Therefore, is the treatment of patients with antileprosy drugs, as far as nerve damage is concerned, only helping in the formation of scar tissue? If so, restoration of the dying nerve's function needs to be considered as a separate entity from the antimycobacterial treatment in relation to leprous neuritis.

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Pyrazinamide as a Part of Combination Therapy for BL and LL Patients—a Preliminary Report

To the Editor:

In a recent paper in the International Journal of Leprosy ([1]), Katoch and colleagues report the addition of pyrazinamide in a dosage of 1.5 g daily for the first 2 months to therapy with other drugs. The authors claim that, despite the failure of earlier workers to demonstrate activity of the drug in *Mycobacterium leprae*-infected mice ([2]), the addition of pyrazinamide to several

chemotherapeutic regimens was accompanied by some beneficial effect. Their evidence appears to consist primarily of the results of measurements of the morphological index (MI) and of inoculation of normal mice. After 2 years, 2 of 51 patients not treated with pyrazinamide were noted to have solid-staining organisms in their smears, whereas solids were found in the smears of none of the 63 treated with pyrazinamide. At this same time, viable *M. leprae* were said to have been detected by mouse inoculation in biopsy specimens obtained from 9 of 38 patients treated without pyrazinamide, and in the specimens of only 1 of 20 patients treated with the drug. Finally, after 4 to 5 years, viable organisms were detected by mouse inoculation in none of 14 specimens obtained from patients treated with pyrazinamide, whereas viables were detected in the specimen of 1 of 6 patients not treated with pyrazinamide.

By Fisher's exact probability calculation, the likelihood of the reported results having occurred by chance, when the two samples have been drawn from the same population, is greater than 0.05 in every case.

Despite the widely publicized injunction against the use in leprosy patients of a drug that has not been shown to be active against *M. leprae* in mice, one is occasionally almost persuaded that such a course is justified, perhaps because the unusual properties of the drug promise great benefits, if only the drug can be shown active. If one permits himself to be persuaded, he should at least maintain his scientific scepticism, and require that the proof that the drug is effective in patients be unimpeachable. This Katoch, *et al.*, have failed to do.

In fact, the injunction against the use in patients of drugs not already shown effective in mice was based on the felt need to protect patients from clinical trials with drugs that were ineffective at best and, at worst, hazardous. Perusal of this paper reveals that patients were exposed to regimens that included isoniazid and thiacetazone, drugs that are also potentially toxic and, with respect to isoniazid, a drug that has not been demonstrated effective against *M. leprae* in mice.

One additional criticism must be leveled against the authors. Nowhere are given the criteria for multiplication of *M. leprae* in the mouse foot pad, despite the obvious importance of the results of mouse inoculation to the authors' case. In the report of the THELEP trials in Bamako and Chingleput ([3]), to which the authors refer, persisting *M. leprae* were carefully defined.

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Reply to Dr. Levy

To the Editor:

Dr. Levy has raised some questions about our findings on pyrazinamide reported in the International Journal of Leprosy recently ([6]). We have been fully aware of various issues raised by Dr. Levy, and have considered these in depth even before reporting our results. In the following paragraphs we would like to clarify these questions: a) criteria of multiplication, b) reasons