Letters to the Editor

Reply to MB Katan et al

Dear Sir:

The goal of our report was to provide a systematic review of the association of dietary saturated fat with cardiovascular disease (CVD) by performing a meta-analysis of the data provided by prospective epidemiologic studies conducted to date (1). The conclusions of the article are, by definition, based on our analysis specifically, and we disagree with the suggestion by Katan et al that they are misleading. Although many individual prospective cohort studies have specifically examined the hypothesis that saturated fat is associated with CVD, a summary evaluation of these studies was lacking until now.

We are confused as to why Katan et al would assert that we neglected to consider saturated fat in the context of the nutrient that replaces it. Indeed, we stated the relevance of the replacement nutrient in the discussion of our findings (1) and the accompanying Opinion piece (2), pointed to the diminished statistical power of our study to evaluate this appropriately (1) due primarily to the limited number of studies that have explicitly considered the substitution effect, and refer to the very study that Katan et al cite as supportive of this concept (3). The second study cited by Katan et al was published only after our report appeared (4), but it, too, provides evidence that concurs with the postulation by us and others that the replacement of saturated fat with polyunsaturated fat, and not carbohydrate, is associated with CVD benefit. The main results of our meta-analysis, which suggest a lack of association between saturated fat and CVD in comparison to carbohydrates (which, in dietary practice, is the nutrient that usually replaces total and saturated fat), are consistent with those from these 2 studies (3, 4).

With regard to the methods of nutritional assessment used by some of the component studies in our meta-analysis, we are keenly aware of the relative weaknesses associated with 1-d dietary assessments and food-frequency questionnaires compared with multiday diet records (considered the gold standard). As was presented in our article, the method of nutritional assessment and its validation were used to derive a quality score, which also considered the number of covariates included in the model. Evaluation of the component studies on the basis of these quality scores did not change our findings.

The population trends cited by Katan et al suggest a parallel reduction in saturated fat intake and serum cholesterol concentrations as a result of the emphasis on reduction of saturated fat as a dietary goal (5, 6). However, we argue that the emphasis on reductions in dietary saturated fat may have inadvertently led to an increased consumption of carbohydrates (particularly refined carbohydrates and added sugars) and total calories. In the context of the current epidemics of obesity and insulin resistance, this trend may be especially deleterious for CVD risk by worsening components of the metabolic syndrome (2, 7). We believe that reductions in the consumption of refined carbohydrates and added sugar, in addition to weight control and obesity prevention, should be the prioritized public health goals.

Finally, although Krauss has advised the dairy industry in the past, these activities were discontinued over the several years preceding the publication of our meta-analysis. The website link cited by Katan had not been updated for a number of years, and this has now been corrected. Any insinuations that our work was influenced by NDC sponsorship are specious. The NDC had no part in designing, preparing, or evaluating either of our manuscripts for publication.

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