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Inaccurate data in meta-analysis: “dietary patterns and colorectal cancer risk: a meta-analysis”

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Dear Editor

I read with interest the article by Yu-Liang Feng et al (Feng *et al*, 2017) titled “*dietary patterns and colorectal cancer risk: a meta-analysis*.” The significant results presented in the abstract motivated me to read further and I noticed several inaccuracies that require clarification from the authors. These relate mainly to the inclusion of studies with outcomes other than incident colorectal cancer and studies that examined single dietary factors and not dietary patterns; all in contrast to the inclusion criteria listed for this meta-analysis:

Figure 2 provides a summary estimate of the association between intake of the “healthy” dietary pattern and colorectal cancer risk based on 34 studies (some studies provided >1 effect estimate). Five of these studies (Botma *et al*, 2013; Cottet *et al*, 2005; Makambi *et al*, 2011; Mizoue *et al*, 2005; Nimptsch *et al*, 2014) had adenoma (not incident colorectal cancer) as the outcome, whereas one paper (Zhu *et al*, 2013) had colorectal cancer recurrence and overall survival as outcomes. Adenoma and colorectal cancer recurrence and survival were not part of the eligibility criteria for the Feng *et al* meta-analysis. Given the rigorous data abstraction procedure described in the manuscript, it is important that the author clarify this inaccuracy?

The two hazard ratios abstracted from the (Cottet *et al*, 2005) adenoma paper are crude estimates instead of the adjusted hazard ratios. In the (Makambi *et al*, 2011) adenoma paper, the correct rate ratio comparing the highest vs. lowest quintile for the prudent pattern is 0.66 (0.50–0.88: Table 3) and not 0.91 (0.70–1.18) as the authors presented in figure 2 of their meta-analysis. Similar inaccuracies in data abstraction were observed for the (Mizoue *et al*, 2005) adenoma paper. Therefore, the authors did not only include outcomes other than incident colorectal cancer for an analysis aimed at assessing risk of incident colorectal cancer, they also did not abstract the data correctly. I did not verify the accuracy of the data abstracted for the studies that had colorectal cancer as outcome, but given these observations, it is important that the authors conduct the verification.

Figure 3 presents findings for the Western dietary pattern and colorectal cancer risk. The six studies I highlighted in figure 1 were all included in figure 2 with an additional study (Meyerhardt *et al*, 2007) that examined risk of colorectal cancer recurrence and mortality among patients diagnosed with stage III colon cancer. This is clearly not incident colorectal cancer.

Figure 4 presents the summary estimate for the association between the alcohol-drinking dietary pattern and colorectal cancer risk. I noted that out of the 17 unique studies presented in figure 4, only five studies (Kesse, 2006; Kim, 2005; Slattery *et al*, 1998; Stefani E.D, 2011; Terry, 2001) derived dietary patterns described as the “alcohol- or drinker- pattern.” All other studies examined alcohol (and other lifestyle factors like smoking) as a single dietary factor and not as a dietary pattern. This is in contrast with the major objective of this meta-analysis. In addition, among the 11 studies that examined alcohol as a single dietary factor, not all had incident colorectal cancer as the outcome of interest.

Heterogeneity

Given the significant between-study heterogeneity that the authors observed in figures 2, 3 and 4, and that random effect models do not necessarily remove heterogeneity; is it useful to calculate an overall summary estimate of the associations?

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