Reply to Drs. Nelson and Schauf's Letter to the Editor

TO THE EDITOR:

We appreciate the critical comments of Drs. Nelson and Schauf and are in general agreement with the difficulties of selecting the right type of control for comparing the risks of acquiring leprosy. Both the conventional measures of prevalence and incidence suffer from several limitations in the case, and due caution is required in their interpretation. Drs. Nelson and Schauf have used the data in Table 7 to compute yet another index, the relative risk or odds ratio, and point out that those in the second five years of employment have a significantly greater risk of acquiring leprosy (13/1000 vs 4/1000). However, as can be seen from the same table, these risks did not increase further in those beyond ten years of service, where the rate is only 7/1000. Thus the relationship between development of leprosy and duration of service is not clearly established. The point we wished to emphasize in this paper was that the attack rate in the institutionalized population of staff and students was quite low. We agree that any increased risk, even if present, is quite small, and are happy with the recommendations made by Drs. Nelson and Schauf.

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IgE in Leprosy

TO THE EDITOR:

In a recent communication to the JOURNAL ([2]), Nuti, *et al.* reported the existence of elevated IgE levels in Somalian leprosy patients.

On the basis of these results, the authors endorse a previous hypothesis ([3]) of hyperproduction of IgE due to deficient T lymphocyte control activity in leprosy (particularly LL) patients.

We would, however, like to remark that we have found no significant elevation of serum IgE levels in Venezuelan leprosy patients ([1]), and have emphasized the importance of both the appropriate selection of control groups (due to the augmenting effect of intestinal parasitosis on IgE synthesis) and the statistical methods employed (because of the non-normal distribution of IgE levels). Nuti, *et al.* have included the proper controls in their study (individuals of the same socio-economic level as the leprosy patients), but the statistical analysis applied should be clarified. Considering the fact that the authors do not specify the form in which their results are presented and compared, the data in the table are probably arithmetic means and standard deviations.

Since the most common statistical treatment of such parameters is Student's *t* test, we presume that this was applied; indeed the probability values reported are consistent with this assumption. However, the non-normal distribution of IgE levels (demonstrated by calculation of coefficients of the 3rd and 4th moments about the mean; skewness and Kurtosis) precludes the use of these statistical methods.

It is now widely accepted that geometric means are the most appropriate form of presentation, and that the statistical tests applied should be "non-parametric" (e.g., Wilcoxon-Mann and Whitney tests). A much-used alternative statistical treatment is normalization by logarithmic transformation and subsequent use of conventional analysis. We would be particularly interested in the recalculated significance of the relatively modest (27%) difference between LL and TT patients, because if their overall T

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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