

essential—particularly for children with a high prevalence of undernutrition and disease burden, who tend to be more physically vulnerable—and robust evidence on the age-specific dose of daily multiple micronutrients supplementation is urgently needed.

There was almost no difference between the intervention and control groups in the tendency for the stunting rate to increase from baseline to 24 months. The prevalence of stunting in the overall study population was high, even though basic counselling on breastfeeding and complementary feeding was provided. For such a population, a single intervention like multiple micronutrients tends to have limited effects,² and multiple micronutrients combined with protein energy are more effective than multiple micronutrients alone.³ Rather, multifaceted interventions beginning in the prenatal and early postnatal periods need to be examined.⁴ Improvement of the health and nutritional status of children begins with ensuring maternal nutritional health in pregnancy, the lactation period, and even pre-pregnancy. As young children's nutritional status results from a complex interaction of various factors such as food consumption, safety of food and water, sanitation, nutritional knowledge of caretakers, and access to health care, an integrated strategy is necessary for their growth.⁵

We declare that we have no conflicts of interest.

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Authors' reply

We thank Parminder Suchdev and Lynnette Neufeld for their comments on our study¹ and the reservations expressed on the conclusions.

First, the quality of evidence is questioned on three points: the trial was not placebo-controlled, the data for morbidity were captured by maternal recall, and the compliance rates of about 50% represent poor behaviour change. The vast majority of micronutrient powder trials to date have not used a placebo because there are challenges in manufacturing a safe placebo formulation for daily use. We acknowledge this limitation, common to most trials of micronutrient powders. We acknowledge (and discuss in our paper)¹ the limitations of morbidity data derived from maternal recall. Many longitudinal studies use maternal reporting of morbidities and in our study¹ this was accompanied by corroboration of reported maternal respiratory morbidities and assessment by trained community health workers. Our trial was designed to mimic as closely as possible what is feasible in real-life programmes, and we do not believe that the overall compliance represents poor behaviour change. The compliance observed over a 12-month period is better than that reported in other effectiveness trials of micronutrient powders including the programmatic rollout in Kenya led by Suchdev,² which only achieved an average consumption of 0.9 micronutrient powder sachet per week. We believe the observed coverage is consonant with what is realistic in real life settings and if anything, makes our findings all the more important.

Second, we did not claim that our findings are generalisable to

all populations. We discussed this point in our paper, noting that our findings were from a very poorly malnourished population, similar to many populations in South Asia. The current WHO guidelines on the use of micronutrient powders recommend the use of micronutrient powders for children aged 6–23 months in populations in which the prevalence of anaemia is 20% or higher. In many low-income populations at risk for anaemia and micronutrient deficiencies, the observed rates of stunting and wasting would be similar to those that we encountered. As far as we are aware, there are no excluded subgroups or differential implementation in programmes, and future studies of micronutrient powders might explore this possibility.

Third, as discussed in our paper,¹ we are uncertain about mechanisms for the observed respiratory effects, but these findings are by no means unique. In the CIGNIS trial,³ increased rates of lower respiratory tract infections or pneumonia were reported in children receiving micronutrient fortified weaning food. By contrast, the observations on diarrhoea in our study should not come as a great surprise. We disagree with Suchdev and Neufeld that the reported diarrhoea cases were all mild and that mechanisms are unclear. There was an increase in bloody diarrhoea and some evidence of an effect on severe diarrhoea (6+ stools although not achieving $p < 0.05$). A recently published trial⁴ of micronutrient powders in a malaria endemic area of Africa reported an excess in hospital admissions due to all causes among children taking micronutrient powders containing iron during the 5-month supplementation period, with marginally higher rates of diarrhoea diagnosed in outpatient settings. The statement that there is no clear pathophysiological mechanism for diarrhoea and no increase in specific stool pathogens is incorrect. Among children receiving micronutrient powders we observed an excess of

Aeromonas, a known pathogen causing diarrhoea.⁵ A number of studies have reported and discussed potential mechanisms for excess diarrhoea with use of iron. Future studies should be designed to explore this in greater depth.

Moreover, despite the extra intake in both micronutrient powder groups and the existing programme for vitamin A supplementation, we did not observe any change in biochemical measures.

Fourth, we believe that the argument that the benefits for growth are small but significant while the apparent effects on morbidity are small and therefore not significant is inconsistent. We want to re-emphasise that what we are calling for is a proper discussion of the benefits and potential harms in existing and future programmes of micronutrient powders. We hope that our findings from the largest trial of micronutrient powders in populations representative of urban and rural Pakistan, will spur further robust evaluation of the benefits and risks associated with this intervention.

We also thank Ruoyan Tobe-Gai and Rintaro Mori for their comment. Their point that the same iron dose should not be applied throughout the age range 6–18 months needs further evaluation and consideration by WHO. Changes in intestinal maturation throughout infancy could be important. These might require varying entry criteria for intervention and consideration of iron stores before trials of different doses. We also agree that micronutrient powders alone are not the solution in poorly nourished populations—a point that we noted in recommending alternative and additional strategies in our concluding statements.

We declare that we have no conflicts of interest.

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Retraction of the Jikei Heart Study: misquotation of a Comment

The *Lancet* Editors recently announced (Sept 7, p 843)¹ the retraction of the Jikei Heart Study. The Editors stated that when the Jikei Heart Study² was first published, “Staessen and Richart³ congratulated the investigators on their report.” However, this citation would have been much more balanced had the Editors also quoted the next sentence of our Comment:³ “Nevertheless, one should not accept at face value the main conclusions of the Jikei report”. We clearly listed the many weaknesses of the study, including inadequate sample size, insufficient dosing of study medications, the weakness of soft endpoints, the open-blinded-label endpoint assessment that did not account for bias in reporting endpoints by unblinded investigators, the between-group differences in blood pressure during the early phase of the randomised follow-up, and the lack of generalisability to general practice. We were the first to show that blood pressure level, not drug class,

is key in preventing cardiovascular complications associated with hypertension.⁴ In our opinion,³ the Jikei Heart Study² had no value if marketers would spread the message that valsartan, or for that matter any other antihypertensive drug, might confer benefits beyond blood-pressure lowering. In conclusion, we should have been given full credit for our interpretation of the Jikei results, and we should not have been quoted in a way that might be misinterpreted.

We declare that we have no conflicts of interest.

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Performance indicators and clinical excellence

Attending post-intake ward rounds in various National Health Service (NHS) trusts around the UK, I am concerned that clinical expertise is being crowded out by a need to meet various key performance indicators.

In one hospital, I was told on arrival that the trust had, over a year, moved from the bottom to the top category of performing trusts. I was therefore puzzled when soon thereafter



India Picture/Corbis