Answer to the Letter to the Editor of Gui-Tao Li et al. concerning: “A systematic review and meta-analysis of biological treatments targeting tumour necrosis factor α for sciatica” by Williams NH, Lewis R, Din NU, Matar HE, Fitzsimmons D, Phillips CJ, Sutton A, Burton K, Hendry M, Nafees S, Wilkinson C (2013) Eur Spine J; 22:1921–1935

Nefyn H. Williams · Ruth Lewis

Received: 10 December 2013 / Revised: 2 January 2014 / Accepted: 4 January 2014 / Published online: 17 January 2014
© Springer-Verlag Berlin Heidelberg 2014

Thank you for your supportive comments of our systematic review. In answer to the specific points in the letter, we decided that a random effects model was more suitable than a fixed effects model for our meta-analyses. As can be seen from Table 1, there were both clinical and methodological heterogeneity between the included studies, although they were all evaluating similar interventions (biological treatments targeting tumour necrosis factor α) for the same condition (sciatica) [1]. We, therefore, considered that a pooled estimate would still make sense, but that the use of a conservative random effects method was warranted, which assumes that the different studies are estimating different, yet related intervention effects. Where there is no presence of heterogeneity, the random effects model gives identical results to that of the fixed effects model. In other words, a random effects analysis addresses the question “what is the average intervention effect?” We do not believe that the assumption of the fixed effects model, that the true intervention effect is the same in every study (and that the only source of variation is sampling error), would hold in our review. We used methods of meta-analysis recommended by the Cochrane statistical methods group [2]. For random effects models they recommend the inverse-variance method for continuous outcomes, and the Mantel–Haenszel or inverse-variance methods for categorical outcomes. The authors of the letter do not state the reasons why they believe that an alternative method is superior. As for assessing publication bias we

did produce funnel plots which are available on the online resource accompanying the paper. Visual inspection of these plots did not reveal any obvious asymmetry. Tests for funnel plot asymmetry were not carried out as there were fewer than ten studies in the meta-analysis because test power is usually too low to distinguish chance from real asymmetry [3].

Conflict of interest None.

References

1. Williams NH, Lewis R, Din NU, Matar HE, Fitzsimmons D, Phillips CJ, Sutton A, Burton K, Hendry M, Nafees S, Wilkinson C (2013) A systematic review and meta-analysis of biological treatments targeting tumour necrosis factor α for sciatica. *Eur Spine J* 22:1921–1935
2. Deeks JJ, Higgins JPT, Altman DG (2008) Analysing data and undertaking meta-analyses, Chap 9. In: Higgins JPT, Green S (eds.) *Cochrane Handbook for Systematic Reviews of Interventions*. Wiley, Chichester
3. Sterne JAC, Sutton AJ, Ioannidis JPA, Terrin N, Jones DR, Lau J, Carpenter J, Rucker G, Harbord RM, Schmid CH, Tetzlaff J, Deeks JJ, Peters J, Macaskill P, Schwarzer G, Duval S, Altman DG, Moher D, Higgins JPT (2011) Recommendations for examining and interpreting funnel plot asymmetry in meta-analyses of randomised controlled trials. *BMJ* 342:d4002

N. H. Williams (✉) · R. Lewis
North Wales Centre for Primary Care Research, North Wales
Clinical School, Bangor University, Gwenvro 4–8, Wrexham
Technology Park, Wrexham LL13 7YP, UK
e-mail: nefyn.williams@bangor.ac.uk