strong correlation with 10-wk absorption, we used baseline calcium absorption as a covariate in all of our models for 10-wk calcium absorption. The reason to do this is exactly as the authors point out: we wanted to account for as much of the heterogeneity in outcome (10-wk calcium absorption) as possible, which means the inclusion of all informative predictors into the regression, because this will reduce the variance of the estimate for the effect of dose or vitamin D status.

A different question is whether there is a statistical interaction between vitamin D dose or 25(OH)D concentrations and baseline calcium absorption, meaning, is the effect of dose different for individuals with low baseline absorption values compared with those with high baseline values? We examined this as much as the data would allow by dividing the patients into baseline calcium absorption strata by quartiles, and we did not see a significant interaction in either analysis.

We stand by our original conclusions that our data fail to support a nonlinear or threshold effect with respect to calcium absorption in the range studied (40–130 nmol/L) with supplementation of 800–4000 IU vitamin D3/d.

Finally, it is important to understand the context of our study. Calcium absorption declines when there is insufficient 25(OH)D as substrate to generate sufficient calcitriol (<30 nmol/L). If subjects with severe deficiency had been included in our study, the response curve would undoubtedly be curvilinear with a decline in calcium absorption with concentrations <30 nmol/L (3). However, the hypothesis that there is a threshold at a higher serum 25(OH)D concentration (80 nmol/L) has now been shown from our study and others to be false (4–9). This finding is important for public policy in vitamin D recommendations. The statement by the authors about the harms of calcium would undoubtedly be curvilinear with a decline in calcium absorption with concentrations <30 nmol/L (3). However, the hypothesis that there is a threshold at a higher serum 25(OH)D concentration (80 nmol/L) has now been shown from our study and others to be false (4–9). This finding is important for public policy in vitamin D recommendations. The statement by the authors about the harms of calcium is odd because it should be the calcium absorbed that matters, whether it is accomplished through calcium intake or vitamin D intake.

Finally, we appreciate the authors pointing out the inconsistency between our reported overall mean for baseline calcium absorption and the group means by dose presented in Table 1. A corrected version of Table 1 appears in an erratum published in this issue of the Journal.

Neither of the authors reported a conflict of interest.

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motivated salt appetite found in rodents do actually apply in humans because, as Hendriksen et al (1) report, sodium intake of humans is very high. However, humans do not appear to increase drinking in response to the increased osmolality that should follow an increase in sodium intake. This is a confounding problem because blood volume can be restored easily by drinking sufficient quantities of water.

Furthermore, until blood volume is restored, the regulatory hormones mentioned above will continue to be released. Interestingly, the overwhelming majority of medications used to combat cardiovascular disease are blockers of the renin-angiotensin system (3). More recently, antagonists of the aldosterone receptor have shown their efficacy when given in association with the other medications (9). The presence of increased plasma concentrations of angiotensin and aldosterone would suggest that humans are chronically dehydrated, or hypohydrated, and yet they continue to consume large quantities of sodium as if they were trying to repair the volume loss. Why humans do not drink despite the combined physiologic signals of increased osmolality and plasma concentrations of angiotensin could be considered the central problem of cardiovascular disease as well as of increased sodium intake.

Perhaps the more appropriate recommendations would be to increase fluid intake, mainly water, with an industry-associated modest reduction in the sodium content of processed foods. The principal objective would be to restore blood volume and decrease concentrations of the regulatory hormones, especially those associated with motivated salt appetite and cardiovascular disease. Furthermore, early rodent work suggests that increased drinking is associated with increased effectiveness of the principal cardiovascular treatments (10). Once concentrations of the hormones are decreased, excretion of sodium in the urine would regulate physiologically most of the dietary intake in excess of need.

Neither of the authors declared a conflict of interest.

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Reply to SN Thornton and P Lacolley

Dear Sir:

We appreciate the interest of Thornton and Lacolley in our study on the potential health impact of salt reduction in processed foods. They state that an increase in fluid intake is an important measure to deal with high salt intakes. We agree that fluid intake is essential for many physiologic processes in the human body (1) and that consumers should drink enough fluid, preferably water. This view is also expressed in the Dutch dietary guidelines for healthy nutrition, which indicate that fluid consumption should be ~1500–2000 mL/d (2). However, in our opinion, the specific theory that an increase in fluid consumption is crucial for reducing the health burden related to an excessive salt intake has not been proven. This hypothesis must first be tested in long-term randomized controlled trials in humans, and the potential underlying mechanisms need to be supported by physiologic studies. For the moment, a population-based approach to reduce sodium amounts in processed foods can be considered an effective intervention to lower blood pressure and to decrease the burden of cardiovascular disease in the general population (3).

None of the authors declared a conflict of interest.

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