

Meta-analysis: Adding apples and oranges?

Sir,

I read with interest the results of the meta-analysis by Krishna *et al.*,^[1] on the role of noninvasive positive pressure ventilation in postextubation respiratory failure and I commend the authors on their detailed and comprehensive methodology and well-written paper. However, I would like to share a couple of suggestions which are crucial to the interpretation of the results of this meta-analysis.

The authors found moderate heterogeneity between

included studies (I^2 statistic of 48.5%). Summary statistics for a meta-analysis can be calculated using two types of statistical models: fixed-effects, when there is minimal heterogeneity and random-effects, when there is a higher level of heterogeneity. Of these, the random-effects model is more conservative and in the setting of heterogeneity, is likely to give more dependable results.^[2] For better understanding and to allow readers to judge the validity of the results, the authors should have specified the type of analysis which was used in this review.

The quality of any systematic review or meta-analysis is only as good as that of the included studies. The authors have done a quality assessment of the studies incorporated in this review and it appears that several of the studies were not of good quality (score of 16 or less). In particular, one study by Jiang not only had a low quality (score of 13) but also had results which were diametrically opposite to the other studies in the analysis. This study was given a high weightage of 20% in calculating the overall summary statistic. It is surprising that the authors did not conduct a sensitivity analysis by excluding this particular study, and also another analysis by separating good from poor-quality studies. This would probably have made the results of the meta-analysis more precise and reliable.

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