

among the patients on monotherapy the reinfection could have occurred by the DDS-resistant organisms, although this is a rare possibility.

I shall be much obliged if the authors can kindly offer their comments.

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## Dr. Kurz Replies

### TO THE EDITOR:

We are thankful to Dr. B. N. Reddy for his valuable comments on our paper on relapses in multibacillary leprosy (<sup>2</sup>). We wish to offer the following clarifications.

Our topic was relapses—their rate and time distribution in relation to regularity of treatment—not a discussion of their possible causes. Whether the underlying phenomenon is reinfection, resistance, or any other mechanism had no bearing on the question at issue: Do multibacillary (MB) patients really benefit from life-long dapsone monotherapy after negatization? From our data, the answer is yes, and at any time after negatization relapse occurrence was affected by regularity during both smear-positive and -negative periods. As discussed in the paper, this result contrasts with the proposition that relapses occurring beyond the first 3 years of negatization could well be reinfections (<sup>1</sup>). It explains our statement on reinfections pointed out by Dr. Reddy.

We did not include an initial bacterial index (BI) in our analysis. A first reason was the skewness of the BI distribution, most patients being in the 2+ and 3+ categories and, following current practice, it did not seem necessary to go beyond the MB classification. A second reason was the impact of case-finding activities on the BI at diagnosis, and thus the difficulty of interpreting results of relapse rates by BI categories.

Dr. Reddy suggests that “rigid” selection criteria could have biased our results. We do not share his view. We wanted to study relapses in true negative subjects, not a mix of relapses and reactivations in patients falsely declared negative in a single examination. Hence, our admissibility criteria emphasized specificity rather than sensitivity. In no way do they affect the validity of the results. Rather, they enhance it by se-

curing homogeneity of the study base (the 1883 MB patients who showed 2 consecutive years of BI = 0).

Comparability would be an issue only if eligible subjects were not enrolled, whatever the reason, and it was not the case. The only subjects of concern in this study are 160 enrolled patients who dropped out somewhere after negatization. They would bias the results only in case of their dropping out being related to relapse occurrence. In view of the organization of leprosy services in the Polambakkam Leprosy Centre area, we consider it unlikely, although we cannot be quite certain.

During the study period (1955–1982), MB cases in Polambakkam received life-long dapsone monotherapy. As proposed by Dr. Reddy, surely some of them did not consume their drug, and there must be some cases where the treatment was stopped. Our purpose was to assess a policy (life-long dapsone monotherapy), not dapsone effectiveness. Those behaviors being inherent to the policy, they are part of its assessment. They were taken into account by dividing the study base into groups of different regularity.

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### REFERENCES

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