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Salt Intake Is Related to Soft Drink Consumption in Children and Adolescents: A Link to Obesity?

To the Editor:

The hypothesis that salt intake is a driver of soft drink consumption¹ is an interesting one, but unfortunately this study takes us no further than speculation. Having experience in epidemiological analysis of the National Diet and Nutrition Survey, I beg to differ with the calculations in this article and also the authors' interpretations.

Specifically, I suggest the following: (1) the energy from soft drinks in the National Diet and Nutrition Survey is half what these authors estimate; (2) their study does not "demonstrate that salt is a major determinant of fluid and sugar-sweetened soft drink consumption during childhood" (only $\approx 2\%$ of the variance in sugar-sweetened soft drink consumption is explained by salt intake; and (3) their study provides no evidence for the extrapolation that "a modest reduction in salt intake would also reduce sugar-sweetened soft drink consumption."

First, the energy content of the nondiet soft drinks consumed by children in the National Diet and Nutrition Survey is given (variables N06110, N06111, and N06112) and does not need to be estimated. It equates to a mean of 317 kJ/d (76 kcal/d) or approximately half of what He et al¹ calculate (146 kcal/d). In the National Diet and Nutrition Survey, the volume of water used as diluent for concentrated squash was erroneously double counted. Because the calculation by He et al¹ is based on volume, combined with an assumption that soft drinks all have 26.5 g of sugar per serving, the predicted impact on energy of lowering salt intake is overestimated by $\approx 92\%$.

We repeated their analysis using regression models adjusted for age, sex, and weight, looking at the predicted impact of change in salt intake on other foods. He et al estimated that

reducing salt by 3 g would reduce sugar-sweetened soft drink consumption by 81 g/d. By the same logic, a 3-g reduction in salt is associated with a 44-g decrease in fruit and vegetable consumption ($r=0.16$; $P<0.0001$). The approach by He et al¹ is flawed, because it is reductionist and fails to consider the totality of diet. People who have the highest salt intakes tend to be those who eat the most food, and this is not adequately dealt with by adjusting for age, sex, and body weight. Energy intake would be predicted to decrease by 2 MJ with a 3-g reduction in salt, making the contribution from soft drinks negligible. In practice, of course, this is unlikely to happen.

Lastly, as the authors acknowledge, these data are cross-sectional and cannot prove causality. Yet this caveat seems to be forgotten in the enthusiasm to interpret the regression equations as demonstrating a public health benefit. There is, to my knowledge, no evidence to suggest that reducing salt intake, per se, necessarily leads to a reduction in soft drink consumption among free-living people. To attempt to link salt intake with obesity from these data is surely clutching at straws. Or is that an unfortunate metaphor?

Disclosures

S.G. is an independent consultant in nutrition who has previously received funding for related studies on dietary habits, salt and sugar intake, from UK government and nongovernment sources, including the Food Standards Agency and the food industry.

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1. He FJ, Marrero N, MacGregor GA. Salt intake is related to soft drink consumption in children and adolescents: a link to obesity? *Hypertension*. 2008;51:629–634.