a dose-dependent pattern ([1]). Hence, altered susceptibility of DDT-exposed mice to *M. leprae* infection might be attributed to DDT-induced immune dysfunction. Although it appears that the immunosuppressive effects of DDT could be one of the factors for increased growth of *M. leprae* in normal mice, it is not known whether the pesticide itself could also enhance the multiplication of the bacilli. More light can be thrown in this direction by *in vitro* studies of the effect of DDT on the growth of *M. leprae*-related mycobacteria, peritoneal macrophage activity, and the release of interleukin-2 by T cells.

Testing of the effects of DDT on host resistance is important in relation to the health aspects of pesticides, particularly due to the widespread use of DDT and its persistence in the environment. It is apparent that a more complete understanding of the toxicity of DDT is necessary in order to determine the human health hazards and to establish guidelines for acceptable DDT residues in the environment. Adverse effects of DDT on immune function could place the host in a more vulnerable position regarding various pathogens. There is a need for further detailed studies on the dose-time relationship of DDT exposure and the growth of *M. leprae* in mice since repeated exposure and contamination are possible in nature.

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Regarding Ebenezer, *et al.*'s MB Nerve Histology in Clinically Diagnosed BT Leprosy Patients

To the Editor:

I am writing concerning the paper by Ebenezer, *et al.* entitled "Multibacillary nerve histology in clinically diagnosed borderline tuberculoid leprosy patients," which appeared in Volume 64, No. 3 of the International Journal of Leprosy. The findings are interesting and may be useful; however, I believe the conclusions may not be warranted and that some of their patients may not be paucibacillary. The use of one World Health Organization (WHO) term (multibacillary) and one Ridley-Jopling term (borderline tuberculoid) in the title demonstrates the source of the confusion.

The authors state: ". . . patients clinically classified as borderline tuberculoid (BT) and, therefore, belonging to the PB group."

There is not a direct correlation between the Ridley-Jopling classification and the WHO classification. Thus, it is erroneous to state that all BT patients should be classified as PB. This would depend upon the findings in the specific patient. In addition, they state that slit-skin smears were done ". . . from six routine sites in all patients and selected sites in some patients." They do not state how many patients did not have slit-skin smears prepared from lesions. In patients toward the tuberculoid end of the leprosy spectrum, classifying the patient as PB without doing slit-skin smears from lesions may give erroneous results. A WHO Expert Committee report states: "Skin smears should be taken from a minimum of three sites, including one earlobe and two representative active skin lesions. In paucibacillary patients, if there is only a single skin lesion, the two smears may be taken from its active edge at sites diametrically opposite to each other." ([1]). The same WHO publication briefly discusses the difficulty in classifying borderline tuberculoid patients with multiple macular lesions. The report states: "Classification of these patients should therefore be undertaken only after careful consideration of all clinical features." ([2]). In order to deal with the problem of classifying such patients, a subsequent WHO publication recommends that any patient who has more than five skin lesions and any patient who has more than one nerve trunk involved be classified as multibacillary ([3]). Thus, it is unclear how many of the 21 patients in the Ebenezer paper in fact have paucibacillary leprosy.

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Dr. Ebenezer Replies

TO THE EDITOR:

Professor Richard I. Frankel's comments on our paper entitled "Multibacillary nerve histology in clinically diagnosed borderline tuberculoid leprosy patients," which appeared in the September 1996 issue of the INTERNATIONAL JOURNAL OF LEPROSY, has given us an opportunity to restate our opinion.

All patients in our study had either five skin lesions or less. Only borderline tuberculoid patients who had an obvious cutaneous nerve enlargement were selected, and none had any nerve trunk enlargement. Inadvertently, this information was not presented in our manuscript. Out of the 21 patients selected, 1 was skin-smear positive for acid-fast bacilli (AFB) and skin biopsies from 2 and nerve biopsies from 10 showed AFB with borderline lepromatous histology.

In practice, most programs use clinical criteria for classifying and deciding the appropriate treatment regimen for individual patients, particularly in view of the nonavailability or non-dependability of skin-smear services ([1]). Therefore, irrespective of the skin smear, clinically grouping such patients as paucibacillary may inappropriately include a significant number of patients having multibacillary leprosy.

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