

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—ie, 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,

cut-off point for the definition of malnutrition is -2 Z-scores,³ 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 mother 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.⁴ 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 Houtkooper, whose reply follows.—EDL.

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,¹ 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 9.² 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