Reply to G.A. Sforzo.

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Sforzo et al. 2021 (1) criticized our systematic review and meta-analysis (2) due to an alleged lack of a standardized definition of health coaching, leading to a "problematic" selection of studies. In their latest compendium on health coaching (3), the authors stated that "Health and wellness coaching is an emerging discipline championing healthy behavior change as means of averting or mitigating chronic lifestyle related diseases". In selecting studies for their compendium, they used the following criteria: "Training: Health coach was trained and used behaviour change theory and coaching processes; Professionals: Health coach was a trained health care professional; Goals: Patient partially or wholly determined behaviour change or health goals; Accountability: Patient progress was monitored; Relationship: Patient-clinician relationship provided opportunity to develop (one coach per patient and at least 3 sessions)". These criteria demonstrate how open and vague definitions of "health-coaching" really are, given that they can equally be applied to most lifestyle interventions, with no clear distinctions between coaching and other behavioral programs. Adding confusion to these already subjective criteria, the authors stated that "inclusion was at the discretion of the reviewer in that not all criteria had to be met for an article to be retained". This means the authors could select studies involving any sort of lifestyle intervention. For instance, studies by Janssen et al. (4) and Lin et al. (5), which were included in the compendium, actually investigated the use of motivational interviewing-based lifestyle interventions, conducted by psychologists (4) or nurses (5). Motivational interviewing, which has been studied and implemented long before the emergence of health coaching, is a behavioral technique based upon robust principles of experimental social psychology and applying processes, such as attribution, cognitive dissonance and self-efficacy (6). There was not a single mention of "health coaching" in the original manuscripts (4,5). Therefore, it is difficult to justify their inclusion in a

health-coaching compendium. To avoid similar selection bias, for our review, we opted to select those studies that self-defined their interventions as health coaching. Amidst such an uncertainty, we deemed the researchers themselves to be best-placed to define their own intervention.

Notwithstanding, as the authors claimed that our outcomes were influenced by our selection criteria, we analyzed the quality of those studies included in their compendium but not in our review (n = 16), using the same quality assessment described in our study (2). We found that 56% were of very low, 6% of low, 19% of moderate, and 19% of high quality. This aligns well with our original data, in which 58% of the studies were of very low, 13% of low, 8% of moderate, and 21% of high quality, thus supporting our main conclusion that health coaching literature lacks quality, irrespective of selection criteria.

The authors also criticized our meta-analytic approach, particularly the interpretations based on effect sizes. The choice of how to pool and present data in a succinct, informative and robust manner is challenging indeed. Given the relatively small number of studies and common reporting of weight, BMI or waist circumference, it was decided that it would be best to pool all three measurement outcomes as standardized effect size. It provides an intuitive understanding of how future individual's performing similar interventions would be expected to change relative to the sampled population. In their letter, the authors quote the importance of considering reported effect sizes within the context of what is expected for specific interventions. And this is exactly what we did. A change of 0.1 SDs demonstrates very little change relative to the population, and taking into consideration the use of only high-quality research, this shrinks to 0.04 SDs. Putting that into perspective, meta-analytic data on motivational interviewing for weight loss show standardized effects to the order of ~0.5–0.7 SDs (7,8), at least ~5 times

higher than those seen in our study. Thus, in contrast to the authors' interpretation (1), we deemed the effect to be trivial not in relation to generalized, heuristic definitions, but in relation to expected effect sizes for apparently similar interventions. Moreover, to directly address the authors query, the unstandardized meta-analytic effect of coaching on weight loss was -1.4 kg [95%CrI: -3.0 to -0.1]. Considering only the highest quality studies, the effect was -1.1 kg [95%CrI: -3.1 to 1.1]. Although there is room for discussion about what constitutes meaningful weight loss in different contexts, we believe most would agree that these average effects are, indeed, trivial.

The root of the discrepant outcomes found in our study and in the compendium is methodological in nature. While we assessed the quality of studies using a clear systematic approach, and in accordance with GRADE recommendations, their compendium (3) did not employ a systematic approach to assess either study quality or effect magnitude. The authors' conclusion that health coaching is beneficial for treating obesity is based on the observation that "A large portion of the studies showed a positive effect on weight reduction" (3). This is not a valid assertion, particularly considering the publication bias identified in our study. This highlights the need for exploring the potential impact of conflict of interests on health coaching literature, in which objective research by those with no vested interest should be important.

To conclude, our findings represent a first attempt to systematically assess the health coaching literature. The main conclusions are that *most* of the studies present serious methodological flaws and divergent theoretical backgrounds, hampering the clinical use of this technique in an effective, uniform way. This is not to say that health coaching cannot be an effective and well-structured intervention. A few good examples on how this strategy can be a useful co-adjuvant therapy in obesity management do exist (9–10). Expanding the number of high-quality studies, with detailed information of their

interventions and reporting on all aspects of their study design is essential to pave the way for a more evidence-based use of health coaching in clinical practice. Simply incorporating any lifestyle behavioral intervention under the obscure umbrella of health coaching will not help build a scientifically-oriented body of knowledge.

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