggest adherence to the present system delineated in the American Thoracic Society classification system. While it takes a bit longer to say or write "pulmonary disease due to Mycobacterium kansasii," or "cervical lymphadenitis due to Mycobacterium scrofulaceum," or "dissiminated mycobacteriosis due to Mycobacterium intracellulare," such expressions unequivocally describe the disease state. With regard to a generic designation for those mycobacteria, the terms tuberculous or saprophytic mycobacteria are neither informative nor accurate. Recent case reports decisively document the pathogenetic roles of many of the species which previously had been lumped as saprophytic. Thus, again foregoing brevity for clarity, we would propose adopting "Mycobacteria other than Tuberculosis" (MOTT).

Try it! You MOTT like it!

Michael D. Iseman, M.D., F.C.C.P.,
Associate Professor of Medicine; and
John A. Sbarbaro, M.D., FCCP, Professor of Medicine,
University of Colorado School of Medicine, Denver

To the Editor:

The concluding words of Iseman and Sbarbaro are rather flippant, and the letter appears to contain inconsistencies: in the first part of the last paragraph they seem to advocate the exclusive use of the species names, and there are obvious times when these should be used, but in the last part of the paragraph they advocate the use of MOTT as a group name. Fortunately, it has never attracted wide support—in addition to the tuberculous bacilli as defined by us, MOTT must include the Leprosy and Jones bacilli and all the saprophytic mycobacteria; indeed, all mycobacteria other than M tuberculosis.

They overlook the fact that our definition is not based just on pathology but states that, unlike tubercle bacilli, the tuberculous bacilli do not have the power of epidemic spread in man. Even on pathological grounds, they seem to agree with us: they write that the lesions (produced by tuberculous bacilli) are, in large measure, tubercles, and if they consult the dictionaries cited by us, they will find that—oid implies resemblance or likeness to something, whereas—osis implies the thing itself. They mention Dorland's excellent dictionary a little disparagingly for (so rightly) using the term tuberculous for any of the infectious diseases caused by species of mycobacteria—that is, M tuberculosis, M bovis and M avium. As mentioned above, non-infectivity for man is part of our definition of the tuberculous bacilli.

In the classic days of the study of tuberculosis around 1910, when it was still a major scourge of man and his domestic animals, especially in Europe and North America, the British Royal Commission on Tuberculosis and most other writers simply referred to the "human, bovine and avian types of tubercle bacilli" because they all produced epidemic infectious tuberculosis in the species concerned. Arnold Rich in his classic work

There are no perfect names but, after much consideration, we have suggested tuberculoid bacilli and tuberculoidosis as the most meaningful and useful. Therefore, rather than somewhat factious criticism, we trust that the combination tuberculoid bacilli and tuberculoidosis will receive careful consideration by the many people who desire simple and meaningful group names for this clinico-pathological group of mycobacteria and the illness these bacilli cause.

Try it! After you have said it enough times you WILL like it.

John Francis, C.B.E., D.Sc.
Emeritus Professor, Department of Veterinary Pathology and Public Health, University of Queensland, Brisbane, Australia; and
E. W. Abrahams, M.D., Formerly Director of Tuberculosis, Department of Health, Queensland, Australia.

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Anaerobic Metabolism or the Increased Work of Breathing?

To the Editor:

I was most interested in the data of Estenne et al (Chest 1984; 86:936-938) demonstrating an increased carbon dioxide production in a patient with profound pulmonary edema. The authors consider an increased oxygen utilization of respiratory muscles to explain their finding. I would propose a possible alternate explanation which would be of both clinical and physiologic interest. Conceivably, during the low output state, there is poor perfusion to a limb or visceral organ such that anaerobic metabolism ensues. With improved contractility, aerobic metabolism would resume. To this end, serial measurements of electrolytes, lactate, or oxygen consumed
would be of interest.

Marc H. Lavietes, M.D.
Associate Professor of Medicine
University of Medicine and Dentistry
of New Jersey, Newark

To the Editor:

We agree with the suggestion of Dr. Lavietes that anaerobic metabolism due to poor perfusion of a limb or visceral organ may contribute to an increased carbon dioxide production in a patient with profound cardiac pulmonary edema. Serial measurements of blood lactate obtained in our patient, however, argue against such a mechanism. All lactate values were indeed within the normal range (<2 mmol/l), and failed to show any concomitant change with the increase in carbon dioxide production.

It appears likely, therefore, that the additional carbon dioxide production in our patient originated, at least in most part, in an increased work of breathing.

Marc Estenne, M.D.; and J. C. Yernault, M.D., F.C.C.P.,
Chest Service, Erasme University Hospital
Brussels, Belgium

Radiotherapy Alone for “Nonoperability”? 

To the Editor:

The article by Cooper et al (Chest 1985; 87:289-92) is of great potential value to those of us interested in preoperative pulmonary function assessment. In their study, 46 of 72 (64 percent) of the patients were deemed to be anatomically resectable but physiologically inoperable due to their pulmonary status. It would be interesting to know some quantification of these patients' pulmonary function using FEV1, FEV1 as a percent of predicted, or some other comparable physiologic assessment. These quantitative data could then be compared to published survival statistics of patients with chronic obstructive pulmonary disease alone.1 Also, it would be helpful to know if any further assessment of physiologic operability was performed, such as quantitative lung scanning or exercise testing.2 Previously, those patients with severe dysfunction (FEV1 <2L) as assessed by quantitative lung scan have experienced reasonable survival with resection up to and including pneumonectomy.3 As admitted by the authors, their findings with relatively low dose radiotherapy (<5000 rads) do not settle the previous debate concerning the efficacy of radiotherapy (>5000 rads) as an alternative to resection.4

Gerald N. Olsen, M.D., F.C.C.P.
University of South Carolina
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REFERENCES


To the Editor:

In the March 1985 issue of Chest (1985; 87:289-90), an article appeared by Dr. Joel D. Cooper et al entitled, “Radiotherapy alone for patients with operable carcinoma of the lung.” On page 290, there were two figures, one entitled “Survival with thoracotomy for lung cancer” and the other entitled “Survival with radiotherapy for operable lung cancer.” I found the figure for survival with thoracotomy for lung cancer particularly confusing. Obviously, the legends for these two figures have been reversed. But more importantly, the numbers in parenthesis in this figure which I assume represent the total number of survivors yield a five year survival of only 14 percent (17 divided by 123) yet all throughout the text the authors refer to a 46 percent five year cumulative survival. I would appreciate some clarification as to this discrepancy. Obviously, this point is a particularly important one to the argument put forth by Dr. Cooper and his colleagues.

Ralph M. Nietzeba, M.D., F.C.C.P.
Humana Hospital,
Sunrise, Las Vegas

To the Editor:

Dr. Olsen correctly points out that the information presented in the paper cannot be utilized to make a meaningful comparison of surgery versus radiotherapy for identical groups of patients. This was not the point of our article, but rather, we tried to indicate what the results have been when radiotherapy has been utilized as the primary treatment for a group of well-staged patients with operable carcinoma, deemed to be at high risk for surgical resection for other reasons. The paper is really a response to an article published several years ago in the New England Journal of Medicine entitled, "The fallacy of the five year cure for carcinoma of the lung". That article suggested that, for elderly patients, radiotherapy may provide better results than surgery and utilized what we considered inappropriate data to justify this conclusion. Our publication is meant to do nothing more than to indicate the results of radiotherapy in this very special group of patients. We attempted to make this point clear, and to indicate that our manuscript was not intended to compare surgery versus radiotherapy in general. Nonetheless, it does present documentation of the results of radiotherapy in a well-staged group of patients whose tumor was operable by all of the standards we normally employ prior to undertaking thoracotomy.

Dr. Olsen's letter does point out one fault in our manuscript; namely, the title, which may be misleading. Perhaps a better title for the paper would have been "Radiotherapy alone for high risk patients with operable carcinoma of the lung".

Unfortunately Dr. Nietzeba is correct that the legends have been mixed up between figures, thus the title of the one "Survival with Thoracotomy for Lung Cancer" has the legend for the opposite figure and vice versa. Fortunately, because each figure is titled, it should be obvious to the reader that there has been a mix-up.

Regarding the numbers in parenthesis, this is standard method of displaying a cumulative survival using the Life Table Method. The numbers in parenthesis show the number of individuals at risk at each of the periods. In using a Life Table analysis, the 5 year survival is calculated before all patients have passed the 5 year milestone, which is the virtue of using this method. The numbers in parenthesis show the number of individuals entering each year of the analysis. Thus at the end of 5 years there were indeed only 17 patients for analysis, whereas at 2 years there were 55 patients available for analysis etc. The calculated 5 year cumulative survival is indeed 46% as the graph indicates.

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