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# Authors' response

We appreciate the interest shown by Birajdar *et al*[1] in our meta-analysis[2]. They acknowledge that such meta-analyses are of considerable importance in guiding policy. In fact, it was driven by a question which arose during antimicrobial stewardship activities, *i.e.,* significance of procalcitonin based decision in different settings within a hospital.

For the first point raised by Birajdar *et al*[1] wherein they say that only one study was available for ward setting and hence was not amenable to meta-analyses, we could agree no less. However, readers would know that RevMan, the software used for the meta-analyses represents the data for subgroup analyses, whether or not data have been pooled for the subgroup. It can easily be deciphered from the Forest plot (Fig. 2)[2], the confidence interval was the same as that shown in the individual study. The data for this single study, however, need to be entered and depicted to enable overall pooling which is represented at the end of the Figure. In fact, deleting this information would not only have flawed meta-analysis but also rendered the Forest plot incomplete. However, one does conclude that more ward based studies need to be done.

The reason for difference in the number of studies for different outcomes was because not all studies reported this outcome. This again, is more of a norm than exception. As regards to the method of quality assessment, the authors need to refer to the section on Quality Assessment[2] wherein the method has been referenced and explained briefly.

Heterogeneity was assessed and wherever it was significant, appropriate model was used. The details of the same could have been added in the methods section. However, we thought that the Forest Plots would be self explanatory.

Sensitivity analyses based on the putative causes of heterogeneity were not planned *a priori* and was not presented in the paper. However, the suspected reasons for clinical heterogeneity have been commented upon in the discussion section[2]. As far as meta-regression

is concerned we would have needed to specifically address a factor or a set of factors for seeing impact on outcome. For our current analysis we did not undertake meta-regression knowing the shortcomings of post-hoc selection of variables[3]. It would be interesting to see someone undertake this exercise.

Birajdar *et al*[1] referred to two meta-analyses with stricter inclusion criteria[4,5]. One of these[4] was available at the time of submission of our meta-analysis and was referenced. The other one[5] was published later. In the latter meta-analysis[5], within critically ill patients, the focus of infection has been specified or not specified. There are more specific examples, which readers may have referred to. Studies with tighter inclusion criteria would affect heterogeneity favourably. Our meta-analysis was directed towards a very pragmatic decision making exercise during management in a hospital settings. Infections of various kinds are addressed in emergency, wards and intensive care units.

Regarding the Table of all included studies, we would agree as it is an important aspect of the study. However, in the past, we have had the experience of having been asked to either delete it or present it as an appendix as the journals are hard pressed for space. We have given the reference of the included studies.

Regarding the conclusive remark regarding challenge of 'lumping and splitting studies for meta-analysis', Ioannidis *et al*[6] who used these term explained at length the ''difference in opinion of reviewers" to be an important determinant of whether to pool or not pool the data. In fact they made a case for pooling the data using appropriate methodology in case heterogeneity was present[6]. We refrained ourselves from undertaking a meta-analysis when we are convinced any exercise in pooling would be logically and logistically flawed[7].

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