LETTER TO THE EDITOR

Systematic and meta-analytic analysis of cross-sectional and longitudinal explorations of bone health in youth with obesity: from methodological considerations to clinical relevance

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Our team has recently performed a systematic and meta-analytic analyses of the literature related to the effects of multidisciplinary weight loss interventions on bone health in children and adolescents with obesity [1]. In a recent issue of the *Journal of Bone and Mineral Metabolism*, Alwardat and Alwardat commented on our work, nicely pointing the scientific and clinical interest of our review while stressing several methodological limitations [2].

We understand the points raised by Alwardat and Alwardat through their all commentary, highlighting some important methodological aspects of meta-analysis and systematic reviews' processes that must be respected by authors to ensure the validity of such analyses and the strength of their conclusions. Although we want to thank the authors for their interest, we believe that their comments do not contribute to the scientific area, being moreover not adapted in most cases.

First, the author seems to suggest that we did employ a non-systematic approach, mainly by including studies of various designs and using different methodological approaches. It seems to us that the aim of systematic reviews is precisely to cautiously detail, describe and then discuss the methodological discrepancies between studies addressing a same scientific question. Moreover, contrarily to what the authors seem to suggest when writing that we mixed heterogeneous results for meta-analysis, and that we included indifferently results from cross-sectional and longitudinal studies which according to them biases our results; it is clearly stated in our paper that two different objectives were addressed, one regarding cross-sectional analysis and the second one questioning the effect of longitudinal approaches. This distinction is made even clearer thanks to the presentation of these results using different tables for both our systematic approaches and meta-analysis. On top of that, it has been clearly stated in our methods section that "our meta-analysis was focusing on studies analyzing bone health with DXA in order to compare studies' results", once more avoiding any inappropriate comparison of data obtained with different methods.

Concerning the evidence of the qualities of the included studies, it is clear that no real consensus exists to date regarding one specific evaluation gird to be used, shall it be the PEDRO or GRADE ones, the Scottish International Guideline Network (SIGN) checklist or Methodological Index for Non Randomized Studies (MINORS). Moreover, Cochrane recently released a risk of bias tools for non-randomized controlled interventional studies called ROBINS-I, which is an updated version of the ACROBAT-NRSI [3].

The authors also rightly report that we did use the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) in our publication, reproaching however, the non-respect of the PICO format. Once more, while the PICO format is indeed one of the potential methods proposed to authors to structure their work, using PRISMA does not necessarily imply the adoption of this PICO format. Although we acknowledge the utility of the PICO format in the redaction of such analyses, the singularity of our work led us to do otherwise to ensure clarity.

Alwardat and Alwardat also mention in their comment that our paper did not consider the structure and content or the interventional studies mentioned, mainly pointing to the

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lack of details regarding physical activity. Here again, it is stated in the aim, methods, results and conclusions of our review that the longitudinal analyses include only papers that integrated PA in their interventions. Moreover, both the tables and results section detail the structure of the included interventions, physical activity included.

Overall, we collectively believe that, while the different points raised by Alwardat and Alwardat are methodologically correct, they do not seem adapted in the context of our review. These observations were certainly more adapted in the numerous similar comments they previously published, sometimes using the exact same semantic, in different areas of research [4–7]. Moreover, if we may, we also believe that comments should be dedicated to improve the scientific meaning and conclusions of published papers, pointing some methodological limitations when pertinent, and not to propose potential methodological views that, to us, come under the competency of reviewers and editors.

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