Answer to the Letter to the Editor of J. Steele et al. concerning manuscript “Is a positive clinical outcome after exercise therapy for chronic non-specific low back pain contingent upon a corresponding improvement in targeted aspects(s) of performance? A systematic review”. Eur Spine J 21(4):575–598, by F. Steiger, B. Wirth, E.D. de Bruin, A.F. Mannion (2012)

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We thank the authors for their interest in our work and their letter to the editor. However, we must correct them in their assertion that we did not consider any of the research using isolated lumbar extension resistance exercise. Whilst the included articles may not have used the specific makes of device mentioned in their letter (“MedX Lumbar Extension” and “Backup Dynamometer”), a number of studies [1–4] *had* used the David Back Clinic DBC110 back extension device, which also purports to isolate the extension movements to the lumbar region using appropriate restraint mechanisms for the pelvis (for details see Fig. 2 and “Discussion” in [3]). These studies were among those that *did* examine the association between the changes in performance and the changes in pain/disability, finding no significant correlations. Hence, whilst it may well turn out to be true that without the use of appropriate restraint mechanisms for the pelvis to isolate the lumbar extensors neither lumbar extension strength nor pain/disability shows improvement as a result of training, it also seems to be the case that even after a programme of training *with* the use of appropriate restraint mechanisms, lumbar extension strength/endurance and clinical outcomes show improvements but the two do not necessarily correlate.

The specific articles involving MedX Lumbar Extension or Backup Dynamometer devices, cited by the authors of the letter, were not included in our review because they either did not appear in the results of our systematic search conducted in accordance with our predefined criteria (e.g. they had no accompanying clinical data [5]), or they were published after our search date [6, 7]. The only exception was the article by Nelson et al. [8]; this was identified in the database search of CINAHL, Cochrane, Embase, Medline and Pedro, but was not included in our review because neither the title nor the abstract described the relevant outcomes assessed (see “study selection” section of our review).

Whilst the article by Smith et al. [6]—had it been published earlier—would most likely have been included in our systematic review after the first round of inclusion/exclusion, examination of its findings reveals that it nonetheless failed to evaluate the correlation coefficients (i.e. using individual data points) between the changes in strength and changes in clinical outcome; group mean change values favoured the pelvic restraint group in terms of both strength and clinical outcomes, but this does not confirm that the two were directly related within any given group, the phenomenon of relevance in our review. Smith et al. [6] gave no information about the specific randomisation procedure used, the equivalence of patient characteristics/LBP history in the groups at baseline, concealment of treatment allocation, blinding of assessors or ‘blinding’ of patients to control for expectation bias (especially important in view of the fact that the patients in the control and no-stabilisation groups were aware of

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the study objectives and offered access to the “real treatment” afterwards, presumably upon the expectation that their own treatment would fail!). This means it cannot be ruled out that other factors that may have differed between the three groups may have acted as confounders, serving to influence both the mean change in strength (or lack thereof) and mean change in patient-rated outcomes (or lack thereof). In other words, the group mean values for both performance and outcome could have been influenced by a common third factor (e.g. expectation bias, or LBP-history) peculiar to and “favouring” both functional and clinical outcomes in a specific treatment group. In the absence of any information about the correlation coefficients describing the relationship between individual changes in performance and changes in outcome, the Smith et al. study [6] does not, therefore, alter our main findings.

We too hope that other researchers will address in their future studies the important question of correlation or concordance between individual values of performance change and of clinical change. The intention to do so by the authors of the letter is most encouraging and is to be applauded.

Conflict of interest None.

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