However, this hypothesis cannot be tested from the available data. The fiber contents of the diets, and not to a single dietary component, are attributable to multiple factors, such as the saturated fat, cholesterol, and fiber contents of the diets, and not to a single dietary component. Hence, it is inappropriate to compare those data with the data that were collected during the controlled intervention period (Table 3 from reference 2) and draw conclusions. Regardless of the intervention, as we have seen in the case in their latest study (2), in which all treatment groups showed improvement, as has been reported numerous times before. The authors had no conflicts of interest to declare.

Alice H Lichtenstein
Nirupa R Matthan

Dear Sir:

We would like to respond to the letter by Lichtenstein and Matthan regarding my editorial comments (1) concerning their recent publication (2). The statements in contention refer to the change from baseline in lipid values and to the hypocholesterolemic effects of soy proteins compared with those of animal proteins. These statements were meant to call attention to the very phenomenon that the authors describe in their letter; ie, that lipid and lipoprotein values observed at an initial screening have less favorable cardiovascular risk profiles than do those observed during intervention periods. This was the case in their latest study (2), in which all treatment groups showed improvement, as has been reported numerous times before. The editorial comments refer to this in a qualitative manner and were not meant to imply that statistical analyses were performed to examine that relation. It is well appreciated that the nature of the study was a randomized crossover design and that the statistical analysis used analysis of variance with the main effect of diet and subject as repeated measures for each outcome variable with pairwise comparisons between endpoint values for each of the treatment groups. There is also no misunderstanding that the conclusions stated by the authors in their publication referred to outcomes from treatments based on differently processed soy products compared relative to each other and with animal protein and to a comparison between the effects of animal protein and soy protein on CVD risk factors. In summary, we agree that soyfoods included in the diet could be

REFERENCES

The authors had no conflicts of interest to declare.
beneficial for overall cardiovascular disease risk reduction through displacement of atherogenic dietary components or other mechanisms that have yet to be elucidated.

The author had no conflict of interest to declare.

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REFERENCES

Is there any monitoring of the quality of vitamin A capsules used in supplementation programs?

Dear Sir:

We are writing to comment on the publication entitled “Randomized controlled safety and efficacy trial of 2 vitamin A supplementation schedules in Tanzanian infants” by Idindili et al (1), which was recently published in the Journal.

We conducted a similar trial in Kintampo, Ghana, and our study and that by Idindili et al conducted in Tanzania were both funded by the World Health Organization in 2002. Both studies assessed the effectiveness of a new maternal and young infant vitamin A dosing regimen, in which mothers and infants in the intervention arm were given 400 000 IU vitamin A within 6 wk of delivery, and their infants were given 50 000 IU vitamin A at 6, 10, and 14 wk of age as recommended in the Annecy Accords (2). Mothers and infants in the control arm were given the old regimen of 200 000 IU vitamin A within 6 wk of delivery, and their infants received 25 000 IU vitamin A at 6, 10, and 14 wk along with DPT (diphtheria-pertussis-tetanus) immunizations.

The effect of the 2 supplementation regimens on breast-milk retinol concentrations and on infant serum retinol concentrations, assessed by using modified-relative-dose-response tests, were assessed at 6 wk, 6 mo, and 9 mo of age. The incidence of side effects, such as bulging of the anterior fontanel, vomiting, and incidence of severe morbidity, were also measured at these ages.

Our results were similar to those of the Tanzanian study, but were only available for a small number of infants who were enrolled early in the trial because the routine monitoring of capsule content showed degradation of some vitamin A capsules. The vitamin A capsules used in both trials came from the same source. The capsules used in the Tanzania study also underwent degradation; however, enrollment in the Tanzanian trial was completed before the degradation occurred; the Ghanian trial started a year later.

The conclusion of both trials was that a regimen of 50 000 IU vitamin A with early infant vaccines has no advantage over the previously recommended regimen of 25 000 IU, which has important policy implications.

The combined evidence from those trials and the original World Health Organization multicenter trials (3) in Ghana, India, and Peru suggests no overall benefit of vitamin A supplementation in combination with vaccines in the first 5 mo of life. Indeed, it is no longer a policy recommendation to do so; the emphasis remains on larger doses to older children.

However, these trials also raise questions about the stability of capsules being used in vitamin A supplementation programs and the need to monitor their retinol content to ensure that young children actually receive adequate amounts of vitamin A.

The authors had no conflicts of interest.

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REFERENCES

A controlled trial of reduced meal frequency without caloric restriction in healthy, normal-weight, middle-aged adults

Dear Sir:

Studies in rodents have shown a marked increase in lifespan and disease resistance in animals subjected to intermittent fasting (1, 2). The suggestion that humans may respond similarly to a reduced meal frequency raises several important questions. Is reduced meal frequency feasible in humans? Does reduced meal frequency confer health benefits independent of calorie restriction? Are there alternative approaches by which to study the relation between meal frequency and longevity? The recent article in the Journal by Stote et al (3) provides insight into this inadequately studied aspect of human nutrition (4). With respect to the first question, the trial by Stote et al (3) and that by Heilbronn et al (5) showed that compliance with once-a-day and alternate-day feeding is feasible for humans participating in controlled trials. However, greater subjective ratings of hunger indicate that compliance is unlikely in free-living persons. In addition, restricting consumption to once a day may compromise

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