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Rifampicin therapy in shigellosis in infancy

Sir,
The article under this title by Naveh *et al.* (*Archives*, 1977, 52, 960) deserves comment. It seems beyond doubt that the 11 children referred by the authors made up an exceptional group, both from the protracted course of the diarrhoea (1 to 4 months) and from the unexpected resistance *in vitro* to antimicrobial drugs. Diarrhoea due to shigella is usually a self-limited disease lasting a few days (Roy *et al.*, 1975). As with other acute bacterial diarrhoeas, prolongation of its course is generally not due to the infection itself, but rather to sugar intolerance or to some form of the postgastroenteritis syndrome (Gribbin *et al.*, 1976).

Failure to respond to furazolidone is not surprising; drugs not absorbable by the intestine can hardly affect the course of diarrhoea caused by invasive bacteria such as shigellae (Drachman, 1974). On the other hand, the reported failure to respond to treatment with chloramphenicol, ampicillin, or co-trimoxazole to which the shigella revealed *in vitro* sensitivity is astonishing and difficult to understand; such a situation has been only sporadically reported (Haltalin *et al.*, 1972).

Commenting on the good results obtained with rifampicin, Naveh *et al.* state that it is 'a useful alternative preparation in drug-resistant shigella infection . . .', a contention which they justify insufficiently.

The *Archives* has a world wide readership which includes many of the developing countries, and it is in these countries that shigellosis, tuberculosis, and other infectious diseases are prevalent. Rifampicin occupies an important place in the treatment of severe tuberculosis. A superficial reading of the article of Naveh *et al.* could lead to the more liberal use of rifampicin in disease other than tuberculosis, and this would be undesirable. For this reason, it seems that a more accurate and descriptive title such as 'Rifampicin therapy in protracted and resistant shigellosis in infancy' would have been preferable.

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Dr Naveh and co-workers comment:

We agree with Professor Salazar de Sousa that the group described in our paper was an exceptional one and this was the reason for using an unconventional antimicrobial agent. The assumption that protracted diarrhoea in our group might have been the result of sugar intolerance cannot be correct as in shigellosis the infection selectively affects the colon (Roy *et al.*, 1975), which has nothing to do with the split and absorption of ingested sugars.

We did not suggest that rifampicin be used in a normal case of bacillary dysentery. We have used rifampicin in shigella septicaemia (Naveh and Friedman, 1973) and recently in a shigellosis situation where 11 infants had been crossinfected and we were urged to eradicate this infection from the nursery, and thus protect babies from being crossinfected. Furthermore, use of rifampicin in non-tuberculous infections did not result in higher incidence of resistant strains of *Mycobacterium tuberculosis* in those countries where rifampicin was used in tuberculous and also nontuberculous infections (Acocella *et al.*, 1977; Trallero *et al.*, 1977).

Finally, we agree with Professor Salazar de Sousa that a more accurate and descriptive title for our paper might have been 'Rifampicin therapy in protracted and resistant shigellosis in infancy'.

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