

Comment on “Perspective: The Dietary Inflammatory Index (DII)—Lessons Learned, Improvements Made, and Future Directions”

Dear Editor:

We read with interest the article by Hébert et al., which was published in the March issue of *Advances in Nutrition* (1). Considering the impressive amount of work by these authors in compiling the Dietary Inflammatory Index (DII) over the past decade, we were looking forward to their views on the different dietary inflammatory indexes that have been used in the literature and their perspective on future directions. To our surprise, Hébert et al. question the use of the Adapted DII (ADII) in our latest article (2). This ADII, which was computed by our research group (3), would be an adaptation of an old, now defunct version of the original DII and, in their opinion, should not be used anymore.

We made improvements to the original version of the DII, which was developed in 2009 by Hébert et al. (4). As a matter of fact, we made these improvements at the same time as Hébert and colleagues made changes to the original version and launched the second version of the DII (5). While they acknowledged our ADII in their letter to the editor of the *American Journal of Clinical Nutrition* (6), Hébert et al. now seem to question it (1). This is remarkable, as some of our improvements of the original DII, such as standardization of the intakes and reversing the scoring system, are similar to their improvements of the original DII. In addition, our improvement to adjust all dietary components for energy instead of including energy as a component of the index, has actually been the approach that Hébert et al. adopted when they developed the energy-adjusted DII (E-DII) a couple of years later (7).

Hébert et al. are correct when they state that we excluded some food items when we calculated the ADII (1). As described in our previous publications (2, 3), this was partly done to avoid an overestimation of the inflammatory effects of alcohol, fat, and energy (3). Since these food items as a whole as well as parts of these food items were included in the original DII and as such were taken into account more than once, we either excluded the overall food item (total energy and total fat) or the separate food items (beer, wine, and liquor) from the ADII. Although Hébert et al. also took the overestimation of the inflammatory effects of alcohol and energy into account when they made their improvements to the original DII, they still included total fat

as well as all separate fatty acids in the second version of the DII (5).

We also excluded some food items, such as rosemary, saffron, and turmeric, in calculating the ADII, because they were not measured with our very extensive FFQ and cannot reliably be estimated with any FFQ. Despite these exclusions, the variances of the ADII scores are explained by multiple components of the diet, and the ADII has been validated against a summary score of low-grade inflammation, which included 6 markers of inflammation. Thus, it still gives a good reflection of the inflammatory potential of the entire diet. Since Hébert et al. have used their DII in over 200 studies, it would be informative to know how they handled the use of their DII in studies that did not assess the intake of all 45 food items.

When we continued the use of our ADII in our article on the inflammatory potential of the diet and colorectal tumor risk in individuals with Lynch syndrome (2), we acknowledged in the discussion that the DII is used in the majority of studies in which the inflammatory potential of the diet is investigated. We even calculated the second version of the DII of Hébert et al. and assessed its association with colorectal tumor risk in our study population to be able to compare these results to our ADII results and to be completely transparent, which is one of the greater goods in science. Because the DII results we calculated were similar to our ADII results, the variance of the ADII scores was explained by multiple components of the diet in our study, and the ADII has been validated against a summary score of low-grade inflammation, we strongly believe that the ADII is suitable to estimate the inflammatory potential of the diet.

Finally, we applaud Hébert et al. for the important work that they performed on the dietary inflammatory potential of the diet. We encourage them to keep this up and to stay transparent about the additional indices [e.g., the E-DII and the children's DII (C-DII)] that can only be computed with access to the comparative databases they developed.

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