

Morgenstern M, Vallejo A, McNally MA, et al. The effect of local antibiotic prophylaxis when treating open limb fractures: a systematic review and meta-analysis. *Bone Joint Res* 2018;7:447-456.

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Sir,

I enjoyed the authors' recent talk at The European Bone & Joint Infection Society (EBJIS) meeting, and I read the full article¹ with great interest. However, I believe the authors were taken by a certain publication practice which was entirely common almost three decades ago but is now discouraged for obvious scientific reasons. Having a look at the institutional details, recruitment intervals, specification of interventions, patient profiles, and authors' lines of the studies by Henry et al,² Ostermann et al,³ and Ostermann et al,⁴ it is quite clear that this was the same sample stacked by subsequent patients over time. Thus, the three reports must not be regarded independently, which influences the overall weighting of studies in the authors' meta-analysis. Excluding the earlier two papers will lower the number of subjects from retrospective comparisons to 1701. By a re-analysis, the pooled odds ratio (OR) in favour of additional local antibiotics can be calculated at 0.41 (95% CI 0.28 to 0.59). The difference between estimates from the single randomized controlled trial (RCT) and pooled cohort studies is noteworthy, and should be respected by a stratified analysis with individual results (this can be done easily with RevMan or STATA software).

The authors use the term 'case-control-study' which is incorrect, as cases in such a design are determined by the presence of a condition or event, not by the intervention or exposure of interest.

While all this does not fundamentally change their conclusions, they may consider my suggestions and remarks for a clarification.

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