studies on groups on very high intakes compared with those on lower intakes have revealed no differences in mean haematological values, whether in children or adults. Observations on contrasting groups have revealed no differences in mean serum calcium levels, nor in the mean cortical thickness or other dimensions of the second metacarpal. Indeed, we have found satisfactory calcification even in groups of mothers who have had numerous pregnancies and long lactations.² Nor in the groups mentioned have we found differences in the growth rate of children. In our appreciation, Third World experience does not support the view that phytic acid is significantly prejudicial to mineral metabolism or to health.

In Iran the reported ill effects due to a very high phytic acid intake, associated apparently with a high calcium intake, are extremely interesting.¹ Nevertheless, definitive studies on the occurrence of rickets in Iran (of the calibre of the studies that have been made on Asian children in Britain) do not appear to have been published, nor have studies on bone density, etc, such as we have undertaken on African Blacks.

As an illustration of the extravagant claims made regarding phytic acid it has been reported that researchers at the Danish Food Institute are maintaining that "the ingestion of bran may also lead to cancer, arteriosclerosis, or phlebolithiasis."³ Soon after the last war, in a discussion on the nutritive value of bread, Fredericia⁴ stated, "In Denmark we eat, and have done for thousands of years, brown rye bread, 100% wholemeal. If what has been said about brown bread is true, we should get rickets, and yet we have very little rickets in Denmark. . . ." To which of these views are we to attach credence?

It must be understood that even clear-cut evidence from experimental studies on animals and man may disagree profoundly with what is found epidemiologically. For example, the cariogenic effect of sucrose is very readily demonstrable in experimental studies; yet epidemiologically differences in caries scores in high compared with low consumers of sucrose consistently are disappointingly small.

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¹ Reinhold, J G, *Lancet*, 1976, **2**, 1132.

² Walker, A R P, Richardson, B D, and Walker, B F, *Clinical Science*, 1972, **42**, 189.

³ *World Environment Report*, 1977, 25 April, p 8.

⁴ Fredericia, L S, *Proceedings of the Nutrition Society*, 1976, **4**, 28.

The community physician: will he survive?

SIR,—Under the above heading Drs W H Parry and J E Lunn (27 August, p 589) report the reduction in responsibility and job satisfaction perceived by the majority of 52 former medical officers of health of county or county borough councils employed in community medicine posts in 1976 compared with their pre-NHS reorganisation experience in 1972. They further show that a group of 52 carefully matched doctors who were not working in the public health field in 1972 did not feel that they had suffered a similar decline in responsibility and job satisfaction over the same period.

To argue, as the title suggests, that on the basis of these findings the existence of community physicians is threatened is to go much further than the data warrant. In the first place, according to the establishment and in-post figures presented in the article the 52 erstwhile medical officers of health make up only 8% of the community physicians in established posts in 1976 and the views they express may therefore be unrepresentative of the majority of community physicians, many of whom entered the specialty by quite different routes. Secondly, while it is unlikely that doctors in this group would encourage aspirants to careers in community medicine, it is probable that the slow rate of recruitment to the specialty is also the result of other factors such as the unavailability of UMT payments in the training grades in this specialty. Moreover, it is assumed that all the posts currently established in community medicine will endure.

A rather more obvious deduction from the data presented would be that the previous training and experience of some public health doctors did not fit them too well for the interdisciplinary approach to the planning and administration of the reorganised Health Service and that the short-term training measures taken in an attempt to equip them for their new roles have failed to maintain their professional satisfaction.

A more appropriate title to the report might have been "Paradise Lost for the MOH."

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Antibiotics for presumed viral respiratory infections

SIR,—In the results of their trial of antibiotics for presumed viral respiratory infections (27 August, p 552) Dr Brent Taylor and his colleagues report that placebo treatment allowed an earlier return to normal activity; but as over 10% of the placebo-treated children were withdrawn with complications compared with only 1.5% of the antibiotic-treated children it might reasonably be assumed that those remaining were not properly comparable. Despite this bias they report a significant difference in favour of the antibiotics ($P < 0.002$) when analysing all criteria for improvement simultaneously, but later suggest that the method of analysis (MANOVA) may have allowed small, otherwise unnoticed, differences to have been detected. (The statisticians do it with mirrors, you know.)

Yet if one simply counts as "failures" all those children who had to be withdrawn or who after eight days had purulent nasal discharge, sore throat, wheezy chest, or fever the difference between the groups is obvious enough without any statistical expertise, as shown in the accompanying table.

Now the concluding statement of their summary, that "the benefits of antimicrobial

Results of treatment at eight days

	Amoxycillin	Co-trimoxazole	Placebo
No entered	56	75	66
No "failed"	9 (16%)	12 (16%)	26 (39%)

treatment in presumed viral respiratory infections are marginal, and they should not be routinely prescribed for these conditions," may well be true. It has certainly been repeated often enough. But it is disturbing to find it repeated in a reputable scientific journal in conjunction with evidence that appears to argue the contrary.

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Treatment of cutaneous larva migrans with local thiabendazole

SIR,—The recommended treatment of creeping eruption (cutaneous larva migrans) includes ethyl chloride spray, oral thiabendazole,¹ and local applications of thiabendazole.² Cutaneous freezing by ethyl chloride spray is quite unpleasant. Oral thiabendazole, in the recommended dose of 22 mg/kg body weight can lead to unpleasant side effects such as vomiting. Thiabendazole cream is often not available.

In two children aged 2½ years infected with creeping eruption treatment with ethyl chloride for five days was ineffective, as was a five-day course of thiabendazole 25 mg/kg. Consequently a topical preparation of thiabendazole was prepared by triturating an 0.5-g tablet of thiabendazole with 5 g petroleum jelly, using a pestle and mortar. The resulting cream was applied liberally over the track of the worm daily for five days. After the first application the worms had ceased moving and by the fifth day were completely resorbed. This treatment was also given to two other patients, who prepared the cream and applied it as indicated, also with success.

Systemic treatment with thiabendazole is associated with vomiting and nausea. In our hands, it was not shown to be effective. As the topical application is easily prepared and apparently successful, without the risk of side effects, we would recommend it as the treatment of first choice in this condition.

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¹ Forfar, J O, and Ameil, G C, *Textbook of Paediatrics*. Edinburgh, Churchill Livingstone, 1973.

² Conn, J D, *Current Therapy*. Philadelphia, Saunders, 1976.

Death from ischaemic heart disease

SIR,—Your leading article on this subject (27 August, p 537) presents the conclusion of Clayton and his colleagues¹ based on an analysis of death rates in England and Wales from 1950 to 1973. In their analysis they assumed that the decline in deaths due to hypertensive disease has been largely artefactual, resulting from the transfer of diagnosis from hypertensive disease to arteriosclerotic and ischaemic heart disease. Consequently they combined deaths due to hypertensive disease with those due to ischaemic heart disease and concluded that mortality from ischaemic heart disease from 1950 to 1973 increased considerably in men (particularly men aged 35-44 years) but hardly increased at all in women.

These conclusions differ from our own. In