

cannot be excluded and there is no evidence that the analysed groups were balanced with respect to the (joint) distribution of important confounding factors and effect modifiers. There is, in particular, a need to control for energy intake from the home diet and for possible replacement effects before regarding data as evidence for a causal relation.

All these restrictions notwithstanding, there is a strong suggestion that there might be a biological effect. One is then tempted to read the report from the perspective of the programme manager who regards "supplementary feeding [as] an option as a component of maternal and child health services of nutrition programmes". One's priority would indeed be the reduction of the problems that have the greatest clinical and social consequences—i.e. wasting and overt clinical malnutrition. Unfortunately, the data provide no evidence for such reduction. The only documented effect—assuming it is real and not biased—is on linear growth, which may or may not be related to immediate public health concerns. Furthermore, this effect is modest in operational terms. The maximum difference between the groups was for height-for-age at about age 9 months: 53% of the study children versus 80% of the controls were on or below  $-1$  Z score of the reference. According to standard formulae, one can calculate an effect in 34% of the study group. The impact at the population level would, however, be less encouraging: if coverage under routine field conditions were as high as in the research setting (which is improbable), 26% (190/741) of pregnant women would accept supplementation and comply with the full three months programme. Thus a supplementary feeding programme for pregnant women could, at best, reduce by some 10% the proportion of children at or below  $-1$  Z score of the National Center for Health Statistics height-for-age distribution by age 9 months. Before one accepts that targeting supplementary feeding programmes to pregnant women are "more productive and cost-effective", one should ask whether this option is productive and cost-effective enough to justify the effort, and whether it is a real alternative to anti-poverty programmes as advocated by Gopalan.

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### Energy supplementation during pregnancy and postnatal growth

SIR,—Professor Kusin and colleagues report higher growth rates in infants and lower levels of stunting in preschool children after food supplementation of their mothers during pregnancy in East Java. They provide new information of considerable interest, but we wish to point out some limitations in their design and analysis.

First, the study is described as a double-blind, controlled, randomised trial. Data collection may have been blind, but the analysis, at least, could not have been so, since the impact of supplementation on birthweight had been previously examined. Furthermore, the analysis is done on a subsample of children, selected through several steps (mother compliance, minimum number of measurements per child), which might have affected group comparability.

Because of missing measurements during the follow-up from birth to age 5 years, growth curves were calculated from a mathematical model. The three-variable infancy component of the ICP-model was used from birth to age 9 months and resulted in an overestimation of growth during the first month of postnatal life. The infancy component overestimates weight at age 1 month, as is common in three-variable models.<sup>1</sup> An overestimation of weight at age 1 month will bias the estimation of the 1–2 months growth rate, and therefore a four-variable model such as the Reed model<sup>1</sup> would be preferable. When interpreting statistical tests on growth increments, one should be aware that models reduce variability by smoothing the growth curves, so that differences can artificially reach statistical significance.<sup>2</sup>

A further issue is that prevalence of stunting was significantly lower in the test group between age 6 and 15 months, when  $-1$  Z-score was chosen as the cut-off point. Since the recommended

cut-off point for the definition of malnutrition is  $-2$  Z-scores,<sup>3</sup> it would be interesting to know the differences between groups at various ages with this cut-off point.

Lastly, Kusin et al compare the impact of supplementation of pregnant mothers on the child's growth in East Java to the impact in the Bogota study, in which mothers and children were also supplemented after birth. Since the impacts on linear growth are similar in the two studies, they suggest that postnatal supplementation of infants or mothers provides no additional benefit and that supplementation during the last trimester of pregnancy might be more cost-effective than supplementation of children under 5 years. This may not be so if supplementation is targeted to infants for a short time, where linear growth is especially sensitive to interventions.<sup>4</sup> Specific controlled clinical trials are needed to compare supplementation during pregnancy with supplementation during infancy in different communities.

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\*These letters have been shown to Professor Kusin and Dr Houkoooper, whose reply follows.—ED.L.

SIR.—Dr van der Stuyft and Dr Lerberghe have understandable, but unjustified, concerns about our restriction of analyses to the groups who complied most. Our hypothesis was that prenatal energy supplementation for a minimum of the previous 90–100 days would improve postnatal growth, and consequently analysis of the most compliant groups was the best means to quantify an effect.

Self-selection cannot be excluded, but the study design of a random allocation to the low-energy and high-energy supplement within villages should take account of this factor. In our earlier report of effect on birthweight,<sup>1</sup> a full account is given of the comparability of these two energy groups. Particular attention was paid to the possibility of replacement: home diets were measured by the test weighing technique during 3 consecutive days at pregnancy months 4–6, 7, 8, and 9.<sup>2</sup> The supplement was offered daily at home, field workers stayed until the mother finished drinking, and left-overs were brought back to the field station. In field studies this is the most accurate and feasible method. There is a limit to collection of information and the most probable confounding factors were scrutinised before the final analysis.

Which group constitutes the highest priority, what one wants to achieve, and by which means this goal can be reached are matters for debate. If one aims at more than "death control strategies",<sup>3</sup> one would target at a satisfactory nutritional status, which we did. Prevention of severe malnutrition and wasting should be approached on infectious disease control and management of sociocultural factors; nutrition is usually secondary to these. Moreover, routine supplementary feeding programmes for preschool children do not prevent and too often do not permanently cure severe malnutrition and wasting. Linear growth was used as an index of a positive effect, to demonstrate whether the children achieved their genetic potential physical growth, as an indicator of health.

One should keep in mind how much can be achieved in a specific time. The difference in mean height between supplemented and unsupplemented groups was 1.7 cm at 60 months. Improved living conditions and socioeconomic status in industrialised countries brought about a secular trend in adult height of 1 cm per decade.

We did not target severe or moderately undernourished women: 12% of women of reproductive age were this undernourished. At

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