7 Giusti C, Gakis C. Temperature conversion factor, activation energy, relative substrate specificity and optimum pH of adenosine deaminase from human serum and tissue. Enzyme 1971; 12:417-25

To the Editor:

We were very interested in the communication from Gakis et al. This group has made a decisive contribution to the understanding of many problems related to ADA activity and its correlation with some cells involved in immunologic phenomena. We are very grateful for their comments on our article.

It was not our aim to study the contribution of the values of ADA activity or its enzymes separately in the diagnosis of tuberculous or neoplastic pleural effusions. On the contrary, in our work we considered two groups of patients in whom these clinical situations are definitively diagnosed on the basis of clinical, bacteriologic, cytologic, and histologic elements, as we described in our Materials and Methods section. Our purpose was to investigate only an eventual correlation between ADA activity and lymphocytic populations in tuberculous and neoplastic pleural effusions.

The crucial importance of T-cell populations in the pathogenesis of tuberculous processes after stimulation by the infected macrophage is well known. So, we can assume that, notwithstanding the results of Gakis et al, the proved concentrations of ADA T-lymphocytes, associated with the great activity of these cells in tuberculosis, may contribute to the correct evaluation of our results.

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References


Advertising and Clinical Investigations

To the Editor:

The exchange between Drs Ciaglia, Levi, and Ryan and Dr Petty that recently appeared in Chest (1990; 96:1309-10) touches on some potentially important issues. I recognize that Dr Ciaglia and his colleagues were appropriately concerned about the advertisement covering Dr Petty's study. However, I think that their concerns are misplaced with regard to that study, since it is commendable that practicing physicians would be organized to do an appropriately designed clinical trial. Perhaps this should be more widespread. Indeed, it is even acceptable to do "n of 1" trials for controlled comparisons in individual physicians' offices, as well as in hospitals. (Dr Petty's, of course, was much more than an "n of 1" approach.)

Dr Ciaglia and his colleagues do have a valid objection to the pharmaceutical company's usage of the expression "major multicenter study." Although Dr Petty defined "center" in a fairly narrow manner, which would be literally correct, there is no question that the pharmaceutical company meant to suggest multi-medical center, to imply that these were institutions rather than many individual offices. The criticism, I think, should be entirely directed to the pharmaceutical company.

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Errata

In the article "Bronchoalveolar Lavage Cell Data in 19 Patients with Drug-associated Pneumonitis (except Amiodarone)" (Chest 1990; 99:98-104), the author in reference 13 should have been shown as "Ahmad S," rather than "Saed A."

The editors of Chest wish to thank David Lyons, M.D., for pointing out that in the article "Rounded Atelectasis Complicated by Obstructive Pneumonia and Pulmonary Arterial Thrombosis" (Chest 1990; 96:1283-85), rounded atelectasis was incorrectly referred to as "Blesovsky's syndrome," rather than "Blesovsky's syndrome," in the right-hand column of page 1283. The name Blesovsky was also misspelled in references 1 and 3.