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Low-Carbohydrate Diets as Compared with Low-Fat Diets

TO THE EDITOR: The studies by Samaha et al.[1] and Foster et al.[2] (May 22 issue) are intended to expand our knowledge about low-carbohydrate diets. One potential long-term concern about the low-carbohydrate diet is its relatively high protein load and the effect this has on kidneys. This issue is especially important in persons with diabetes, who are more likely than others to have an underlying nephropathy. As a matter of fact, the population studied by Samaha et al.[1] consisted of obese patients and among whom the prevalence of diabetes was high. For this reason, we are very interested in the data on the serum creatinine level at the conclusion of the study. Unfortunately, the study had a high dropout rate; nevertheless, data on renal function from the subjects who completed the study might shed light on this important issue. In addition, the death of one subject with diabetes in the low-carbohydrate group (whose death was thought to be due to poor compliance with drug therapy) should be kept in mind when the results of this study are interpreted.

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TO THE EDITOR: I do not agree with the conclusions of Samaha et al. and Foster et al. that a low-carbohydrate diet was more effective than a low-fat diet. This may be true if we simply rely on statistics, but both studies failed to achieve even a 10 percent reduction in weight with either diet. So the only conclusion that can be inferred from these reports is that neither diet was effective. Likewise, in both studies there was a very high dropout rate, making interpretation of the results difficult, as Ware points out in his accompanying editorial.[1]

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TO THE EDITOR: Samaha et al. do not report the proportions of different fatty acids in the dietary intake of subjects in their low-carbohydrate group. It is known that intake of monounsaturated fat increases the concentration of high-density lipoprotein (HDL) cholesterol[1,2] and reduces the glucose concentration[1] and the degree of insulin resistance, at least in patients with diabetes mellitus.[3] Replacing dietary carbohydrate with polyunsaturated or monounsaturated fat decreases the risk of coronary heart disease, as predicted by favorable changes in the concentrations of triglyceride and HDL cholesterol.[4] Therefore, information on the kind of dietary fat that subjects in the low-carbohydrate group consumed to replace carbohydrate intake would be important for explaining the lipid profile and insulin-sensitivity results.

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TO THE EDITOR: The difference in weight loss between the two diets described by Samaha et al. reflects the greater energy deficit with the low-car-

bohydrate diet (approximately 34,000 kcal over a six-month period) than with the low-fat diet. Data on body composition are not provided, but such data are critical for documenting the components of weight loss (body fat, body water, glycogen stores, and muscle tissue). The low-carbohydrate diet varied only slightly from the base-line diet in macronutrient distribution (37 percent carbohydrates, 41 percent fats, and 22 percent proteins vs. 49 percent carbohydrates, 33 percent fats, and 17 percent proteins, respectively) and contained less fat than the base-line diet (74 g vs. 76 g) and an equal amount of protein (89 g). The absence of a true low-carbohydrate diet explains the absence of change in the uric acid concentration[1]; data on ketosis are not presented. The comparison of these diets is futile because the low-carbohydrate diet (which included approximately 150 g of carbohydrate per day) was not representative of the usual low-carbohydrate diet.[2] The low-fat diet was not low in fat (33 percent of total calories) and included the same percentage of fat as the base-line diet.

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DR. SAMAHA AND COLLEAGUES REPLY: Dr. Duggirala and colleagues raise an important question about protein intake and renal function in the subjects in our study. Protein restriction has been shown to have a small, long-term protective effect on renal function in persons with overt renal dysfunction at base line.[1] This effect may be greatest with nondairy animal protein.[2] In our study, we found no significant changes in the serum creatinine concentration from base line to six months in either diet group (from 1.00 to 0.99 mg per deciliter in the low-fat group and from 1.03 to 1.03 mg per deciliter in the low-carbohydrate group). However, assessment of more sensitive measures of renal function, such as the glomerular filtration rate, would be important for future studies of carbohydrate-restricted diets. Interestingly, a recent study found that patients with diabetes have regression of microalbuminuria in association with improvements in glycosylated hemoglobin and triglyceride concentrations[3] — two factors that improved in the subjects on a carbohydrate-restricted diet in our study.

In response to Dr. Aziz's comments, we would like to clarify that we concluded that subjects on a low-carbohydrate diet lost more weight and had greater improvements in the triglyceride concentration and insulin sensitivity than those on a low-fat diet. These metabolic benefits were clinically meaningful and occurred independently of weight loss. We agree that a 10 percent reduction in weight by six months is an appropriate target. Nevertheless, achieving and maintaining this target have proved to be elusive, even in carefully controlled clinical trials.[4,5] Those studies also demonstrated clinically important improvements in metabolic risk factors with lifestyle modification, despite weight loss of only 5 to 6 percent of base-line weight.[4,5]

Dr. Garrido inquires about the types of fats consumed by the subjects in our study. Subjects in both diet groups reported consuming roughly equal proportions of saturated fat at base line and at six months (29 percent of total fat at base line and 28 percent of total fat at six months).

Drs. Roberts and Barnard raise an important issue regarding changes in body composition, which we did not measure in our study. We suspect that our subjects did not lose only body water or muscle mass, given the clinically significant improvements in metabolic variables such as insulin sensitivity and triglyceride concentrations. With regard to dietary adherence, we acknowledge that neither group reported achieving their study-specified diet goals. Nevertheless, the subjects in the low-carbohydrate group did substantially reduce their carbohydrate intake, whereas those in the low-fat group followed fairly closely the National Heart, Lung, and Blood Institute Obesity Education Initiative guidelines for macronutrient composition, with a reduced intake of total calories and a slightly reduced intake of total fat (mean decrease, 4 g per day).

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DR. FOSTER AND COLLEAGUES REPLY: We agree with Dr. Aziz that it is important to distinguish between statistically significant and clinically significant differences in clinical trials. However, previous studies have shown that a weight loss of as little as 5 percent in obese persons — a weight loss similar to that observed in our low-carbohydrate diet group — is associated with considerable clinical benefits.[1]

The fact that the low-carbohydrate and high-carbohydrate (conventional-diet) groups had lost similar amounts of weight at one year allowed us to assess more precisely the relative clinical importance of the two approaches. Our data revealed no significant differences between the groups in total or low-density lipoprotein cholesterol concentrations but significantly greater increases in HDL cholesterol and significantly greater decreases in triglycerides in the low-carbohydrate group than in the high-carbohydrate group.

We agree that there are limitations in interpreting data from studies that have a high attrition rate. The attrition rate (approximately 40 percent at one year) was higher than we typically observe in clinic-based treatments[2] but remarkably low, given the self-help approach used in our study. As Dr. Ware notes in his editorial, the similar pattern of results between analyses in which the base-line value is carried forward and "completers" analyses provides some reassurance that our results were not unduly biased by attrition. Nonetheless, larger studies of longer duration and with less attrition are needed to evaluate fully the efficacy and safety of low-carbohydrate diets as a potential therapy for obesity.

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Heart Failure

TO THE EDITOR: In their article on heart failure (May 15 issue),[1] Jessup and Brozena do not discuss heart transplantation, which has become a mainstay of therapy for patients with end-stage heart disease. Considerable effort on behalf of clinicians and researchers has been directed toward increasing the availability of donor hearts and reducing perioperative morbidity and mortality. For example, the introduction of interleukin-2–receptor antibodies has been associated with a decrease in the frequency of acute rejection.[2] Similarly, it has been shown that ABO-incompatible heart transplantation can be performed safely in infants, thereby markedly reducing mortality among children with end-stage heart disease who are awaiting transplantation.[3] Recent improvements in our understanding of the adaptive changes in the donor heart after transplantation[4,5] may further contribute to a reduction in morbidity.

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