



Review of chemoprophylaxis in leprosy contacts

To the Editor:

It was with great interest that I read a review in this journal by Reveiz and colleagues titled, "Chemoprophylaxis in contacts of patients with leprosy: systematic review and meta-analysis" (1). However, despite the fact that I agree with the final conclusions, there seem to be several discrepancies in the data that warrant revision.

Several studies were not included in the review because "they were nonrandomised or noncontrolled trials" (1), among them a report by Dharmendra and colleagues (Reference 20 in the review) (2). Indeed, the description in this report raises doubts as to whether the allocation process was random. However, Reveiz's review included data from a report by Noordeen (3), which is from the same excluded study. Therefore, it is not clear whether or not the authors of the review considered this study eligible. Furthermore, the report by Noordeen and colleagues (3) presents 48 cases in a placebo group and 23 cases in the dapsone group, numbers which do not appear in Figure 1 of the review; nor are the number of cases in Figure 2 found in the report.

There are other concerns in Figure 1 of the review, as follows. Wardekar (4) reported results from two surveys, but only those 0–25 years of age were allocated to receive dapsone or a placebo, and thus, only this age group was used to estimate the effects of the chemoprophylaxis. The rates in the two groups among those more than 25 years of age were similar because none received chemoprophylaxis. However, the numbers of cases presented in Figures 1 and 2 in the review (76 for chemoprophylaxis and 132 for placebo groups) are leprosy cases in the total population 0–25 years of age, and more than 25 years, thus diluting the effect of the chemoprophylaxis. Even making calculations based on intention-to-treat analysis, as stated in the review, does not clarify why those more than 25 years of age were included, since they cannot be compared with the younger age group. The total number of leprosy cases in the two surveys among those 0–25 years of age was 43 in chemoprophylaxis group and 119 in placebo group. The denominators are quite similar, and thus, an estimate of the risk ratio taking the two surveys together would be 0.36. There seems to be more heterogeneity than what is reported in the Figure 1 of the review.

In addition, the data presented as coming from a report by Noordeen published in 1969 (3) were actually reported by Noordeen and colleagues in 1978 (5). The 1978 data were presented separately for contacts of "lepromatous" (three arms: placebo, low dose, and usual dose) and of "non-lepromatous" index cases (two arms: placebo and dapsone). However, Figure 1 of the review presents only placebo and

dapsone groups with usual dose among contacts of lepromatous cases. As in the 1978 report (5), the rate ratio between low dose of dapsone over placebo (0.60) is quite similar to the rate ratio between usual dose over placebo (0.62); so, it seems the data from the two groups with low and usual dose of dapsone should be combined. Therefore, the effect of chemoprophylaxis in Figure 1 does not consider contacts of lepromatous who received low dose of dapsone and the contacts of non-lepromatous cases (rate ratio = 0.66). The disease frequency was presented as a rate in the original report (5) and the estimates of effect were similar (see the three risk ratios in Reveiz [1], Table 1). Since Figure 1 does not show effect separately for the clinical form of the index cases, cases and person-time of all control and experimental groups could be combined to a single rate for chemoprophylaxis and for control groups. It is not clear why the authors of the review made the choice to represent only the effect using two groups when the paper by Noordeen (5) presents three effects that could be combined.

Finally, the data presented in Figure 1, and attributed to the 1983 report by Neelan and colleagues (6), are actually from the 1986 report by Neelan and colleagues (7); and the data presented originally in the 1983 report by Neelan and colleagues (6) are presented as data from the 1978 report by Noordeen and colleagues (5).

Furthermore, there are discrepancies among numbers in Figures 1–3 and Table 3. All these differences can change the overall estimate of the chemoprophylaxis and the assessment of heterogeneity. Again, despite the fact that I agree with the final conclusions, it seems that Figure 1 should be redone.

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