receiving identical vitamin A–sufficient diets” (6). On the basis of this observation (Figure 2 in reference 6), the RDR was originally conceived as “the degree by which plasma vitamin A levels otherwise ‘normal’ to an individual are reduced by deficiencies in hepatic stores.” Contrary to what was stated (without reference to published evidence) by Wieringa et al, this would imply that R0 and R5 are independent. Wieringa et al seem to suggest that the regression-to-the-mean effect as reported in our models 1 and 2 were due to the independence between R0 and R5. This would be incorrect: such an effect cannot exist when 2 variables are independent, and the values for these 2 variables in our models were generated independently and randomly by computer. The regression-to-the-mean effect that we reported was due to the fact that R0 occurs both on the x and y axes of Figures 1 and 2 in reference 1.

Our concern with the article by Wieringa et al (4) relates mainly to their analysis of the relation between the MRDR and plasma retinol concentrations; we acknowledged that their MRDR values were less than expected for the serum retinol concentration in infants who had received iron. This does not mean that the mathematical artifact we described did not play a role in their analysis, which it must have for the reasons outlined in our article.

We referred to the article by Wieringa et al (4) as being from “our group” because it was conducted under the academic supervision of Clive West. To the extent that we may have given the erroneous impression that the work by Wieringa et al (4) was carried out exclusively by the group at Wageningen University, I acknowledge that our wording was unfortunate.

Our assertion that the MRDR is not normally distributed was indeed not based on the original data by Wieringa et al (4), but rather on mathematical considerations, and was supported by visual inspection of our Figure 3 (1) and their Figure 3 (4).

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How justifiable is it to distort the energy profile of a diet to obtain benefits in body weight control?

Dear Sir:

We read with great interest the excellent article by Luscombe-Marsh et al (1). Without doubt, this study provides valuable information on the complex subject of weight control and the fight against obesity (2). However, the article raises some questions.

The authors study the progress of overweight and obese volunteers who received 1 of 2 isocaloric diets that differed in their protein and fat contents: 27 subjects received a low-fat, high-protein diet (29 ± 1% fat, 34 ± 0.8% protein), and 30 received a high-fat, standard-protein diet (45 ± 0.6% fat, 18 ± 0.3% protein), both over a 12-wk period of energy restriction and a 4-wk period of energy balance. The magnitude of weight loss and the improvements in insulin resistance and cardiovascular disease risk did not differ significantly between the 2 diets, and neither diet had any detrimental effects on bone turnover or renal function (1).

Our first concern is that the diets followed appear to differ too widely from the theoretical ideal (2, 3). There is general agreement that <30% of energy should come from fat, that 10–15% should come from protein, and that the remainder should be supplied by carbohydrates (2, 3). It would only be justified to diverge from this standard if weight control benefits could not be achieved by any more reasonable method. If overweight and obese persons can only lose weight by following such a drastic diet—and bearing in mind that a high percentage of the population (4) is currently trying to lose weight—then current nutritional aims for the populations of developed countries need to be modified. Perhaps the percentage of energy from carbohydrates should be reduced and fat and protein intakes increased?

A second concern is that if the usual reference values are taken into account (2, 3), the low-fat diet (29 ± 1% fat) to which the authors refer is actually one with an adequate amount of fat, whereas the standard-protein diet is somewhat high in proteins (18 ± 0.3% protein). Nevertheless, both diets provide just 37% of energy from carbohydrates. It is common in developed countries to see energy imbalances in the diet, with excessive intakes of protein and fat and only a small energy contribution from carbohydrates. It has even been shown that this imbalance is greater in overweight and obese individuals (5, 6). Given this background, the imposition of a further imbalance in the energy distribution of the diet would not appear to be the best form of weight control (7).

Some studies report that low-fat diets appear to be effective in the maintenance of weight control (7). Even in the absence of energy restriction, this type of diet can lead to weight losses of 5–10 kg in obese persons (8). Nonetheless, high-protein diets remain popular, which suggests that people perceive some type of benefit in them. One such benefit may be the increased feeling of satiety provided by dietary protein, which makes it easier to adhere to a reduced-energy diet and contributes to spontaneous reductions in energy intake (9). However, the role of protein in the regulation of long-term food intake and body weight is less clear (9). A major criticism of low-carbohydrate, high-protein, high-fat diets is that they may have adverse effects on health (9).

It is likely that many kinds of dietary modification can lead to weight loss, perhaps the most aberrant achieving the greatest losses. But it is important is to find out which can be successfully and healthily followed over the long term. The article by Luscombe-Marsh et al provides valuable information, but we question why a
diet similar to a balanced diet in energy terms is ruled out in the control of body weight.

The authors had no conflicts of interest to report.

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Reply to RM Ortega and AM López-Sobaler

Dear Sir:

Thank you for the opportunity to reply. Ortega and López-Sobaler have not appreciated the point of our study, which was to answer a specific question about the metabolic effects of exchanging protein for fat in the context of energy restriction, in particular, “is an increased amount of protein useful for weight loss or is the effect seen with increased protein related to carbohydrate restriction?” Thus, the high-fat, high-protein diet was an experimental diet to answer this question and not necessarily one we would recommend for long-term use. In addition, the “theoretical ideal” as described by these authors refers to a dietary pattern for energy balance and is not necessarily relevant to weight loss. Indeed, 15% of energy from protein in the context of energy restriction will result in less than adequate protein intake. The recommended ranges for protein intake are based on both adequacy of protein intake and the usual intakes seen in developed countries. Our recent publication showed that an energy-restricted, high-protein, low-fat diet provides nutritional and metabolic benefits that are equal to and sometimes greater than those observed with a high-carbohydrate diet (1). We accept that low-fat diets can lead to long-term weight loss, but high-protein, low-fat diets may well be as effective for some people with the advantage of promoting a better lipid profile. The recent editorial in the June issue of the Journal also makes the point that there may not be a diet that suits all and that “the best diet for maintaining weight loss may be different from the best diet for achieving weight loss” (2). However, both low-fat, high-carbohydrate diets and high-protein diets are a deviation from those commonly consumed in Western countries, and long-term compliance is an issue for both.

The authors had no conflicts of interest to report.

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Potential harm of vitamin E supplementation

Dear Sir:

In their recent Journal review of the safety of vitamins E and C, Hathcock et al (1) stated, “At present, the evidence is not convincing that vitamin E supplementation up to the UL [ie, the tolerable upper intake level, or 1000 mg/d] increases the risk of death due to CVD [cardiovascular disease] or other causes.” However, to focus only on the effect on mortality is a very narrow view of safety. A recent double-blind, placebo-controlled trial in 652 Dutch persons aged ≥ 60 y found greater severity of respiratory infections among participants supplemented with 200 mg vitamin E/d than among those not given vitamin E (3). During respiratory episodes, the presence of fever (P = 0.009) and the restriction of activity (P = 0.02) were more common, the number of symptoms was higher (P = 0.03), and the total illness duration was longer (P = 0.02) among the vitamin E–supplemented participants than among those who received no vitamin E. These findings directly point out that some population groups may be harmed by vitamin E supplementation. Thus, when assessing the safety of vitamin E, it is not reasonable to focus only on mortality when there is evidence of aggravation of a disease that is very common.