lymphocyte reactivity is really greatly different, so should be their IgE levels, according to the proposed hypotheses ([1]).

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Reply to Drs. Lynch and Lopez's Letter to the Editor

To the Editor:

We were very interested to learn that the study of Dr. Lynch and co-workers [IJL **51** (1983) 169–173] did not show any significant elevation of IgE serum levels in Venezuelan leprosy patients. In fact, in our previous communication ([4]), we pointed out that reports on IgE in leprosy are often discordant ([2, 3, 5, 6, 7]). Differences in the immunological responsiveness and the genetic background of different populations studied as well as an enhancing effect of concomitant parasitic infections might account for this.

The two main criticisms of Dr. Lynch and Dr. Lopez to previous studies on IgE in leprosy—including one of ours—are very appropriate. However, healthy native subjects living in the same area of the leprosy patients and matched for ethnic and socioeconomic status were used as controls in our study.

Concerning statistical analysis, we usually transform serum IgE levels into logarithmic values in order to normalize the frequently occurring bimodal distribution.

This was done in our study to compare IgE values of both healthy natives and healthy Europeans to leprosy patients. On the other hand, this was not necessary in comparing LL and TT forms of leprosy patients, since when leprosy patients were considered as a group the distribution of IgE values was not bimodal. Parametric tests were therefore used on absolute values.

We do believe that apart from this specific case, Dr. Lynch and Dr. Lopez—as we also did on another occasion ([1])—outline the very relevant role of proper controls and of adequate statistical analysis in order to obtain reliable results in IgE studies.

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