To the Editor:

Drs. Bovornkitti and Matangkasombut have raised some questions regarding one or two features of the experimental protocol and the interpretation of results which we reported in *Chest*.

We generally agree with these investigators, but wish to point out that the study being of a clinical nature, was designed to identify some of the problems encountered by the physician using the PPD skin test reaction as an adjunct for the diagnosis of tuberculosis. We take this opportunity to clarify the experimental design and comment on those features of the results, and their interpretation, that have been questioned.

The accepted use of PPD as a diagnostic aid for tuberculosis has resulted in the commercial distribution of standardized preparations. Generally, these clinical reagents are prepared in large quantity and the 5TU dose standardized against PPD-S. The 250TU dose is made from dilutions of the stock material and represent calculated potencies determined from the standardized 5TU preparation. The preparations we used (Connaught Laboratories) were further standardized in well-controlled studies using human subjects.1,2 Although we used a 5TU dose as an intermediate strength, we have not referred to the 250TU as second strength and furthermore feel that the practical usefulness of bioequivalent PPD preparations eliminates the need to quantify, in mg, the concentrations of PPD.

Both the American Thoracic Society4 and American Lung Association4 have indicated that a positive skin test to 5TU PPD given intracutaneously is represented by induration equal to or greater than 10 mm. Both published guidelines also suggest that induration from 5-9 mm should be considered doubtful. Although we chose to call a skin test reaction negative when induration was less than 10 mm, we felt it necessary, for completeness, to identify the number of doubtful reactors. Furthermore, when assessing reactivity to 250TU in patients not responding to 5TU, any response suggests that the patient is not generally anergic, but his responsiveness to PPD remains "questionable" since a minimal reaction could represent sensitivity to cross reacting contaminating antigens.

Lymphocytes for in vitro studies were obtained from all subjects prior to skin testing. Repeat skin tests were usually done within 10-15 days. This was the case for both the 5TU and 250TU tests done on negative responders. We did not note any "booster" effect following the second STU tests. It is possible, however, that some of those patients responding to 250TU, but not 5TU might be responding to a prior but heretofore latent sensitivity to PPD. Thus, any conversion following the 250TU dose could represent a booster phenomenon. Other possibilities are discussed.

Differences observed for total serum protein levels could very well reflect a difference in disease state, a variable which was not considered in our study. Other differences in serum protein values, obtained when the positive and negative PPD responders were compared, demonstrated statistical significance, but it is doubtful that these differences are clinically significant and as such have little, if any, practical value. Nevertheless, a more complete analysis taking into account the disease state, age, malnutrition, alcoholism, coexisting diseases and any other variable known to influence serum protein levels should probably be pursued in order to resolve the possibility that some serum protein values might correlate with PTD skin test reactivity in patients with pulmonary tuberculosis.

We agree with Drs. Bovornkitti and Matangkasombut when they suggest that suppressor cells could be influential in abrogating cellular responsiveness. We wish, however, to point out once again that soluble serum factors, inhibiting both in vitro and in vivo responsiveness, have been described. In our discussion, we identify several known inhibitors, both cellular and humoral, present in the systemic circulation of tuberculosis patients.

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 references

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4 Diagnostic standards and classification of tuberculosis and other mycobacterial diseases. New York: American Lung
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Severity of Aortic Stenosis

To the Editor:

We would like to thank Dr. Spodick for his remarks and comments on our paper on the severity of aortic stenosis. (See page 122, January, 1981 Chest).

We agree completely with his comments on our use of the Bazett formula. Our goal was really not to establish rate-related normal values for T time, U time, etc, but rather to evaluate them by one recommended technique. For an extremely short interval such as the T time, different methods of rate correction are unlikely to yield a practical difference in evaluating disease states.

We were very surprised that our Q-MP data correlated better than other measurements (although still poorly) with the severity of aortic stenosis. Noninvasive records were indeed read without definitive knowledge of catheterization results. We were interested in investigating this because Bonner et al had reported these results. Before commencing our study, we felt we would be unlikely to reproduce these data because of the variability that is commonly encountered in phonocardiograms. Hence, we also were surprised by our findings.

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References